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Interactive comment on "Assessing the performance of a distributed radiation-temperature melt model on an Arctic glacier using UAV data" *by* Eleanor A. Bash and Brian J. Moorman

Anonymous Referee #2

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Review of "Assessing the performance of a distributed radiation-temperature melt model on an Arctic glacier using UAV data" by Bash & Moorman

General Comments

This study draws upon high-resolution glacier surface melt estimates to validate and test the robustness of an enhanced temperature index melt model across the terminus of a glacier in Arctic Canada. While the study draws heavily upon previous work by the authors, the novelty of this particular study is clear as one of the first opportunities to investigate the performance of a distributed melt model at high spatial resolution, allowing for insights into the topographic controls on model performance. Overall, it has the potential to be an impactful and citeable study for glacier mass balance and energy balance researchers by providing new insights into the performance of distributed melt models, and the comments here primarily focus on strengthening the arguments of the authors and improving the clarity in places:

1) There are several instances where the authors draw upon the methodologies of other studies to guide their ETI model development. However, what is often lacking is a simple description of the approach used in the cited study, and a statement explaining why this methodology was chosen by the authors. Examples of cited works that would benefit from deeper explanation include: Goswami et al. (2000); Bugler (1977); Rippin et al. (2015); Höhle and Höhle (2009)

2) A brief statement should be made early in the paper indicating whether reported melt values (m) are in ice equivalent or water equivalent (i.e. have you converted your surface elevation change observations to melt equivalent, or vice versa).

3) The determination of albedo is somewhat concerning, or at least the section describing the determination of albedo requires more detail and justification for the reader. It would be my impression that the scaling of surface albedo needs to be done twice, once for each orthoimage, with scaling being conducted using the albedo measured at the AWS at the time of the survey as validation for that cell. An off-ice site (rock or debris) could be used as a location of assumed constant albedo of $\sim$0.1. Instead, it is unclear why the authors would use the AWS-derived average albedo (rather than maximum) as the upper limit for their albedo threshold across the area of interest on both survey days. Hopefully revisiting the albedo calculations might also help explain the surface darkening noticed by another commenter on the discussions page! Plotting the progression of surface albedo at the AWS over the study period could help confirm whether this darkening is real.

4) A more detailed error analysis, and clearer reporting of errors and uncertainty, would significantly strengthen this work. Currently "model error" is used to describe the resid-

ual values between modelled (ETI_dist) and "measured" (see note below) melt at each cell. However, each of the parameters included in the model should have an associated (or at the very least estimated) uncertainty, as will the DSM models and calculated melt from DSM differencing.

- To help with this, and to strengthen your argument for correlation between model errors and specific parameters, it could be worthwhile to include a figure with four plots that show model error vs. 1) aspect (with a full range from 0-360°), 2) slope, 3) albedo, and 4) density of water features, by extracting these cell values over the entire region.

5) On a semantics note, the differencing of DSM models to determine melt is not a melt "measurement" but rather it is a calculation, or estimate (considering that the DSMs have their own model uncertainties). It is fair to treat the DSM melt estimates as a measurement for the sake of ETI model validation, but the authors should briefly acknowledge this and be clear about the source and magnitude uncertainties associated with this "measurement."

6) One notable gap is an acknowledgement of, or discussion about, how parameters like aspect and slope are incorporated, or fail to be incorporated, into ETI_dist through the estimation of distributed radiation using the Goswami et al. (2000) approach. There is a good opportunity here to explore the sensitivity of ETI_dist to slope and aspect!

Specific/minor comments:

P1-L19: ...14% of the world's glacier ice... ← specify if this is area or volume

P2-L13: Available measurements... The wording of this sentence is awkward, rewrite or combine with previous sentence to help with clarity.

P2-L25: ...in high temporal and spatial detail... (replace great)

P3-L13-16: Regarding ablation stakes, I am curious why you did not also use these 17 ablation stake observations as validation for ETI_dist, in the same way you use the AWS as a point observation. Perhaps you did not have stake measurements on the days of the UAV survey... However, I am perplexed by the reporting of an RMSE for the stake observations, perhaps you mean the RMSE of the modelled melt (from DSM differencing) at the stake locations? It is important to be clear that your DSM differencing is not communicated "measured" melt, but rather modeled, or estimated, melt using the geodetic method.

