Anonymous Referee #2 Received and published: 16 June 2020

Review of: Seasonal and Interannual Variability of Melt-Season Albedo at Haig Glacier, Canadian Rocky Mountains Submitted to The Crysphere by Marshall and Miller.

Major Revisions required.

Albedo measurements from in-situ weather stations are used to identify melt season albedo dynamics for Haig Glacier. The results are used to comment on the conventional application of degree-day melt rates and on how albedo describes glacier mass balance. C1 TCD Interactive comment Printer-friendly version Discussion paper These types of in-situ data driven papers are very important to the understanding of glacier dynamics and glacier mass balance, especially for mountain glaciers.

AU: We thank you for the time spent reviewing the manuscript and providing constructive suggestions for improvement. These are all helpful suggestions and we believe that we have been able to respond to these, leading to a better-organized and more clear contribution. Please see below for our point-by-point response, in blue. Page and line numbers refer to the track-changes copy of the manuscript.

The manuscript is well written with a logical presentation of material. I would suggest a minor re-organisation of the Introduction section to separate the literature review from specific mention of the study on Haig Glacier, as the sporadic reference to the study on Haig Glacier comes across as a bit disjointed. The final paragraph of the Introduction should be devoted to specific details regarding Haig Glacier. Specifically, how the study on Haig Glacier addresses the limitations related to glacier albedo and modelling.

We agree, we were jumping around far too much in an attempt to state the objectives of the paper in the opening paragraph. We have now reorganized as suggested, with the specific details of the study site and the aim(s) of the study in the final two paragraphs.

Abstract: The improvements related to the stochastic model on mass balance and the modification of the degree day model should be provided in more detail.

This is difficult with the limited space, but we have revised and added more detail here. This may need to be trimmed in the next round of revisions, as we are at 389 words for the abstract.

Line 11: Summer should be defined in the abstract (e.g., June 1 to August 31). Summer is defined on Line 104.

Thanks – JJA is now defined in the abstract.

Body of text:

Line 28: It is true that albedo is involved in the control of surface energy balance, but it is the net radiation (short wave and long wave) that mostly controls melt. Net radiation was previously mentioned, but a better description of how net radiation is related to albedo and what the proportion of shortwave to longwave radiation is, would be very useful.

This is true – it is net radiation that really matters, but with albedo as the main influence on melt-season variations in net radiation on mid-latitude mountain glaciers. It is a bit hard to compare the importance of net SW and net LW balances, as the latter is an energy sink. Hence we cannot say that X% of the melt energy is due to absorbed shortwave radiation and Y% from the net longwave. As a measure of this, we now report the correlation of each to the net energy that is available for melt, based on previously published data at our study site (Marshall, 2014). This interferes with the attempt to move all mention of Haig Glacier to the end of the introduction (per below), but it is relevant here and addresses this request to articulate the importance of albedo. Other references to Haig Glacier have been moved to the end of the introduction, as suggested.

We calculate the mean daily surface energy fluxes for the set of all summer (JJA) days reported in Marshall (2014), N = 1012. The average net energy, Q_N, is 101 W/m^2, with 79 W/m^2 from the net radiation, Q*, and 22 W/m^2 from the turbulent fluxes (26 W/m^2 from the sensible heat flux, Q_H, and –4 W/m^2 for the latent heat flux, Q_E). Net radiation accounts for 79% of the net energy that is available for melt. Within this, net radiation is dominated by net shortwave radiation in the summer months: 107 W/m^2, vs. –28 W/m^2 for the net longwave radiation. We also calculate Pearson's linear correlation coefficients, r, for net energy against each of the radiative fluxes and the albedo (N = 1012):

r (Q_N, SW in) = 0.39
r (Q_N, net SW) = **0.84**
r (Q_N, albedo) = **–0.81**
r (Q_N, LW in) = –0.09
r (Q_N, net LW) = –0.20
r (Q_N, net radiation) = **0.94** r (net radiation, albedo) = **–0.83**

These values are summarized in the introduction, although we tried not to get too bogged down in what feels like results (albeit from previously published data), p.2, ll.6-12. We also rewrote this to clarify that net radiation dominates net energy, but net shortwave radiation dominates net radiation in the summer melt season (with albedo being the main control of daily mean net shortwave radiation).

Line 29-30: Reference to Haig Glacier should probably come at the end of the introduction.

This sentence has been moved to the end as suggested.

