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The Cryosphere Discussions
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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 #1

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[Review of manuscript 'TC-2019-81']

[Main Comments]

The authors present a case study of a distributed enhanced-temperature index (ETI) model compared to very high-resolution digital surface models (DSM) for a glacier terminus in Arctic Canada. The authors note a good model performance comparing median changes, though indicate that the model does not capture the full variability of the measurements which are partly associated with surface channels, 'water features' and aspect of asymmetrical banks of surface channels. The study highlights that models are not representing such features, and as such, they must be addressed for future modelling attempts on Arctic glaciers.

The manuscript is well presented, written and draws upon a great dataset with which to highlight problems of the broad application of ETI models. The work is based upon sound scientific knowledge and the expansion of previously published work using this dataset. While the authors justify their usage of their ETI model, I believe that testing model limitations against a very high-resolution UAV survey requires leveraging available (and local) AWS data for running a full energy balance first, to better understand the processes and errors of the model vs. the measurements. With a greater understanding of the problems facing the modelling efforts on this glacier (and potentially other Arctic glaciers), the authors should then approach the ETI model and demonstrate the significance of their results to the wider science which relies on these simpler approaches.

In places the manuscript also suffers from a lack of justification and reasoning for its methodologies, and in other places, I believe that some key pieces of information are missing. I feel that more analysis, and potentially additional figures may help to strengthen the work. I would very much like to see this work published and I think it is of a high quality for publication in The Cryosphere. I would nevertheless like to see and review the implemented changes. Therefore, the changes I have suggested here and detailed below constitute a major revision.

[Specific comments]

(Page)1-(Line)1: While I think that all the necessary information is here, the first 5-6 sentences should be reworked to improve the flow from short, individual sentences. It currently reads like bullet points.

2-8: Rework the sentence to reflect citations relevant to TI, ETI and EB, separately.

2-12: Check sentence continuity.

2-25: Typo

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Discussion paper

2-35: NU? I assume Nunavut. Perhaps write out in full for the few instances for the benefit of the reader.

3-10: Include the size of your study area: 0.185km2

3-12: AWS melt I assume refers to a sonic depth gauge? Specify what you mean here. . . It is measured quantities that you refer to??

3-17: Try to keep consistent with units. . . Here you move from metres when talking about error to cm for melt rates. These values are also surely in a water equivalent? This was a concern of mine when reading the manuscript as I was unsure if you are comparing UAV differencing (Z difference in m./cm.) and modelled melt (m w.e. /cm w.e.).

3-25: You describe your methodology for pole tilt of the SDG, but the dates of your ETI model (at both point and distributed) extend beyond the UAV measured differences with no real justification for why. . . perhaps I've missed something here or it hasn't been explained.

3-30: "30" what??

3-31: I'd like to see some reasoning for the selection of your chosen methodology for dealing with SDG data averages and gaps. Why the specified interval? Does it affect your results?

4-4: Whilst this work is published in Bash et al. (2018) and doesn't all need to be repeated, I would like to see a few of the core details and justifications for methodologies of the SfM model construction. . . For example, why M3C2, and not 2.5d differencing was considered. A short reminder here would be useful.

4-7: Is interpolation justified here? With what method? Why were their gaps in your DSM?

6-7: So Iabs is SWnet? Perhaps change this terminology to be clear and consistent

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with the literature.

6-8: What are your derived TF and SRF values?! This is important to mention somewhere. Are your values believable? How do they compare to the other TI, ETI models (as you compare to in your discussion)?

6-9: I need some justification for modelling over these dates, and not just the DSM differencing period (3 days).

6-11: Have some tests of the 0C threshold been performed? This threshold can be rather variable. I believe that optimising this threshold could be beneficial. This holds then to my major comment regarding the model. I believe the data and locality of the AWS makes it highly desirable to perform an energy balance approach for the point-scale, if not for both point and distributed models, to aid process understanding and support the application of your ETI approach. The authors have a lot of valuable data with which to calibrate/validate their model at the distributed scale (such as stakes and surface information from the UAV) with which to provide the best possible ETI model. Propagating the model error then with the DSM error can provide a strengthened analysis of how, in the best case scenario, the ETI succeeds/fails to capture important information about glacier mass loss.

6-12: To distribute model variables. . . reword here.

