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Discussions

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

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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.
First, we want to thank the reviewer for his/her thorough review, which greatly improved this manuscript. We have bolstered the discussion on the sensible heat flux overes-

timation, including by describing equifinality, and explained the moisture distribution and debris in greater detail. The concentration of moisture at the debris – ice transition matches both our own and others' (Collier et al., 2014; Nicholson and 10 Benn, 2012; Rounce and McKinney, 2014) observations but is, as we show, also a function of glacier slope (with flatter debris layers holding more moisture).

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).
We have added "Field Site and Data" before the model description (Section 3).
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.
Our revised text highlights these relationships, particularly a central takeaway, "not only does a wet debris layer transfer heat less efficiently from its surface to its base than dry debris because of a decreased thermal diffusivity, but also it has less energy to transfer in the first place because of the other energy fluxes (mainly the surface latent heat) associated with the scenario."
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.
We strengthened our description of the links between the debris properties and moisture. For example, "During the summer, the glacier is melting, and the bottommost layer of debris is almost always saturated with liquid water, such that it has a thermal conductivity K of $\sim$*1.16* $W\ m^{-1} K^{-1}$ *(Table 1). The thermal conductivity of layer*

13 changes little throughout the day; layer 1 shows the most variation in conductivity because its water content experiences the most variation (condensation followed by evaporation). A higher value means more water content while a lower value indicates dryness. The diurnal patterns in conductivity (Figure S4a) are replicated in specific heat capacity (Figure S4b) because both are functions of water content, and both thermal conductivity and heat capacity have higher values for the water-saturated debris in layer 13 than the drier debris in the overlying layers." (Section 5.1)
We have addressed the concerns listed, and details on how we revised the manuscript and figures can be found in the "Specific Comments" Section below.

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.
Making a comprehensive uncertainty analysis, including Monte Carlo simulations, is beyond the scope of the paper, though such detailed studies have been undertaken for ISBA (Emery et al, 2016) and a SURFEX soil model (Garrigues et al, 2018). We believe that our sensitivity tests are sufficient for capturing uncertainties inherent to the model and uncertainties associated with the debris data paucity on West Changri Nup glacier. Accordingly, we changed the language surrounding the purpose of the sensitivity tests to convey their role and importance: "We performed sensitivity tests on the six parameters. . . to explore and quantify uncertainty associated with parameterizations in ISBA-DEB."
>>Emery, C., S. Biancamaria, A. Boone, P.-A. Garambois, B. Decharme, S. Ricci and M. Rochoux, 2016: Temporal variance-based sensitivity analysis of the large scale hydrological model ISBA-TRIP: Application on the Amazon basin. J. Hydrometeor., 17, 3007-3027.
>>Garrigues, S., A. Boone, B. Decharme, A. Olioso, C. Albergel, J-C. Calvet, S. Moulin, S. Buis, E. Martin, 2018: Impacts of the soil water transfer parametrization on

the simulation of evapotranspiration over a 14- year Mediterranean crop succession. J. Hydrometeor., 19 (1), 3-25.
Additionally, it is worth noting that modeling on the glacier scale was one of our initial goals, but our focus was on 1 dimension after deciding to adapt ISBA-DEB. At this stage, such scaling (point to glacier-wide) is not possible. Nevertheless, we believe that our study can help to inform development of a spatially distributed model through our findings about the most sensitive parameters controlling the melt at point scale. Such an assertion/sentiment has been added to the Conclusion.

Furthermore, do you think that the findings determined for this glacier would hold for others, for instance with finer or thicker debris?
The idea is certainly to apply ISBA-DEB to other debris covers, and we try to make the user flexibility (and model applicability) clear through Figure 3 and Table A1. We did not run ISBA-DEB for a debris cover of a different porosity or thickness, so we cannot include such details.
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.
The main purpose of this paper is to present the new modeling; we are happy that the reviewer considers this interesting. In the revised version, we distill take home messages and highlight the paper's contribution to debris-covered glacier surface energy balance modeling. These are now summarized in our revised Conclusion, part of which reads, "In its simulations of West Changri Nup glacier, enhanced melt occurs below dry debris due to a combination of greater thermal diffusivity and little loss of energy to evaporation or sublimation. ISBA-DEB explicitly accounts for the

atmosphere-debris latent heat exchanges in the top (surface) layer of the debris only. The large difference in glacier melt below dry and saturated debris shows that latent heat is enormously important in removing energy from the system. Accounting for moisture in the conductive heat flux alone is insufficient when modeling melt under a debris-covered glacier. It is, therefore, an essential next step to examine and incorporate the latent heat exchanges of moisture at all depths in the debris.
ISBA-DEB provides a basis for developing a model that can be applied at the glacier scale by identifying not only the importance of atmospheric exchanges throughout the debris column but also the most sensitive parameters controlling the melt at point scale. In addition to using accurate roughness lengths, it is crucial to represent moisture sources and sinks correctly."

