Dear Prof. Hauck,

We thank both Reviewers for their thorough comments on our manuscript, and given that both reviews raised similar comments we provide a response to both together here. We are willing and able to address all the major and minor comments raised in the reviews to produce a revised manuscript with a tighter focus on the presentation and implications of our new data describing supraglacial debris temperatures on Khumbu Glacier.

We provide a response to all of the points from both reviews but have not yet uploaded a revised manuscript, as before we finalise a revised manuscript, we wish to first ascertain that the revisions proposed here are deemed to meet the requirements for publication in *The Cyrosphere*.

With best wishes,

Ann Rowan and co-authors

05/04/18

Summary of Reviews

The Reviewers consider the scope of the manuscript to be too broad in its present form. No concerns are raised about our primary data collection and analysis.

Reviewer #2 lists the five questions that the manuscript seeks to address and suggests that we focus on fewer of these.

Neither Reviewer considers any of these topics to be difficult to answer for debris-covered glaciers in the Himalaya and instead request only that we limit the focus of our study.

The main objection is what we claim our data means; we assert something unsubstantiated with the ablation calculations, as there are differences in the total ablation calculated using each method and stake measurements for validation were only available for one site (IM13). Both Reviewers wished to see our thermal diffusion model applied to calculate ablation here as this offered the potential to evaluate our ablation calculations.

Authors' Response to Main Points Raised

A revision would be modified from the original submission in the following ways;

1. Our revised manuscript will focus more tightly on what can be observed from the unique dataset that we have assembled describing multiannual supraglacial debris temperatures in the Everest region of Nepal. In doing so we would address only two research questions: (a) *How do debris temperature profiles vary in the Everest region?* and (b) *What are air and debris temperatures on Khumbu Glacier?* as suggested by Reviewer #2, thereby retaining the core of our study which is the presentation and analysis of new data from Khumbu Glacier and the comparison with similar observations at Ngozumpa and Imja-Lhotse Shar Glaciers.

2. The motivation of our revised manuscript is that seasonal sub-debris ablation is difficult to measure directly and thus any suitable data are scarce. In response to this problem, our goal is to see what can be gleaned from relatively more abundant temperature (heat flux) measurements. The manuscript will be express this more clearly at the outset and refer explicitly back to this motivation in the discussion and conclusions.

3. Section 6.4 'Contribution of sub-debris ablation to glacier-wide mass balance' and any related text that discussed the implications of our results for calculations of glacier-wide ablation by comparison with geodetic measurements of surface lowering/mass balance, and suggested causes for the discrepancy between these results and our calculated ablation values, will be removed as part of this processes of narrowing the focus of the paper. This addresses the reviewers concerns about making conjectures beyond the scope of our data.

4. In the discussion section, we will address the issue of *which melt modelling approach is best for estimating sub-debris melt across debris-covered glaciers?*, specifically for the monsoon season (3 June to 15 October) that encompasses the entire ablation season (Nicholson and Benn, 2012, *ESPL*). However we will exclude the

DDF approach, as this is based on data from a much shorter period (21 May to 1 June 1999). The text describing the application and results of the ablation calculations would therefore be considerably shortened and would only compare the downward heat flux and thermal diffusion methods to consider the causes of mismatches between estimates of ablation using different methods.

5. We have now applied the thermal diffusion model to the three sites at Imja-Lhotse Shar Glacier where ablation stakes were installed and will add these results to the revised manuscript. Results from Imja-Lhotse Shar Glacier are useful to evaluate the model as both reviewers identified, and indicate similar total ablation to stake measurements with a slightly better agreement between measurements when the moist debris layer is simulated. These results have been incorporated into the revised Table 5 and Figure 10, presented below. We noticed that the results from the experiment with a moist debris layer had been incorrectly sampled for the KH and NG sites to give lower-than-expected values. This has been corrected in the revised Table 5. These results demonstrate that the thermal diffusion model is able to simulate seasonal debris temperature-depth profiles ($\delta T_d/\delta h_d$) that give a good match to observations for debris thicknesses up to 0.5 m. The inclusion of moisture within the debris layer makes little difference to the calculated $\delta T_d/\delta h_d$ although simulated ablation is lower in each case where debris moisture is included. Total simulated ablation using a dry and wet debris layer are similar at Imja-Lhtose Shar Glacier and within 10% of values derived from stake measurements at IM13 and IM14.

