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Interactive comment on "Incorporating moisture content in surface energy balance modeling of a debris-covered glacier" *by* Alexandra Giese et al.

Anonymous Referee #1

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Review of Giese et al. (2019) Incorporating moisture content in surface energy balance modelling of a debris-covered glacier

Giese et al. present the adaptation of the ISBA model to represent supraglacial debris, and hence allow the calculation of sub-debris melt using this model. The key advance is the incorporation of moisture within the debris, which can vary over time and with depth, and which then influences the thermal properties of the layer and hence the latent heat flux and melt. The authors test their model on West Changri Nup Glacier and conduct an uncertainty analysis by varying parameters based on ranges found from the literature. They also conduct runs under varying amounts of debris saturation. They describe the performance of the model and discuss the processes it includes compared to other models of sub-debris melt.

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Discussion paper

General comments:

It is certainly a good advance to include the moisture explicitly in a model of sub-debris melt and modifying ISBA to do this seems like a good idea and something I would like to see published. The paper is well written on the whole and explains the model advance quite well. Its concisely written and the overall approach seems robust. There are likely still some parts of the model which are less like the reality in the debris (e.g. the overestimation of the sensible heat flux and possibly moisture being held in the lowest debris layer) but nevertheless this is an important step forward.

In places the paper can lose clarity a little, and could do with explaining some parts of the model, and especially the collected field data more explicitly (see specific comments for details). Moreover, I think a key thing which is missing is the ability of the authors to really bring out the understanding of the interplay between the energy and moisture fluxes and the changes to the debris properties and how this affects melt overall. This could be strengthened in the results and discussion by better explaining the links between the debris properties and moisture content when explaining the results (improving some of the figures would help in this regard too). For instance, a clear explanation is missing of how the moisture content, thermal conductivity and heat capacity evolve on a daily and seasonal scale, of why moisture tends to be concentrated in the lowest layer and what the explicit differences are between the dry and saturated conditions.

Also, although the future directions section compares the results with the broader literature, this could be strengthened by having a clearer uncertainty analysis (and hence clearer recommendations for where data and understanding is lacking) and a clearer idea about what steps would be needed to bring this modelling from the point to the glacier scale. Furthermore, do you think that the findings determined for this glacier would hold for others, for instance with finer or thicker debris? At the moment the paper presents the new modelling (which is great to see), but I think it would really increase the usefulness of the paper if it could go beyond this a bit more to both better explain

the findings and think more broadly about the consequences of those findings. I would also say that the key 'take home messages' of the paper are not so clear. I feel less sure about what the new insights are which the modelling has made possible, the authors could therefore make this much clearer in the results/discussion and conclusions.

One other point is the use of the Reid and Brock (2010) parameters for dry debris, when the values were measured under conditions when there was likely melting at the ice-debris interface, and so the values would be for partially saturated conditions.

Specific comments:

Pg 2 Line 19: Reid and Brock don't necessarily assume 'dry' debris, just assume the debris characteristics are constant and the same as the average measured conditions.

Pg 2 It would be worth mentioning Evatt et al. (2015) Glacial melt under a porous debris layer, somewhere in the introduction (likely in the paragraph beginning on line 18), given their inclusion of the evaporative heat flux at the bottom of the debris layer.

Table 1: It would be better if the equations and the explanation in the caption could be taken out of this table and moved to Supplementary material. It is not clear where the values for ice and water conductivity and volumetric heat capacity are from (even if they are standard values). Given the importance of these parameters it would help to clearly explain the relation between them in the introduction. Also, it is unclear why the Reid and Brock (2010) value of thermal conductivity is used for dry debris, given that in the caption it is termed an 'effective' value from the time of measurements (this is confirmed in Brock et al, 2010). It is likely that this value is for a partially water saturated debris layer, rather than a dry one. Even if you decide this is a value for 'dry debris' then I think you would need to know the porosity of the debris on Miage Glacier to determine the 'debris' thermal conductivity so that you could then calculate water and ice saturated debris. I am not convinced therefore that the calculation of the water and ice saturated debris is correct. Fully name the parameters, e.g. Thermal conductivity, Specific heat capacity.

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Discussion paper

Filed site: it would be good to get an idea of the debris thickness, grain size and geology for West Changri Nup.

Figure 1: This map could be improved. The text on the inset map is unreadable, and the inset needs a scale bar. The main pane is too zoomed in, it would be better to show the full outline of both glaciers and label them.