Line 48-51: This sentence seems to be a bit misplaced and should be moved to the end of the introduction as a bridge between the literature review and the methods section.

True, our apologies. This was definitely out of place. Now moved to later in the introduction.

Line 64: Please define what a melt-albedo feedback is.

We added a sentence to explain this positive feedback, p.3, ll.8-9.

Line 69-71: Snow algae can be of many species (up to 4 or 5). Is there a reference for this material, or is it an observation from Haig Glacier? If it is an observation it would find a better home in the Results section.

This is just an observation from Haig Glacier, a common spring occurrence. In fact we don't know the species for certain, though I have been told it was *Chlamydomonas nivalis.* This comment was meant to make the reading more interesting but is not needed, so we have removed it in the event that we have the wrong species of 'pink algae'.

Line 76: A recent article in Remote Sensing of Environment might be of interest here: Williamson et al., 2020 - Comparing simple albedo scaling methods for estimating Arctic glacier mass balance.

Thank you – now cited, p.3, l.21. Happy to have this paper brought to our attention.

Line 78-79: This material might be better suited in the final paragraph of the Introduction.

Thanks, also moved in the rewrite.

Line 82: Can you provide more detail on what the "simplified parameterizations" entails?

We now refer to these explicitly as temperature index melt models, described in more detail in the abstract and in the lines above and below the sentence that was flagged, p.3, ll.33-41.

Line 111: Campbell does not make many instruments. The details for the instruments should be included (manufacturer and instrument), at the very least for the radiometric instruments, as different instruments are sensitive to different range of the EM spectrum.

This is a good request, for a paper focused on albedo. This information has been added, p.4, ll.36-42.

Line 119-120: Data collection ongoing has previously been mentioned.

Thanks, now deleted.

Line 124: If only one station is collecting data how was the lapse rate estimated? Please provide details.

Details now provided, p.5, ll.16-26. This is based on the 'climatological' mean lapse rates at this site (or really just offsets, with two points), calculated from the multi-year record for all days when both stations were working. This gives daily and monthly mean values for the offset, or one can calculate lapse rates from this for glacier-wide application.

Line 126: How much error is related to the estimation?

This is a good question. Where forefield data are available, which covers about 70% of the data gaps, error is small because we understand the relation well between the forefield and glacier AWS records. The stations are 2.5 km apart, although there are systematic (and seasonally-varying) offsets associated with the different environments: snow/ice vs. rock. Where both stations are missing data, we fill in with the average value for that day from the 'climatological' (historical) data for that day, the mean of available data from 2002-2015. The error can be quantified by applying the gap-filling procedure to estimate data for times with valid data. For interest: comparing observed temperature at the AWS site (as an example) to adjusted AWS data from the forefield gives an average error of –0.13°C (a small cold bias), while using the 'climatological' mean value gives an average error of –0.11°C. A similar analysis for specific humidity gives values of 0.15 g/kg and 0.16 g/kg, compared with a mean value of 3.3 g/kg: hence an error of 5%. These values are for the 30-minute data.

We don't present this because we don't use gap-filled data for the albedo values that are reported here (cf. p.5, ll.9-10) – only the days with quality-controlled in situ data are used for the albedo statistics and plots. That is the primary focus of this study. We do use the gap-filled data to drive the surface energy balance model, e.g., to evaluate the sensitivity of modelled melt to albedo. This is secondary to the main results and discussion, however. A formal error analysis could be done to propagate the error in temperature, wind speed, etc. through the surface energy balance equations, but this would be a tangent to the main points of the manuscript. Interestingly, I seldom see this in surface energy balance studies, i.e. assessment of uncertainty in the meteorological forcing and how this propagates through to errors in the surface energy fluxes.

Line 132: Define "questionable data".

We expand on this now, p.5, ll.4-6 – physically impossible values, off-scale readings (-6999), and 'flat-lining' that we sometimes see if a sensor gets buried by snow in the winter.

Line 154: There is a recursive reflection from the bottom of optically thin clouds or from scattered clouds and a high albedo snow covered surface.

Yes, interesting, but this should be implicitly accounted for in the radiation measurements. The incoming radiation sensor would measure this reflection from the clouds and it would be twice-reflected from the glacier surface. This can lead to overestimates of both the incoming and outgoing shortwave radiation, but this should scale without major effects on the albedo. Small effects are possible by changes in the composition of diffuse vs. direct radiation, but we do not separate these in this study. As a side note, we did examine subsets of overcast vs. clear-sky days, and found no statistically significant differences in average snow or ice albedo on the glacier on these days.