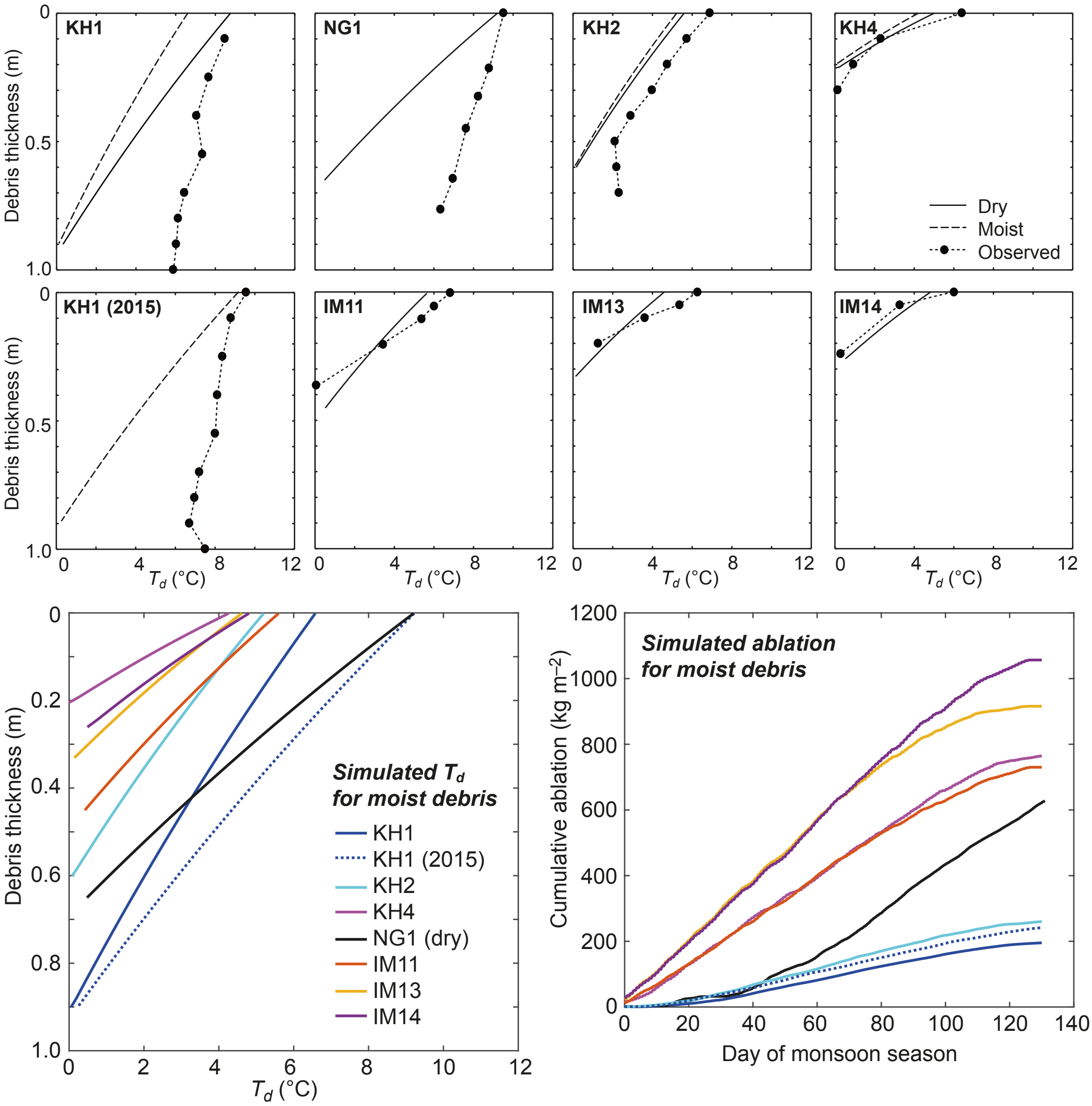
6. To simplify the modelling section, we have removed the former Experiment 2 that reproduced Experiment 1 using glacier-specific *k* values. The experiment comparing moist to the dry debris layer simulations has therefore been renamed Experiment 2.

We expect that these revisions alongside additional editing of the text for brevity will result in a reduction in the overall length of the manuscript of at least 15%.

Revised caption: Table 5. Results from the thermal diffusion model using a generic whole-rock *k* value of 2.5 W m^{-1} $°C^{-1}$ for dry and moist debris layers. The results for sites on Imja-Lhotse Shar Glacier are compared to ablation stake measurements and as such the model results cover a longer period (31 May to 9 November 2014) than the monsoon interval used for the other sites (3 June to 11 October) to enable direct comparison with the stake measurements.