Pg 5 Line 23: 'air temperature, humidity and surface fluxes. . .' Also, for clarity would be better to state the surface fluxes measured (incoming and outgoing short and longwave radiation).

Figure 2: Is the heat flux due to precipitation calculated?

Pg 6 line 1: Is the surface debris temperature calculated, or taken from measurements?

Pg 6 line 8 It is not clear what data is used for the spin up, 40 x the met data?

Pg 6 How many layers are for snow?

Figure 3: Give the symbols for the input variables within this figure (especially the top right bubble), so it is absolutely clear what the input variables are. In the third box down, surely all the fluxes (not just latent heat) are calculated?

Pg 8 line 6: Refer here to the supplementary material where you give the full energy balance.

Equation 3: Should the wi not be wmin in the brackets?

Pg 8 Line 27: 'where Lm is the. . .' Also it would be useful to explain in words what each part of these equations represent. I'm a little unclear about the source and sink terms and how they represent the system.

Pg 9 Line 10: The lack of ice growth in subfreezing temperatures may suggest a lack of vapour transport, but what about in above freezing temperatures?

Pg 9 Line 14: How was the isothermal vapour conductivity derived, or what value was

used?

Pg 9 Line 15: The observations of moisture transport, are these yours, or another author? It would be interesting to know of measurements of moisture transport if these are available. If based on your observations then that's fine but make this clear.

Pg 9 Line 24 Should there not be brackets around wi/w (following Boone et al., 2000)?

Pg 9 Line 32: It would be worth mentioning that debris tends to coarsen upwards (e.g. as mentioned by Reid and Brock, 2010).

Pg 10 Line 6-7: Do you mean with depth of water or with depth in the debris? And why would it increase with depth (of debris, especially?)

Equation 8: I can't see Δzj or wj defined anywhere? (I think the subscript j just needs defined somewhere, unless I've missed it)

Figure 4: Is this depth of the debris or water depth on the y-axis?

Section 4: It would probably be more useful to have the forcing before the model, although I understand that you are presenting a new model method. It is just easier to follow. The meteorological variables could easily be listed or put in a table (maybe refer to Table 2), there can't be that many. The section on in-situ meteorological measurements is lacking detail. How was the surface debris temperature situated (debris surface temperatures are easily overestimated), also give the depths of the sensors? How was ablation measured (with a UDG?) Give a reference for the ISBA-DEB being insensitive to the CO2 flux. The point about the precipitation should be moved to section 4.2 (in fact the section headings 4.1, 4.2 and 4.3 could easily be removed). How was debris density and porosity measured?

Pg 12 Line19: When does the precipitation dataset end? How was the precipitation at Pyramid corrected (or give a reference that explains)?

Pg 15 Line 21 Give the range in error of $\tau\alpha$ and τmax to show that the actual error

values were similar.

Figure 6 it would be more useful to show the mean diurnal values, or at least only the time when both are compared within the same depth, with measured and modelled on the same panel. This would make it much easier to compare the measured and modelled outputs.

Figure 7 Text on these figures could be a little larger. On b) consider including the air temperature and debris surface temperature.

Pg 17 Line 8 The above freezing temperatures propagating into the ice is shown better in Figure S2.

Figure 8 Really Figure S2 is more useful than this figure, consider swapping the two.

Page 18 The description of the change in the debris moisture content is a bit too terse. It would be helpful to describe this more thoroughly, especially as this is a key new function of the model. How does the thermal conductivity and heat capacity vary with the moisture content on a daily and seasonal scale? Also, explain why only the bottom layer is holding water.

Figure 9 It would help to add side panels to both a) and b) of the diurnal average values for each layer (perhaps just during melting). This would help interpretation.

Figure 10 This would be fine as a table, with the 2014 results given too. I am surprised that the results for the full model run with the integrated moisture content is not included too?

Pg 20 Line 5 Compare the dry and saturated debris directly for clarity, i.e. the thermal diffusivity of dry debris is around double that of saturated debris, hence the higher melt under dry debris.

Pg 20 I don't think the model includes the wind dynamics within the debris (unlike Evatt et al., 2015), would doing so change these results?

Figure 11 Again it would be helpful to show the mean diurnal patterns in each layer as a side panel, especially within the ablation season.

Figure 12 Consider changing the upper panel title to 'Debris surface latent heat flux from atmospheric exchange'. Also label panels a) and b) as for the others.

