The Cryosphere Discuss., 9, C1365–C1371, 2015
www.the-cryosphere-discuss.net/9/C1365/2015/

The Cryosphere
Discussions
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Interactive comment on "Debris-covered energy balance model for Imja-Lhotse Shar Glacier in the Everest region of Nepal" *by* D. R. Rounce et al.

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Received and published: 7 August 2015

General comments:

The paper uses a set of field data gathered on the Imja glacier to optimize and examine the sensitivity of a sub-debris ice melt model for debris covered glaciers to three usually poorly constrained inputs. In particular, it is valuable that attempts have been made to assess the impact of various ways of treating the surface latent heat flux and the temporal resolution at which the model is executed, as these are important unknowns in current modeling approaches.

The paper is well-written, and is lacking in only a few methodological details that would aid the clarity. It is a pity that most of the ablation stakes that could have provided

a fully independent validation of the model were lost, but these things happen, and nevertheless the approaches used in this paper could be applied to ablation stake data for debris covered glaciers gathered by other researchers.

The paper introduces a novel way of determining surface roughness, but in its current form it detracts from the paper. The validity of the roughness results are not clear and therefore can add little to the debate on turbulent fluxes over debris covered glaciers, and the sensitivity range tested is in-line with previously published values. While still worth exploring as a method I think this part would be best done as a full study including comparison with aerodynamically derived roughness lengths. (As a side note, I like the idea of what you tried here; I myself have also tried to apply the Lettnau-Munro relationship to SfM surfaces of both clean and debris covered glaciers (unpublished) and concluded that these approaches are not simply to apply reliably on highly heterogeneous rough glacier surfaces).

I suggest either removing this section or simply noting the wide range of geometric roughness of the glacier surface represented by the standard deviation of the SfM surface models.

Removing this section would allow more space to expand the presentation of the sensitivity of the model to LE and timescale, which I consider the most valuable contribution of this paper.

Points to be addressed:

1. SfM DEMs: More specific detail on the error assessment of the derived DEMs is required as detailed below in the specific comments.

2. Newly proposed method for determining microtopographic z0: While I support what was trying to be achieved through this effort, I am not convinced of the value of introducing this new method without a validation of it against an aerodynamic roughness length derived from meteorological instrumentation. The relationships of Lettau and

other similar formulations are usually validated against wind profile determinations of z0 carried out in the field or in a wind tunnel. As stated above, I think the paper would be better and more focused without this section.

3. Model calibration It would be advantageous to additionally perform a multisite optimization to obtain single optimized values for albedo, k and roughness, rather than a value for each stake. These values could then be applied to all ablation stake sites to give an idea of how useful the model will be when applied to sites for which no specific optimization is available.

4. Model results: (a) Including scatter plots as well as the line plots in Figure 4 could be more helpful for visualizing the prevalence and nature of model biases. As you observe a positive model bias for the nightly minimum temperature I would suggest color coding the scatter plot according to hour of the day to show the timing of any biases. This approach might also be a useful for discriminating more detail about the relative performance of the differing methods of modeling LE, by highlighting scatter points for which LE was being modelled in a different colour. (b) I became a bit confused as to exactly what data was being used for model validation, so this needs cleared up, and stated more explicitly, regarding the use of R2 values between modeled and measured temperatures and which sites provided data for comparing modeled and measured total ablation. I'd also like a small table or explicit listing of values comparing the 3 model time resolutions to the available stake data.

Specific comments:

P3507/L23 Replace 'laying' with 'overlying'

P3507/L26 What was the reason for the data loss? Failed loggers? Inaccessible? Sensors becoming exposed? Might be useful information for others.

P3508/L8 Why was site 4 treated differently?

P3508/L8 How did you compute k from the temperatures – I assume you used the

method of Conway and Rasmussen (2000), but you need to state the method and reference.

P3509/L9 What do you mean by unvalidated here?

P3509/L15 What density did you assume for your snowfall rate to get SWE? Was it a constant value?

P3509/L16 Perhaps its useful to add a % of missing data?

P3509/L20 Was this comparison done on a month by month basis or on the average of all 4 months?

P3509/L26 How well did the linearly interpolated diurnal LWI cycle represent that measured at the Pyramid station 2003-2010?

P3510/L21 Can you provide information on the accuracy of your GCPs as it affects the resultant scale of the SfM model as far as I understand it.

P3510/L12 State here that your GCPs were obtained using a total station with an accuracy better than 1mm.

P3512/L7 I am not clear what you mean by the unit width in this part of your method.