P3-L23: Suggest including the manufacturers name for the SR50.

P3-L31-32: Please provide some justification for why a 5-hour rolling average was chosen, and how samples qualified as being "significantly different" from the population.

P4-L3-4: When referring to resolutions (0.10 m, 0.02 m) specify whether these are horizontal resolution (your cell size) or vertical resolution; also be specific with the reported uncertainty – which is presumably vertical uncertainty? +/-?

P6-L13: "...temperature was assumed to remain constant across the area." Out of curiosity, do you have a rough idea of what the temperature gradients are in this area? A quick correlation analysis between model error and surface elevations from your last DSM would be a simple way to verify that this assumption is reasonable.

P6-L15: What do you mean by "modify"? I would suggest giving much more attention to describing how the slope and aspect correction is applied in ETI_dist, particularly given the correlation of your error to aspect later on (see general comment #7)!

P6-L19-30: This is the section where it would be helpful to have some more detail from the previous studies you draw upon build your model and conduct corrections. This section really describes the heart of your model, and any additional detail you can provide will help readers understand the reasoning for your model design and why it performs the way it does. I might even suggest creating a simple flow-chart that illustrates the model inputs and their sources.

P6-L27: Is there a reason your reported range in albedo goes from high to low? (Rather than "0.1-0.55"?)

P7-L12: As noted in the general comments, a brief explanation of the Höhle and Höhle paper's approach would be helpful – I am personally curious why the median is used instead of the mean!

P7-L19: Why was 1500 cells chosen as the threshold for number of cells contributing to a water flow feature, and can you express this value in the area equivalent (e.g. square meters)?

P8-L8: Uncertainties should be included the modelled and measured values reported here.

P9-L4: Be clear what this correlation value is (Pearson correlation coefficient) or by using $r = 0.34$. Putting the correlation value at the end of this sentence seems awkward, maybe try rewording?

P9-L7: Similar to above, correct to include "... much lower ($r < 0.1$, and $p >$ ___)..."

P10-L1: Suggest observation rather than "experience"

P10-L3: "-0.048 m"; also reword for clarity and include r =.

P10-L15: "...deviation of 0.00083 m h-1, which is similar to other..."

P10-L20: Be specific, which year?

P10-L32: Specify, horizontal or vertical resolution

P11-L4: "offsets"

P11-L12: "Correlation between aspect..." This sentence feels out of place in a description of work by Bash et al. (2018), perhaps try rewording or open this paragraph with this sentence or something similar. E.g. "The correlation (r = X) between aspect and model error ..."

P11-L18: Tighten up wording, e.g. "Bash et al. (2018) measured higher melt rates in active supraglacial streams than on the surrounding ice."

P11-L23: "… between the density of linear…"

P12-L9-16: It is unclear what this paragraph contributes to the paper here, rather it seems to interrupt a discussion of water flow features and their impact on melt. Perhaps add more context, otherwise remove.

P12-L20: "…have a greater relative importance."

P12-L31: What do you mean by "simplifications"?

P13-L5: Can you be specific about how you actually build upon the work of Rippin et al. (2015) to estimate albedo? Is this by introducing a scaling approach?

P13-L10: Unclear what this first sentence is saying regarding "other implementations"

P13-L19: It is not clear how Stevens et al. (2018) and the development of weather crusts relates to this study. Either take more time to explain the relevance (in the discussion section) or remove from conclusions.

Figure comments

Figure 2. Extending the E and F y-axis down to zero would help your arguments in the text. Check consistency with bold-type for your graph subsets and parenthesis e.g. A) vs (B), and correct formatting of lin in the 2nd last line. Can you also explain the gap at the beginning of (F)?

Figure 4. Recommend including the study dates – "… across the study area between July 21 (hh:mm) and July 23 (hh:mm)."

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