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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

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This is interesting work and the authors get a lot out of a three-day data record. Of course a month or more of data would be really helpful, to increase the signal to noise ratio in both the UAV data and the melt model, but given that this is what the authors have, I am impressed with their inferences. The high-resolution images of surface changes definitely have something to offer to help understand the uncertainty in measurements and models of surface mass balance, especially when it comes to spatial variability at a given elevation and errors in spatial interpolation.

That said, I have some questions about the melt modelling that would be worth considering:

1. The authors have an automatic weather station (AWS) recording what they need to carry out a proper surface energy balance, which could well apply to their study region (with modifications of the absorbed shortwave radiation for slope, aspect, and albedo). Why are the AWS data not invoked, at least to check how much much net energy was available at the AWS site, the associated melt, and whether this is consistent with the UAV data. It is hard to justify a simplified ablation model when the authors have the data to do a complete energy balance, with proper physics. Longwave radiation sounds like its missing, but there are methods to parameterize this as a function of temperature and humidity. This addition would greatly strengthen this manuscript and has the potential to change the results significantly. I understand the basis for a simplified melt model to apply to large, distributed areas where meteorological data are lacking, so the authors could still test their ablation model, but it would be stronger if compared against the 'gold standard' of proper melt physics. The authors should consider a full surface energy balance as a kind of 'ground truthing'.

2. Within the simplified radiation-temperature index melt model that the authors use, I also think that they made an important conceptual error that needs to be corrected. The authors assume that temperature, T, is uniform over the study site while residual temperature, Tr, needs to be adjusted. It is actually the opposite. The whole idea of constructing a decorrelated temperature time series is that net shortwave radiation influences air temperature (a positive correlation). If there is no relation, then Tr = T. Because there is a relation, the solar radiation effect is removed and the 'residual' can be thought of as the 'proxy' for the energy associated with other sources of heat, such as incoming longwave radiation and sensible heat flux. So it is inconsistent to assume that T and absorbed shortwave radiation are correlated at the AWS site, and then to assume that they are not correlated elsewhere (i.e. variable net shortwave radiation with constant temperature). Because there is a relation, the authors' south-facing grid cells, where shortwave radiation is greater, should be associated with warmer temper-

atures. By assuming a constant temperature everywhere, the authors ends up with lower Tr where net shortwave radiation is high – hence, an underestimate of the melt (consistent with the results). This should be rerun assuming uniform Tr rather than uniform T. This will alter the results and conclusions.

3. I do expect that the interesting correlation with water concentrations on the glacier surface will hold, but the aspect results should change. That said, does the influence of meltwater need to be through kinetic energy? Certainly it is in supraglacial and englacial channels, liberating potential energy for kinetic and thermal dissipation. But another influence for a water film is simply through its albedo effect, water being close to 0.

4. Which brings me to albedo: visually, there appears to be a really large shift in albedo over 1 or 2 days as inferred from the UAV data. This would be hard to explain, the appearance of a systematic darkening of 0.1 or 0.2 over glacier ice, as I read the figure. This is perhaps just the image or illumination angle – reporting actual values here would help. What is the average albedo (and variance) over the study region in these two images, or for each day of the study?

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