6-13: Although I agree that differences will likely be small, I think it valuable to distribute your temperatures in your model domain, as it may still vary enough to influence your 0C assumptions for melt onset. This also draws in to the limits of the residual approach you adopt, as mentioned in the open discussions by Professor Marshall. I think a simple environmental lapse rate would suffice for the small elevation range you describe.

6-15: This is modelled using the surface topography taken from the first DSM right? Specify that here.

6-16: Another interesting and important aspect to test would be the effect of cell res-

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olution. A 0.1 m resolution is incredible and interesting to test what models can and cannot do, but is not realistic for any level of glacier-scale or regional modelling. I think it relevant to have some level of testing, and discussion regarding this. For example, the ETI fails to capture the variability seen in the DSMs, but is this consistent at 1 m, 10 m, 30 m?

6-18: Provide minimum solar angles.

6-26: Check sentence continuity.

6-27: Your figure 2 implies that you have a net radiometer (or rather both up-facing and down-facing pyranometers) to derive your albedo at the point-scale. How does this compare to your albedo map derived from your RGB histogram stretch from 0.55 upward? What are the ranges of albedo derived? Also, the values seem largely different between 3 days when looking at figure 3. It looks as though the whole domain darkened by 0.05-0.1. Perhaps this was some effect of the cloud filtering you performed (according to Bash et al., 2018)?

7-1: Still need some reasoning for your model period selection.

7-10: Measured melt from the DSM? Again is this a Z-difference or ablation? Are you truly comparing the two here? I'm sure your model does not calculate vertical lowering, but melt (in w.e. units). Are you converting your DSM differences with density values of ice?? What values? Measured or assumed?

7-11: Again, please provide some justification for NMAD and ME over other metrics.

7-18: The model was run to quantify surface water production? Or just a 'watershed' analysis? Perhaps add a map of this to Figure 3?

8-9: The lower variability of the modelled values are not surprising, given that the model has two variables to consider for a system that is far more complex.

10-2: Figure 5A4-D4 does not show the correlation, but area of water features. I also fail to see how the errors of the AWS and distributed models are linked here. Perhaps include a correlation figure somewhere.

10-21: What uncertainties? Model uncertainties are not considered against your model-DSM comparisons. Your model error is simply the difference with the observed values.

10-22: Check citation format.

10-27: what do you mean by muted?

10-32: Again, interested to see the effect of model resolution on your results as it would be very relevant for future work.

11-5: And Horizontal motion?

11-12: Again to see another figure with some correlations would be nice.

11-17: Could this be a result of measurement uncertainty? Lighting from the processing of the SfM images? The processing of clouds and histogram stretching applied in your former paper? Although predominantly south facing, roughness may play a role here, which is of course not considered by ETI models.

11-21: Have you considered any uncertainty due to SfM model construction for steep sided relic stream features that you mention? Did you obtain any oblique images? Is there anything worth noting about that here?

12-16: Interesting that Figure 2 doesn't show such a dampened cycle. Please see suggestions for Figure 2. What about Wind speed? (again plot in Figure 2)... A temperature factor will under-represent the contribution from turbulent fluxes if your glacier terminus is heavily affected by strong katabatic winds. Again, anything worth noting here?

13-3: The conclusion reads a bit like a discussion in places.

13-5: ETI 'model'. The albedo sentence feels a little out of place and should be reformulated into the previous sentence.

13-24: Add citations here.

13-26: More pronounced compared to which studies?

[Figures]

Figure 2: Perhaps use colour here to aid the visual differences between measured and modelled point-based melt (or surface lowering??). For panel C. What do you mean by distributed variables? Surely the values are the same, as you are comparing the melt/surface lowering at the AWS? The variables require no distribution and surely are the same as panel B? Why not compare the measured and modelling SWin on the same panel to give confidence to the reader that your modelled radiation, based upon surface topography, is valid? Also, I would suggest converting units to Wm-2, for consistency with much of the literature, and to compare with other studies. As suggested in the specific comments above, I think it would be valuable to demonstrate the full EB data, including wind speed (and direction if there is anything interesting to say about katabatic winds and its directional consistency) as well as relative humidity and LW if available. Equally, showing the sub-period of the UAV DSM acquisitions is key!

Figure 4: Your model error is Modelled minus Measured? So your negative values are under-estimation? Please clarify your convention. Transparent areas don't show where the model performs poorly, but rather where modelled-measured differences are within the error range of the measurements. What about model error estimates here? These should be propagated.

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