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.
Great point. We assume that Reid and Brock (2010)'s value for Miage glacier's debris cover is applicable to Changri Nup glacier in the absence of (a) other observations and of (b) a straightforward way to adopt geological values for solid bedrock (reported for very high temperatures and pressures) to porous covers of sand, gravel, cobble, and boulder sized debris clasts. We amended the language in the caption to Table 1 to make it clear that we make an assumption about the applicability of Reid and Brock (2010)'s value as opposed to mis-cite the source. We discuss this assumption, cite other K values, and calculate thermal conductivity for gneissic debris in the Discussion. The text now includes, the following:
"A study on Miage glacier, Italy provided 0.94 W $m^{-1}K^{-1}$ as a starting point for thermal conductivity tests (Reid and Brock, 2010), though we varied the conductivity values throughout the range reported in the literature, 0.60 to 1.29 W $m^{-1}K^{-1}$ (Rounce et al, 2015). This was a particularly important sensitivity test to perform because, as noted in the caption to Table 1, the thermal values we assumed valid for dry debris on West

Changri Nup glacier were "effective" values reported for Miage glacier. As Brock et al. (2010) measured thermal conductivity in the ablation season, the reported K of 0.94 W $m^{-1}K^{-1}$ (Reid and Brock, 2010) was likely higher than that of perfectly dry Miage glacier debris since any moisture in the pore spaces would have had K = 0.57 W $m^{-1}K^{-1}$ (water) rather than K = 0.024 W $m^{-1}K^{-1}$ (air). Additionally, debris on Miage glacier (Italy) may have a dramatically different lithology than the debris on Changri Nup glacier (Nepal). Reports of debris conductivity on Khumbu glacier, adjacent to Changri Nup, include 0.85 W $m^{-1}K^{-1}$, 1.28 W $m^{-1}K^{-1}$ (Conway and Rasmussen, 2000), and 0.96 W $m^{-1}K^{-1}$ (Rounce and McKinney, 2014) and indicate that 0.94 W $m^{-1}K^{-1}$ is not inappropriate to apply to West Changri Nup. West Changri Nup's debris is most likely comprised of the sillimanite gneiss that forms its surrounding mountains (Searle et al., 2003). The USGS's "Thermal Properties of Rocks" (Robertson, 1988) gives a thermal conductivity of 2 W $m^{-1}K^{-1}$ (see Figures 3 and 16 therein). For debris with 39% porosity and air-filled pore spaces, a weighted-average K for dry debris is 1.2 W $m^{-1}K^{-1}$, which is within our tested range. Thermal conductivity is difficult to measure in the field, and it is not known how transferable the limited available measurements are to other debris covers and conditions. It is also not known whether a weighted average of bedrock and air thermal properties is a valid representation of porous debris. Accordingly, we intended to encompass the true value(s) in the range for which we tested ISBA-DEB's response."

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.
Please see our response above including some of the text added to the manuscript.

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.
This reference has now been incorporated into the text.

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.
FIXED. Table 1 in the text includes no equations or references; all of that information is now part of a complementary table in the supplement.

It is not clear where the values for ice and water conductivity and volumetric heat capacity are from (even if they are standard values).
References for these values have been added.

Given the importance of these parameters it would help to clearly explain the relation between them in the introduction.
Added to the Introduction: "The efficiency with which a debris layer conducts heat is determined by its thermal diffusivity κ (m^2 s^{-1}). κ is given by thermal conductivity K (W m^{-1}K^{-1}) normalized by the volumetric heat capacity (J m^{-3}K^{-1}), itself product of density ρ (kg m^{-3}) and heat capacity c (J kg^{-1}K^{-1})."

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.
See response to the similar comments above.

Fully name the parameters, e.g. Thermal conductivity, Specific heat capacity.
FIXED.

Field site: it would be good to get an idea of the debris thickness, grain size and geology for West Changri Nup.
We add a citation for Lejeune et al (2013), which contains a picture and description. We also include, "The debris is a granitic metamorphic mix, likely consisting of gneissic clasts eroded from the surrounding cliffs."

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.
All suggested changes made.

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).
FIXED.

Figure 2: Is the heat flux due to precipitation calculated?
It is not. We have clarified this in the text.

Pg 6 line 1: Is the surface debris temperature calculated, or taken from measurements?
Neither. We have clarified this in the text: "The prognostic state variables are assumed to be located at the midpoint of each layer; accordingly, the uppermost simulated temperature is at 0.5 cm depth in the debris, not the surface."