Line 165: Please define Jaycar QM1582. What is the spectral range of this instrument?

This is just the brand name of the specific handheld pyranometer we used. Thanks – we now report the spectral range, which does differ from the Kipp and Zonen instruments. Caution is therefore needed in comparing these values with the AWS albedo records, but within the particular spatial surveys conducted in 2017, we can compare these values in space and in time (i.e. for the four repeat surveys). We add a note of caution on comparing with the AWS-measured broadband albedo, p.6, l.31 to p.7, l.2.

Line 221: "this" should be these.

Revised as suggested, p.8, l.27.

Line 245: The introduction mentions two AWS. It is not clear which station these results refer to. I assume from the data period this is the on ice station (upper ablation zone).

Sorry yes, all of the albedo results are from the glacier AWS – the off-glacier AWS is not helpful here, but is just used in this study for gap-filling of missing meteorological data for the energy balance modelling. We clarify here, p.9, l.17.

Line 284: "jump" might not be the best descriptor here.

We revised this to "increase", p.10, l.29.

In Table 2 why is Em larger for August than July? Cloud cover – because Em is using only shortwave radiation?

No, Em also includes longwave radiation. All of the terms in the energy balance, per equation (1). Cloud cover is not the cause - it is in fact directly due to the lower surface albedo in August. Much more shortwave radiation is absorbed in August than in June or July.

Line 289: What type of regressions are these? Linear, least-square regressions, Pearson's? Are the correlations statistically significant? If so, which ones?

These are simple linear Pearson's correlation coefficients. Now stated. We also now indicate in the Table which values are statistically insignificant ($p > 0.05$).

Line 291: What does "correlated" mean in this instance?

Here we mean to say there is a statistically significant negative or positive correlation. This should now be clear from the explicit indication of this in Table 3. The discussion on pp.10-11, Section 3.2, has been revised accordingly.

Line 297: Define "fewer samples".

We specified the numbers but have rewritten through here: we have 14 years of data, 2002-2015, so N=14 for the mean summer conditions and their relation to the annual mass balance conditions. Within each year we analyze data from May through September, giving us 70 months. This sentence has been removed in place of a more clear discussion of sample size, p.10, ll.40-43.

Line 300: Define "melt out" or replace with better descriptor.

Revised to the more specific/technical term "ablate", p.11, l.13.

Line 309: Define "ripened and saturated"?

Revised to "wet, temperate" (at 0°C, with liquid water content), p.11, l.28.

Line 315: Some other citations that might be useful here, especially in the context of spatial variability of albedo. 1. B.W. Brock, I.C. Willis, M.J. Sharp. Measurements and parameterization of albedo variations at Haut Glacier d'Arolla Switz. J. Glaciol., 46 (2000), pp. 675-688 2. S.N. Williamson, L. Copland, D.S. Hik. The accuracy of satellite-derived albedo for northern alpine and glaciated land covers Polar Sci., 10 (2016), pp. 262-269

We were already citing the Brock et al. (2000) paper here, p.11, l.37. We prefer to stay with comparisons to direct/in situ, broadband albedo measurements here, but the Williamson et al. (2016) paper is very relevant to later sections where we discuss spatial variations and satellite measurements of albedo, so we have added this there, p.17, l.12.

Line 323: Describe the film, thickness composition, etc. Is there liquid water in the surface matrix? If so, what effect does this have on albedo? O.k., I see this is addressed on Line 335.

It's about a 1 mm film, with examples in Figure 4, although it is not a continuous film everywhere – in many places impurities are discrete particles, with varying density. Now noted, p.12, l.4. Like most mid-latitude mountain glaciers, the glacier surface is wetted during the summer melt season, but well-drained. Certainly these two effects – the impurities and wetness – contribute to the low values of ice albedo, as discussed, and the generally lower albedo of mountain glaciers compared to polar ice.

Line 325: Not clear where the values for Figure 5 are coming from, and provide how N=224 was derived.

This is for all bare-ice days in the 14-year record (i.e. when there was snow cover at the AWS site). N=224 is the number of days, derived by counting all days with albedo values less than 0.4 after the initial rapid drop in albedo (the snow to ice transition) that is clearly evident each summer (e.g., Figure 3b). We have edited to clarify this, p.11, l.31. The caption of Figure 5 is also revised.