P3513/L5 This is a little unclear to me, but I think you mean that LE=0 unless the RH in the overlying air (at 2m height at the Pyramid AWS) is at 100%, at which point you also set the surface RH to be 100%? Can you express this more precisely in the text please.

P5314/L16 In addition to the reference, please add a sentence describing the nature of the simple snowmelt model used.

P3515/L11 What was the reason for not computing k at site 14 for the 0.05m depth?

P3515/L22 You found previously that k varied with depth, or it was dependent on total debris layer thickness?

P3515/L25 Reiterate here the duration of the measurements used in R&M 2014. Also was there any observable trend in k over time – that would also indicate a temperature dependency.

P3516/L7 State explicitly how this error is computed – is it the RMSE computed between the total station location of the GCP and the SfM DEM location of all 4 corner markers? Presumably before you remove the planar slope? Are the values in the table the average or the maximum of these 4 corner marker errors? What is the error on the DEM produced by Agisoft? Did you need to reject and images from the analysis?

P3516/15 Do you really think 4 GCPs per plot is too sparse? Why?

P3516/L25 How was this value for z0 determined. Why do you consider it more accurate that Munro's method?

P3518/L24 Specifically which field data? Just temperature?

P3519/L2 Remove this sentence. It is tautological as the optimization must achieve reasonable values for these parameters as you constrain their possible range according to values from the literature.

P3519/L11 R2 between what variables? It seems you performed this on all the available temperature records, correct? Perhaps you'd be better off just doing it for the surface temperatures as (a) you have few measurements at depth and one might be poorly located and (b) other things might be going on within the debris and affecting individual temperature readings at depth are in some way taken into account by using a single optimized k value for each site.

P3519/L13 What physical field evidence leads you to believe the sensor moved down over time? I'm not sure how it could do so? Could it just have been poorly located at the outset? If the sensor at 20cm depth in site 13 was actually at a grater depth, this would also affect the calculation of k at that site and be a reason for your anomalously low k value for this site.

P3520/L5 Might it be clearer to use an alternate data format given that much of the English speaking world does not use the US month/day convention?

P3520/L4 Consider using ' . . . There are a few days for which a positive bias in temperature can be seen during the daily high and nightly low', as overestimating a 'low' might imply modeled temperatures lower than those measured during the nightly low.

P3520/L8 Typo: Do you mean daily high here?

P3521/L15 I'd suggest removing this last sentence, as its not really necessary.

P3522/L1 Why not compute ablation for all 14 stake sites? It might provide a more useful model test, as in reality researchers will likely be applying the model to sites for which optimized inputs are not available. See my point above about a single multi-site optimization.

P3522/L21 The time periods of the ablation stake measurements and the modeled ablation do not match. Did you run the optimized model for the whole period of the ablation stake measurements to provide a comparison of these data? It becomes clear from later text that you did this, but make it clear here as well.

P3524/L5 Your sensitivities to k are similar to those found in Nicholson and Benn (2006) for debris cover of ca. 30cm, but in that work we found that the sensitivity of modeled melt (as a % of melt) to k is dependent on the thickness of the debris cover (see Table 2). Your results also show higher k sensitivity at sites with lower ablation, coinciding with thicker debris. I think this is worth mentioning in the context of our results where we explicitly explored the thickness dependency of the sensitivities. Nicholson and Benn (2006) found slightly lower sensitivity to albedo than you, and our sensitivity to z0 was variable but similar to your findings. It might be worth comparing these explicitly as the model in Nicholson and Benn (2006) uses only daily averages, but the sensitivities as quoted appear to be similar.

P3525/L8 So here for the temporal resolution you model ablation for all 14 sites, but in

the evaluation of your standard model only for 10 sites?

P3525/L25 So here you are comparing the total ablation from your stake data – only available at sites 8, 13 and 15? Or are you comparing to your higher temporal resolution model? I am unclear as to which temporal resolution is performing best as compared to the stake data at these 3(2) sites. I say 2 sites because site 13 is clearly complicated, and by optimizing the model on a poorly located temperature sensor at depth it makes a poor test of the model.

P3526/L4 In addition to your comment about snow, which is at least partly dependent on the manner in which the snow is treated in the model, a daily averaged model is likely to perform poorly during seasonal transitions even if the surface is not snow-covered as the temperature profiles through surface debris that is either heating up or cooling down through the seasonally transition are often not linear, as shown by the measurements in Nicholson and Benn (2012)

Interactive comment on The Cryosphere Discuss., 9, 3503, 2015.