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

Pg 6 How many layers are for snow?
We enlarged this detail in the model schematic and also added it to the text.

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?
FIXED. Yes all the fluxes are calculated; we aim to highlight differences between ISBA and ISBA-DEB.

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

Equation 3: Should the wi not be wmin in the brackets?
We do not understand this comment and have confirmed that equations (3) and (4) resemble those in the work cited.

Pg 8 Line 27: 'where Lm is the. . .'
FIXED.

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.
Clarification added to the text.

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?
Clarification added to the text.

Pg 9 Line 14: How was the isothermal vapour conductivity derived, or what value was used?
Reference added.

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.
We do not discuss observations of vapor transport but rather of water pooling at the ice-debris interface. We have clarified this in the text.

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

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

Added to the text.

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?)
The runoff timescale increases with depth in the debris, as water content tends to be higher with greater depth in the debris. An explanation for this distribution has been added to the manuscript in two different places.

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

Figure 4: Is this depth of the debris or water depth on the y-axis?
Y-axis label changed to "Depth from Debris Surface."

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?
The paper structure has been rearranged to include "In Situ Measurements" and

"Model Inputs" subsections within Section 2: "Field Site and Data," before the model description (Section 3), including all information from former sections 4.1-4.3. Forcing variables are now listed and details are added to the in situ measurements section (including on debris temperature sensor depths and ablation measurements). We changed the sentence about CO_2 to mention all SURFEX-required input.
SURFEX requires CO_2 flux because it includes a vegetation option we do not use. Despite the options to include soil organics (Decharme et al, 2016) or soil carbon (Delire et al., under revision), neither option is currently used for ISBA-DEB, the focus of which is rocky debris and moisture.
>>Delire, C., Seferian, R., Decharme, B., Alkama, R., Calvet, J., Carrer, D., Gibelin, A.-L., Joetzjer, E., Morel, X., and Rocher, M.: The global land carbon cycle simulated with ISBA: improvements over the last decade, Journal of Advances in Modeling Earth Systems, under revision.

Pg 12 Line19: When does the precipitation dataset end? How was the precipitation at Pyramid corrected (or give a reference that explains)?
We added a reference (Sherpa et al, 2017) and clarified that the precipitation dataset extends beyond the end of our study.

Pg 15 Line 21 Give the range in error of τ_α and $\tau_{\max}$ to show that the actual error values were similar.
The minimum RMSE, for τ_α =30, was 1.99995 which is a shallow minimum given an RMSE range of 1.99995 – 2.08845. The RMSEs for τ_{max} varied even less, with a range of 0.05; accordingly, we did not find a compelling justification to have the value of this parameter differ from the timescale of sandy soil to drain to its field capacity. While the suggestion to include these values in the text is certainly an important one, we decided against highlighting them for fear that they would confuse the majority of readers and detract from the message.

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.
It is a good idea to include a direct comparison of measured vs. modeled temperatures, and such figures have been added to the Supplement. We have updated the caption of this figure (formerly 6) to emphasize the reason for the figure (i.e. to show why we chose the tuning period we did).

Figure 7 Text on these figures could be a little larger. On b) consider including the air temperature and debris surface temperature.
Text size increased. We have considered adding air temperature and debris surface temperature to (b), but the figure became too noisy. Further, we wish to display model output, and the two suggested variables are both very different quantities (measurement and calculation, respectively, and not model output).

Pg 17 Line 8 The above freezing temperatures propagating into the ice is shown better in Figure S2.
We agree and refer instead to Figure S2 in the text for temperature propagation into the ice.

Figure 8 Really Figure S2 is more useful than this figure, consider swapping the two.
We considered this and think that Figure 8 is a more compelling figure for a non-modeler glaciologist and, as a result, have elected not to switch the figures. However, we have added a specific mention to S2 in the caption for 8. We leave it to the discretion of the editor whether to switch the figures (or to include both S2 and 8 in the

text).

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.
We added more descriptive language to Section 5.1.

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.
We generated the suggested figures and added them to the Supplement as including them as part of Figure 9 made the figure too busy and difficult to interpret.

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?
We converted this to a table, submitted as a supplement along with this response, with model output from both years and available point mass balance measurements. We erroneously compared model-computed melt to point mass balance in the original submission but have corrected this, along with reporting modeled mass balance components and both stake and SR50 depth gauge measurements. The "full model run with the integrated moisture content" is, indeed, included; it is labeled, "partially saturated."

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.

We added the suggested language to the text: "The fully saturated debris conducts heat that leads to melt with an efficiency of $\kappa = 3.572 \times 10^{-7}$ m^2 s^{-1}, while the dry debris has a diffusivity of 5.867×10^{-7} m^2 s^{-1}; hence, more melt occurs below the dry debris in ISBA-DEB."