Line 343: Adding year to the dates will reduce confusion.

We note the year now in introducing this discussion, p.12, l.27.

Figure 6: Mean values should have standard error included on the figure.

We added this for plot 6a. The mean multi-year values have very low standard errors: for a mean daily error of 7% and for 14 years, averaging reduces this to about 2%. Standard error (the uncertainty envelope, really), is higher for individual years, as plotted for the data from 2003 in Figure 6a. For Figure 6b, the mean daily value for each year, we don't plot this because errors are vanishingly small – these values are calculated from a mean over either 92 days (JJA) or 152 days (MJJAS) for each year. On averaging, the uncertainty in an error $d\alpha$ (7% for a mean daily value) is calculated from $d\alpha/\mathrm{sqrt}(N)$, so for JJA this is 0.7%, or 0.004 for an albedo value of 0.6. This is not easily visible on the plot. Note that we have interpreted this request as the reviewer's suggestion to plot the standard error where possible and relevant – we assume that the reviewer is referring to standard error, not standard deviation.

Line 376: "dropping" should be decreasing.

Revised as suggested, p.13, l.29.

Line 385: The values of ~0.1 and 0.07 are close enough that instrument error might render these inseparable?

We conservatively estimate the instrument error to be 10%, double that of the manufacturer-specified accuracy. By taking the average of three measurements, this is further reduced, to 8%. But even at 10%, this means 0.10 ± 0.01 and 0.07 ± 0.007: 0.10 and 0.07 are statistically distinct. We also measured the 7% albedo at 3 different sites (the lowest three points) on the centreline transect.

Line 395: Can evidence be presented that Haig glacier was indeed downwind of the forest fire smoke? For example, can specific fire events be linked to specific albedo declines for 2017? Without this link the material presented here is speculation.

In fact we also consider this to be speculation here, and tried to phrase it that way. That said, we were up on the glacier in thick smoke for many days (smelling of smoke, hazy skies with limited visibility, direct observations of it blowing in from the southwest). Winds on the glacier systematically blow in from the southwest (B.C.), funneled by the valley geometry. We also have wind direction data to support this. However, a thorough analysis of specific forest fire events, black/organic carbon provenance, and plume modelling is beyond the scope and focus of this study. We comment on this explicitly now, and make it more explicit that "we speculate…", p.14, l.9, and subsequent lines. We also note our direct observations of forest fire impacts during this period, as well as the indirect evidence through the increase in particulate concentrations, now included in Table 5.

Line 399: Which year?

2017, per this entire section – now noted

Line 400: Please present pertinent details for the data. The reader can't evaluate the data from an unpublished source.

This is a valid request – apologies not to include this earlier. We had cited the MSc Thesis of Miller (2018), which is available online and contains all of the details, but agree that it is helpful if the manuscript stands alone. We now present the data that we refer to in Table 5. Interested readers can find more detailed data tables and analyses in Miller (2018), but we now include the referenced data in this study. Additional supraglacial and meltwater chemistry data in Miller (2018) will support a separate publication on the supraglacial chemistry of Haig Glacier and its evolution through a melt season. As much of this is not essential and is ancillary to the current study, we present only the data that shows the large increases in impurities and carbon concentrations through the period of regional forest-fire activity in summer 2017, coincident with the observed decrease in ice albedo through this period.

Line 401: How is this "consistent"? Provide details, references or rationale.

Consistent in that forest-fire fallout would be expected to be carbon-rich, so-called 'brown carbon' as well as black carbon and soot (e.g., C.J. Williamson et al., 2020). But as this is results and not discussion, we removed this comment and now just present the observations and data, without commentary.

Line 407: I assume that algae assimilate carbon that was on the glacier before, or during, its growth. If this is correct, then the algae are a carbon flux and not a source per se.

This is partially true – they assimilate available nutrients – but they are also autotrophic, prolifically photosynthesizing and engaging in atmospheric carbon fixation. See e.g. C.J. Williamson et al. (2019), cited in the manuscript, as well as Yallop et al. (2012), Cook et al. (2012).

Yallop, M.L. *et al, 2012.* Photophysiology and albedo-changing potential of the ice algal community on the surface of the Greenland ice sheet. ISME J., 6, 2302–2313.

Cook, J. M., Hodson, A. J., Anesio, A. M., Hanna, E., Yallop, M., Stibal, M., et al. (2012). An improved estimate of microbially mediated carbon fluxes from the Greenland ice sheet. *J. Glaciol.* 58, 1098–1108. doi: 10.3189/2012JoG12J001

Line 420: What does "reasonable" mean? Is this fit presented by the authors?

