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The Cryosphere Discussions

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.

Anonymous Referee #2

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General Comments

Rounce and others apply an energy balance model to the debris-covered Imja-Lhotse Shar glacier in the Nepalese Himalayas to investigate the sensitivity of modelled ablation rates to uncertainty in three key parameters, different formulations of the latent heat flux and the temporal resolution of input data. In addition they attempt to derive surface roughness values using Structure from Motion to constrain the z0 parameter in the model. Understanding of energy fluxes and ablation on Himalayan debris-covered glaciers is currently limited and this study provides useful insights into the validity of different treatments of the latent heat flux on glacial debris covers and the effects of averaging input data at 6 and 24 hour intervals. There are some deficiencies in the field data, but it the difficulty of obtaining data at remote high elevation sites such as this is acknowledged. I would, however, recommend the authors recalculate the sensitivity analysis using a more realistic range of variation in z0 and also consider whether their methodology, which involves optimising model parameters at different sites using field data, undermines the objective of evaluating the three different methods of estimating the latent heat flux. Otherwise, the paper is well written (apart from the title!), generally easy to follow and makes a valuable contribution to the literature.

Specific Comments

Sections 3.1. and 4.2: z0 estimation using Structure from Motion

How was the accuracy of the DEM assessed? You analyse variations at the 0.01 m scale in estimation of z0; is this resolution justified? The maximum area of the DEM, according to the values given at line 20 on p. 3510, is 4 mˆ2, but how much of this area is useable for assessing microtopography?

SfM is not my area of expertise, so I cannot comment on the validity of the methodology, but I like the fact that your approach attempts to generalise the characteristic roughness over the area of the DEM and generate a probable range of z0 rather than specific z0 values on individual profiles. Something that is missing from the discussion in Section 4.2, however, is an appreciation that the atmospheric boundary layer and its corresponding z0 value develops over 100s of m of fetch. It is therefore questionable whether 4 mˆ2 'samples' of the glacier microtopography are sufficient to characterise the surface roughness over such large areas, and it is doubtful whether the wind profile would be able to adjust to order of magnitude roughness changes (table 2) over the short distances in your study area. Consequently it is physically meaningless to have z0 as a tuneable parameter at individual stakes. It would be more realistic if the 4 SfM sites were combined to generate a single z0 value, and its range.

Section 4.1 Calculation of thermal Conductivity

Why were only the near-surface thermistors down to 20 cm used and not the deeper thermistors at sites 4 and 11? This implies that the k calculation is biased towards the openwork clast layers at the surface and not the more compact and humid lower debris layers. It could be reasoned that as void spaces of deeper layers are filled with water and fine rock material the thermal conductivity here would be higher than near the surface where void spaces are filled with low conductivity air. This implies an underestimation of the full depth k due to the use of temperature data only from the upper layers. This reasoning is supported by the fact that in the calibration of model parameters in Section 5.1 the optimal k value is at the maximum for most sites, implying the true value is greater than 1.62.

You use values from Nicholson and Benn 2012 for debris characteristics in the k calculation. Are these values representative for the sites you measured, and how sensitive is the k value to e.g. a 10 % change in porosity?

Section 4.3

It probably doesn't need pointing out, but it is a shame that most of the stakes melted out. Why didn't you drill the stakes in deeper, e.g. 2 m?

Section 5.1

A problem with the method of tuning model parameters separately for each of the 3 different methods of estimating the turbulent fluxes is that the parameter values will compensate for any errors in the turbulent flux calculation. For example, in the LE_Dry method, neglect of the energy used in evaporating water could lead to overestimation of the conductive heat flux, however this is offset by the relatively high albedo and z0 optimised for this run (table 3) which reduce net shortwave radiation and increase sensible heat transfer away from the surface during the daytime. This makes interpretation of the differences in performance of the model with the three different LE flux formulations difficult. Maybe you should select one set of optimal parameters and look at the differences in performance between the three model formulations again. At least this issue needs some discussion.

TCD
9, C1375–C1379, 2015

Section 5.4

I think you should redo the sensitivity analysis, varying each parameter in its range of uncertainty rather than a flat value of +/- 10%. It is obvious a priori that varying z0 by +/- 10% will have little effect on the magnitude of the turbulent fluxes when this parameter can vary across several orders of magnitude. In contrast, a variation in albedo of 10% is quite a significant change as its range of variation is much smaller. This would give a better assessment of model uncertainty which reflects the possible range of values of the input parameters.

Minor Corrections

Paper title. You need to insert ‘glacier’ between ‘Debris-covered’ and ‘energy’. As it stands, the title literally means that the model is debris-covered, not the glacier. Make this change everywhere the phrase ‘Debris-covered energy balance model’ appears in the paper, e.g. section 3.2 title.

3506, 17, I think you mean partial density of water vapour, not water vapour pressure. If vapour pressure was the same at 2 m as at the surface, there would be no vapour pressure gradient and hence no latent heat flux.

3507, 19, ‘cobble and gravel’.

3509. Section 2.2, make it clear that the Pyramid Station is an off-glacier station.

3519, 23, these are not surface temperatures but temperatures at 0.01 m depth – an important distinction.

3519, 25, ‘. . .this particular temperature sensor. . .’, which one? Be specific.

3521, 14, I think you mean overestimate not underestimate.

3522, 12, you can’t say that thin debris ‘promotes ablation’ as you have not measured bare ice melt rates you can’t determine that the melt rate beneath thin debris is greater than that for bare ice. In all likelihood even the thinnest of your debris layers is reducing

TCD
9, C1375–C1379, 2015

Interactive Comment

ablation through insulation.

3523, 20, you can't say the model agrees with the ablation stake data, when the stakes have melted out. All you can say is that the stake data do not contradict the model calculations.

Figure 1. What is the background image? Add this information to the caption.

Figure 2. State the size of the target discs in the caption.

Figure 5. Why is the ablation for LE_Dry so much higher than the other two methods for the thinnest debris layer, compared with the other sites? There is an inconsistency in terminology with the text here: LEDry, LEZero.

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TCD
9, C1375–C1379, 2015