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?

That is correct. We discuss the fact that ISBA-DEB neglects advection throughout the text, though we have added an explicit mention that advection includes the wind dynamics modeled by Evatt et al. (2015).

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.

Added to the Supplement and referenced in the caption for this figure.

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.

FIXED.

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

Yes. Language amended.

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?

We have added an explanation to the text: "The concentration of wetness at the debris

base is due both to the fact that debris coarsens upward (Reid and Brock, 2010) and to the permeability of the overlying debris (precipitation quickly moves through the debris until it reaches the impermeable ice surface)."

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

Our original manuscript stated that "cumulative flux used for melt" was part of figure 12. In the submitted version, we intended to show only latent heat fluxes and have removed the language in the text from a previous revision.

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

This sentence does not appear in the revised manuscript, making this comment moot.

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

In this section, all parameters are described in the text, not just the roughness lengths in lines 15 – 27. We elected to keep the section the way it was originally written, but, if brevity is at a premium, we can shorten it. We leave that to the discretion of the editor.

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.

Symbols FIXED, and we now give % of physically plausible range in the value of the parameter to indicate impact on the results.

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

value?
Yes; caption updated.

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.
We have enhanced the Sensitivity Tests section by clearly stating which parameters the model is most sensitive to, mentioning when a parameter is difficult to measure or otherwise constrain, and suggesting where to focus future work. We updated the table with the suggested %s.

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).
Indeed, if any of the simulated incoming components to the energy balance are too large, the model could potentially compensate by overestimating the debris surface temp. In the text, we have added that there may be an error compensation (equifinality problem), where multiple parameter sets provide equally good model outputs.

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).

Phrasing FIXED, and we have added a discussion of applying ISBA-DEB to an entire debris cover.

Pg 28 Line 11 what ratio are you referring to here?
Ratio of momentum to thermal roughness lengths. This has been clarified in the text.

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).
We have added the explanation: "in contrast to soil, supraglacial debris has a higher permeability and lower evaporation rate. A lower evaporation rate is consistent with the fact that debris stores moisture at depth. Moisture deep in debris is less prone to evaporation than moisture on the surface since permeability (and, thus, ease of air flow) generally decreases into the debris although some does evaporate."

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.
Clarifying language added.

Table A1 I still have concerns over using the Reid and Brock (2010) values of debris thermal conductivity and heat capacity for dry debris.
See above.
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?
Description of b added to subsection 3.3.3 with the introduction of PTFs. Values for b

and matric potential at saturation are calculated and added to Table A2.

Supplementary Material:
I might have missed it but please refer to the energy budget section in the main paper.
FIXED.

Also, it would be helpful to include the equations for the sensible and latent heat fluxes, since these can vary between models.
We have added a citation.

Pg 3 Equation 13 LEgu I think should be LEgi (or vice versa) to match the text. C8
FIXED.

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)?
FIXED.

Pg 3 Line 15 is Ps the melted snow in water equivalent?
It is the snowfall rate in water equivalent, noted on the same page.

Figure S2 This is a useful figure and I would suggest including it in the main text.
See above for rationale behind keeping it in the Supplement.

It would be useful to have the rain and snowmelt also in mm (to match glacier melt).

FIXED.

Rename Layer 13 as the ice surface (rather than glacier surface).
FIXED.

Write 'Temperature' rather than just T in the legend, and say ice content and water content, rather than just ice and water, for clarity.
We have added these clarifications to the caption, as adding them to the legend made the figure too cluttered.
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.
We agree that consistency is important but found that the alternative suggestions do not display the model behavior as well. We have added a note to the figure caption pointing out the larger y-axis range of panel 5.

Technical corrections:
Pg 1 line 12 'debris-covered'
FIXED.

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

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

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'
This sentence was rewritten responding to other reviewer feedback making this comment moot.

Pg 15 Line 18 'measured ones from'
FIXED.

Pg 17 Line 2 'Figure S2'
FIXED.

Pg 25 last paragraph, join this to the next paragraph.
We understand that the second paragraph in question is only a sentence in length. We decide to keep it separate because it concerns a different subject.

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

Pg 28 Line 4 'thermal conductivity'
FIXED.

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

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

reword.
Sentence omitted in revised version.

Pg 28 Lines 12-19 Consider shortening this section.
This has been shortened to a focus on temperature sensors.

Pg 28 Line 30 'in comparing their two data collection sites,'
FIXED.

Pg 29 Line 20 'It also computes glacier melt'
FIXED.

Figure S1 Add a y-axis title 'Energy flux (W m-2)'
FIXED.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-168/tc-2019-168-AC2-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-168, 2019.