Good point, this is imprecise language. We do present statistical fits below, after the introduction of stochastic snowfall events. But for this occurrence, we have revised this sentence to remove this statement, p.15, l.5.

Line 428: What about heterogeneity of albedo? Albedo increases on a glacier as elevation is gained. What is the amount of variability in albedo for a surface that appears to be homogeneous?

We consider variations with elevation in sections 3.4 and 4.3. The albedo increase with elevation is not generally observed when it is all snow-covered (i.e. for up to 9 months per year in the Rockies), but is true in the summer melt season when lower elevations have exposed ice (e.g., Figure 7b). Also, for the exposed glacier ice itself, albedo increases with elevation have been reported elsewhere and are seen on Haig Glacier as well. This is associated with increasing concentration of impurities at lower elevations, and could be incorporated in Eq. (7) through the value of k if one had an idea of the spatial variability of impurities and their influence on snow albedo. But Eq. (7) does not refer to glacier ice, which is where impurity-driven spatial heterogeneity has been documents. Applied across a glacier, Eq. (7) does capture differing rates of melt (i.e. greater PDD at lower elevations) and these effects on albedo decline (wetting, recrystallization, the timing of the transition from snow to ice).

Line 432: "brighten" should probably be changed to increases the albedo to that of fresh snow (~0.85), before declining to seasonal normal values (over a given time period on the order of days).

Revised to remove 'brighten' and use the language 'increase the surface albedo', p.15, l.19

Line 446: What does "some" mean in this instance?

Some has been removed, p.16, l.2 – you are right, not meaningful here. We also added a sentence to explain this more clearly as well, p.16, ll. 2-3 (i.e. random sampling of a normal distribution to introduce realistic variability in this value, vs. assigning a single value).

Line 453: "reasonable" should be described. What is the difference between the two?

Thanks – this was imprecise language again. We have revised to "is in accord with the observations", p.16, l.10. We also statistically assess this now, per the comments of both reviewers, p.16, ll.20-25.

Line 455: What is the temperature control on snow fall events? Snow does fall when the surface temperature is > 0.

Yes, recognized, as it the air temperature (the column) and not the surface temperature that matters. Rain also falls at near-surface temperatures below 0°C. Snow is increasingly unlikely as temperatures increase, however, so we parameterize this simply based on a linearly-decreasing snow fraction f_s from 1 to 0 between near-surface air temperatures T_a of –2 to 2°C. This uses mean daily temperatures. Now explained, Eq. (8), p.15, l.34.

Line 458: What does "this year" mean?

The year being discussed and plotted here, 2007 – now stated again on p.16, l.19.

Line 465: Why were five realizations chosen?

This was arbitrary. In putting together our statistics we increased this to 10 realizations. Each one differs a bit (e.g. Figure 7b), so it is better to include several realizations in the mean, but the values after just a few realizations converge and are representative of model results for a given set of parameters.

Line 468: I don't remember seeing any run-off data?

True, we don't report that here. Like most mountain glaciers, all summer ablation runs off, based on past studies and discharge measurements at this site, so we commonly equate these. But to be careful here, we have removed the reference to runoff, p.16, l.38.

Line 471: Please describe how this is a positive feedback. A warming atmosphere produces more rainfall events (instead of snow) at the glacier's elevation. Rain further melts the glacier causing more rainfall events?

Thank you, good catch. Indeed this is not a feedback, although it excited albedo-melt feedbacks on the glacier. Wording changed that this would accelerate the melting, but of course without feeding back on the precipitation, p.16, l.42.

Line 525: Are there no observations of this behaviour on Haig Glacier?

To our knowledge, glacier albedo trends have not been detected or reported on Haig Glacier or in the Canadian Rockies. Only anecdotal impressions. One of our objectives in this study was to analyze this from our long-term observations, to provide the first assessment of whether albedo is declining. Happily for the glacier, we don't see any evidence of albedo declines over the study period, so we have to reject our null hypothesis that the glacier is darkening due to an extended period of negative mass balance.

Line 529: From which transect date?

This is from late summer, when the seasonal snow is gone and we are comparing ice with ice. Now clarified, p.18, l.28.

Line 536: This paragraph is mostly results and should be presented in the Results section. It is a bit problematic that the authors are relying heavily on unpublished data to interpret the albedo results. Are the unpublished results necessary?