				Ablation calculated using downward heat flux			Ablation calculated using the thermal diffusion model (kg m^{-2})	
Site	Year	Debris thickness (m)	Ablation from stake measurements (mm w.e.)	Mean daily T_s during the monsoon season	Downward energy flux to ice (W m^{-2})	Total ablation (mm w.e.)	Experiment 1: dry debris layer	Experiment 2. moist debris layer
KH1	2014	*1.50*	-	8.8	5.7	216	-	195
KH1	2015	*1.50*	-	7.5	4.9	184	380	241
KH2	2014	0.70	-	7.0	9.8	369	387	259
KH4	2014	0.30	-	6.8	22.2	836	1036	764
NG1	2002	*2.00*	-	8.4	6.0	226	627	-
NG2	2015	1.80	-	7.0	5.6	209	-	-
IM4	2014	*1.50*	-	10.6	14.0	527	-	-
IM11	2014	0.45	>1000	7.6	33.4	1259	737	729
IM13	2014	0.33	850	7.8	46.8	1762	933	915
IM14	2014	0.26	>1000	6.4	48.7	1835	1088	1056

Revised caption: Figure 10. Results from thermal diffusion model experiments showing simulated $\delta T_d/\delta h_d$ through moist and dry debris compared to observations at KH1 (2014 and 2015), KH2, KH4, NG1, IM11, IM13 and IM14 using a whole-rock k value of 2.5 W m^{-1} °C^{-1}, and the simulated cumulative ablation at each site assuming either a moist or a dry debris layer. Note that where thermistors were not installed at the debris–ice interface at KH1, NG1, IM11 and IM13 the debris thickness simulated here is a minimum and T_d is assumed to be zero at this minimum thickness, such that the model gives an underestimate of ablation for these sites. Note that the moist debris results are shown in each case (with the exception of NG1); in the IM11, IM13 and IM14 profile plots these overlap with the simulated $\delta T_d/\delta h_d$ dry profiles and are not visible.



Response to Specific Comments

The Reviewers' specific comments are presented below in *blue italic text* and our responses follow each point in plain text.

Anonymous Referee #1

Received and published: 19 January 2018

Dear Editor and authors,

Review comments on "Multiannual observations and modelling of seasonal thermal profiles through supraglacial debris in the Central Himalaya" by Rowan et al., submitted to The Cryosphere Discussion.

Overview

Rowan and her colleagues conducted a series of in situ observations of air, surface and internal temperatures on debris covered sites of Khumbu Glacier, Nepal. Comparing with other sites on neighbor glaciers, they described seasonal variability of the internal temperature of debris layer. They also applied three different models to calculate ablation, ice melting rate under the debris, and then asserted that the sub-debris ablation is not sufficient to account for observed surface lowering by remotely sensed approach. They concluded that mass loss by supraglacial and englacial processes (pond and ice cliff) should be important. In the Himalayas, huge efforts are required to conduct field observations. The authors had to excavate debris pits deeper than 1-m depth for setting thermometers. However, though I am not going to discredit the authors' effort to get the precious observational data from the Himalayan field, I found a serious gap between the main conclusion and their approach. Both observational data and model calculations do not support the conclusion.

Major Issue

In my understanding, motivation of this study is understanding sub-debris ablation (line 109). To achieve this purpose, the main authors set thermometers in debris and collected similar temperature data from the coauthors, and applied three different models to estimate sub-debris ablation with the observational data. However, the study provides only one measurement of the ice ablation. Both calculated ablation rates by degree-day approach and heat flux model were not consistent with the observed ablation. Thermal diffusion model was not tested for the site where the observational data was available. It means that, though some calculations well represent the observed debris temperature profiles, the calculations of sub-debris ablation were not validated at all. However, the authors proceeded their speculation, and concluded that the observed surface lowering (by other remote sensing based studies) was attributed to supraglacial and englacial processes, but no evidence was provided. This study fatally lack field data/evidence for the motivation/purpose.

Both Reviewers raise an issue with the estimates of ablation that we have calculated from our debris temperature measurements as only three ablation stake measurements are available (of which two only indicate minimum values) with which to evaluate these calculations. Ablation stake measurements are arduous to make, and do not represent the high spatial variability in ablation found on debris-covered glaciers. As improvements are being made in estimating debris thickness remotely from thermal satellite data, the aim of our original study was to develop a method to more efficiently estimate glacier-wide ablation and we attempted to present each method impartially with comments on the advantages and limitations of each (Section 6) in the absence of ablation data to compare these results to, which was stated cautiously in the first paragraph of the Conclusions.

