

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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Thank you for your comments, you bring up several good points. Our response is below:

1. While we had considered comparing the UAV measurements with an energy balance model, our main goal in the study was geared towards a more broadly applicable model. However, your point of using the energy balance as another kind of ground truth is valid and we will endeavor to include this in the revised manuscript.

Regarding the comparison of UAV data to AWS data, this was presented in a previous

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manuscript, but we acknowledge that reiterating it here would also be valuable.

2. This is an insightful comment on the physical underpinning of our statistical model, which we had not considered. We will adjust the temperature index model to hold T_r constant and allow T to vary, and report updated results in the revised manuscript.

3. The manuscript as posted in the discussion forum has already been updated to discuss other potential mechanisms for water flow leading to increased melt. Although this may include an influence from albedo, the weak relationship between albedo and model error suggests that is not the main driver for increased melt in these locations. We have acknowledged that the actual mechanism has not been studied and thus we can only speculate on the possible causes for the increased melt where surface water is present.

4. Indeed there is an overall decrease in the gridded albedo between July 21 and July 23, which is likely due to differences in imagery between the two days, in addition to real lowering of albedo which was recorded at the AWS. The average difference between the two days across the study area is 0.07, but we believe this translates to a minor potential error in the melt calculations. We will address the implications of the albedo more thoroughly in the revised manuscript.

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