Thank you, good comments. As discussed above, we had referenced Miller (2018) for the data, which is published on line and peer-reviewed, insofar as graduate theses have been vetted. But we agree that it is better to have the data presented here, so it is now included in Table 5 and discussed in the results. In the context here, we are going beyond results and talking about the potential for melt-induced concentration increases (vs. atmospheric deposition) – it is more discussion and interpretation than results. We have revised this paragraph though, to refer back to Table 5 rather than present new numbers/results here, p.18, l.37 to p.19, l.4.

Line 551: Upon what basis is this statement made? There is no observation station at lower station, yet the melt feedbacks are the strongest here. What exactly is the melt feedbacks and why is this plural?

Thanks, this was unclear as written. By "changes and melt feedbacks have been strongest here", we were referring to the mass balance and glacier thinning over the study period. The toe of the glacier has largely collapsed. But we don't have albedo data to comment on changes in albedo or whether the lower ablation zone is getting darker. We have rewritten this to specify that we mean "where glacier thinning and mass loss have been most extensive", p.19, ll14-15. We removed the discussion of melt feedbacks – plural because there are a few things happening, e.g. lower elevation = warmer and more melt; more exposed bedrock warms up and melts the glacier terminus more, though sensible and longwave fluxes; potential accumulation of dust and debris which warms up and melts the glacier more, etc. These are all known processes but we don't measure them or present data on these in this manuscript, so we have taken this out.

Line 578: Shouldn't start a new paragraph with "this".

Revised as suggested.

Line 582: Does water vapour pressure increase over the study period, or for that matter, any of the other environmental variables measured at the weather station?

We did not analyze this here, so won't introduce it in the conclusions and will keep the focus on the albedo measurements and modelling. There are increasing trends in summer temperature and melting/mass loss, though with a lot of interannual variability and so only weakly significant. There is no statistically significant trend in vapour pressure. This question concerns the glacier mass balance and weather trends, which are not the subject of this study, so we don't add this to the manuscript in the interests of keeping our focus. At this particular line, we are discussing how summer snowfalls (in general) impact the mass balance, not the trends in such events or in mass balance.

Line 590: What are the "ways" that you suggest?

Apologies, this would only be clear for those that read the results and discussion – it should be explicit in the conclusion (monthly factors or as a function of albedo), now stated, p.20, ll.21-23.

Figure 4: Including dates for the photos would be helpful.

To be honest, we don't know the exact dates, but also that is not important to the visual context that we wish to convey.

Figure 7a: No snow pits appear on the figure. The figure leads me to believe there are additional temperature measurements available.

We have revised the figure to better show the snowpits. It's true, there were three additional weather stations on the glacier this summer (Veriteq/Vaisala temperature-humidity stations), but we don't refer to these data so we have removed this from the plot.

Figure 8: The modelled values seem to reach a maximum at ~0.85. What is the reason for this? The observed data clearly achieves higher albedo values.

This is true – we set a maximum fresh-snow albedo of 0.85 in the summer snowfall model (as defined on p.15, l.10), but this is a free parameter that could be between ~0.75 and 0.9, looking at the data from Figure 8 as pointed out. Most of the fresh-snow events in August and the first two weeks of September that year are experienced by a rapid increase in albedo to values close to 0.8, whereas May snow events come in closer to 0.9. Our value, 0.85, is taken as an average fresh-snow value, not the maximum.

Figure 9: There are ~seven points in the above the trend line (f~7; albedo~0.7) that if removed would greatly improve the correlation. Can the author identify the origin of these points (i.e., a specific year, or month)?

This is interesting and we had thought about this, but cannot justify removing these points. Four of these occur in May, of various years. One is from June, two are from September. We don't observe anything special about these specific months, in terms of the temperature or other aspects of the meteorological conditions. These datapoints imply that there are certain times where there are high rates of melting per PDD even with high-albedo snow cover – degree-day factors of 7 are more typical of ice. A plot of just JJA conditions would give a stronger regression, but melt modelling needs to be inclusive of the shoulder season, May and September (e.g. melt totals in Table 2), so we retain these points, but can't explain what was different about these specific months. There is a fair amount of scatter at all albedo values in Figure 9 – the relation is significant but not as strong as we had hypothesized it would be (R^2 = 44, i.e. albedo explains only 44% of the variance in the melt factor, as discussed on p.17, ll.22-25).