Page 22 Line 5 Including debris moisture does not significantly decrease sub-debris melt – compared to what? (the dry scenario?)

Pg 22 Line 8 Can you explain why only the lowermost layer holds water, is this to do with the runoff parameterisation or the debris porosity?

Pg 22 Line 19 Figure 12 is missing the cumulative flux used for melt.

Pg 23 Line 3 In Figure 10 though it seems to show similar melt between dry and partially debris?

Pg 23 Lines 15-27 Consider just citing the papers in Table 3 to shorten this section.

Table 3 Just give the symbols rather than the shortened variable names in the first column. It would also be helpful to know the % change of the parameters, otherwise its tricky to determine which parameters the model is most sensitive too. I understand the overall rationale of using the literature values to give sensible ranges but this would help clarify the sensitivity of the model.

Pg 24 Line 1 For the extreme values tested, is this with the other parameters at their * value?

Pg 25 Lines 1-3 A more thorough description of which variables the model is most sensitive to is needed here (as mentioned would help to show the % change in each variable) and an idea of which ones are most likely to be well or not well known. Then suggest where work should concentrate to improve the knowledge of the most sensitive variables, including of their spatial distribution.

Pg 27 Lines 6-16 This very high surface temperature could be indicative of the debris

properties not being correct, as the model might be compensating by increasing the debris surface temperature. I am presuming here that the debris surface temperature is modelled (I think it is, although I am not sure if this explicitly stated).

Pg 28 Lines 1-2 Here it would be useful if the authors could comment on what they think would happen over a real debris-covered surface where there is a combination of steep and shallow slopes. What would likely be the overall affect? On line 1 specifically the authors mention' overlying flatter glaciers' which is a strange term, say specifically flatter slopes, as the slope angle will vary across the glacier surface (especially for debris-covered glaciers).

Pg 28 Line 11 what ratio are you referring to here?

Pg 28 Line 33 Explain why moisture deep in the debris is less prone to evaporation. (Possibly because of lower wind speeds and cooler temperatures at depth, but explain this based on your results).

Pg 29 Line 23 'snow melt rate' ? Also consider 'measured snow melt' instead of 'the SR50 data' just so it is clear what you are meaning without readers knowing your sensor names.

Table A1 I still have concerns over using the Reid and Brock (2010) values of debris thermal conductivity and heat capacity for dry debris. There are no values given for the matric potential at saturation or b (what is this?). I understand they are predicted values, but does this mean they change over time if one value isn't given?

Supplementary Material

I might have missed it but please refer to the energy budget section in the main paper. Also, it would be helpful to include the equations for the sensible and latent heat fluxes, since these can vary between models.

Pg 3 Equation 13 LEgu I think should be LEgi (or vice versa) to match the text.

Pg 3 Equation 15 LEgf I think should also be LEgi? I can't see Wn defined, should this be the change in water stored in snow (Ws)?

Pg 3 Line 15 is Ps the melted snow in water equivalent?

Figure S2 This is a useful figure and I would suggest including it in the main text. It would be useful to have the rain and snowmelt also in mm (to match glacier melt). Rename Layer 13 as the ice surface (rather than glacier surface). Write 'Temperature' rather than just T in the legend, and say ice content and water content, rather than just ice and water, for clarity. Ideally the middle four panels should have the same y-axis scale for comparison. Alternatively show all 4 layers of water content together in a pane, with separate panes for ice content and temperature.

Technical corrections:

Pg 1 line 12 'debris-covered'

Pg 4 Line 3 and line 9 'debris-covered'

Pg 4 line 9: 'mostly composed of clean ice.'

Pg 12 Line8: delete 'dot in'. 'AWS data was measured half hourly from the 6th December 212 15:00 – 28 November 2014 13:30 local Nepal time, and provided'

Pg 15 Line 18 'measured ones from'

Pg 17 Line 2 'Figure S2'

Pg 25 last paragraph, join this to the next paragraph.

Pg 27 Line 24 'using an eddy correlation approach'.

Pg 28 Line 4 'thermal conductivity'

Pg 28 Line 5 'varied measurements of roughness length' (I am presuming here)

Pg 28 Line 12 'Measured ablation over the two years modeled' Strange phrasing here,

reword.

Pg 28 Lines 12-19 Consider shortening this section.

Pg 28 Line 30 ‘in comparing their two data collection sites,’

Pg 29 Line 20 ‘It also computes glacier melt’

Figure S1 Add a y-axis title ‘Energy flux (W m-2)’

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