As this is not convincing, we have removed Section 6.4 that addressed glacier-wide mass balance along with text that referred to this section throughout the manuscript and edited the Conclusions to instead focus on the first part of this problem, the seasonal thermal properties of supraglacial debris with the encouraging conclusion remaining that thermal profiles are similar between sites and between years (Fig. 9), indicating that this parameter does not need to be measured in every subsequent study. The thermal diffusion model is useful here as it shows that our measured thermal profiles can be predicted from meteorological data and debris surface temperature measurements, indicating that this approach may be suitable for estimating glacier-wide ablation once appropriate calibration has taken place, which is beyond the scope of the current study.

Major Comments

The last sentence of short summary is problematic. "(We) found that sub-debris ice melt can be predicted using a temperature–depth relationship with surface temperature data." This, "melt can be predicted", is not supported by the study at all.

This sentence has been removed as the manuscript has been refocused.

L83-: I do not understand why this sentence necessary. Neither future projection nor area change of debris-covered glaciers fits the purpose of this study.

L85-: I agree with this sentence, but this study revealed neither spatial variability of debris thickness nor glacier-wide mass balance.

These two sentences have been removed.

L94-: Did this study find any method to calculate spatial distribution of sub-debris ablation? I do not think so.
The citation to Benn et al. (2012) is used here as it confirms the Reviewer's comment that differential ablation occurs but is difficult to calculate.

L108: Only one data of stake ablation is available and contribution of sub-debris to glacier-wide mass loss was not quantified in this study. Rephrase this sentence.
This sentence has been edited to "*We compared our calculated ablation values with stake measurements that were only available on one glacier*.". Three stake measurements are available from Imja-Lhotse Shar Glacier, although two of these stakes melted out completely during the measurement period they do give a minimum estimate of ablation.

L158-: The paragraph looks a part of discussion. It is unclear how this description relates to this study.
This paragraph summarised DDFs measured for debris-covered glaciers and has been removed.

L176: Pay respect to the original studies for the Pyramid Observatory data.
We apologise for not providing the citations for these data here, as they were included where the data is presented in Section 4.1. We have added these citations to Bollasina et al. (2002) and Bonasoni et al. (2010) here as requested.

L249: Debris pit IM4 did not reach to debris-ice interface so that use of term "debris thickness" is misleading.
In Tables 1 and 4 we have indicated where debris thicknesses are estimated rather than measured but had not done this here in the text. We have added a comment to clarify this here and edited this sentence to "*At IM4, the debris a pit was excavated to a depth of 0.83 m did not reach the debris-ice interface, the total debris thickness was estimated as 1.5 m (Rounce et al., 2015).*".

L261: The original study showed seven points. Why the only five data were used? Show the data in Figure 3b.
All data points from the original study by Kayastha et al. (2000) are used to plot the curve for Khumbu Glacier in Fig. 3b. However, rather than attempt to fit a relationship to the entire dataset, the rising limb of the Ostrem curve was excluded to give a simple power-law fit to these data on the falling limb that represent debris thicknesses greater than 0.05 m, as stated in the text. This was done to simplify the calculation of sub-debris ablation as the debris thicknesses on our study glaciers exceed this value. The DDF calculations and associated text including this section have been removed.

L275-: I do not understand why the surface lowering came up first here. Point measurement in this study provides ablation. Then, if emergence velocity can be zero, the ablation equals to surface lowering. Restructure the logic.
The sentence has been edited to remove the reference to surface lowering, as Section 6.4 was removed.

L330: Without description of model structure, the readers are not convinced with "therefore".
The thermal diffusion model is described in detail in the original publications that presented the full energy balance version by Collier et al. (2014, 2015). We had cited these publications where the model is described and outlined the modification that was made from the original version to apply this for our experiments, and feel that this provides a detailed description of the thermal diffusion model without duplicating extensively from Collier et al. (2014).

L357-: Are descriptions for air temperature necessary?
We refer to air temperatures to define the monsoon (ablation) season, and this is an important driver of glacier temperatures so we do consider this description necessary to our study. Furthermore, data describing on-glacier temperatures in this region are scare and as such have an intrinsic value for future studies.

L367: What is implication of the characterization of monsoon? Is this necessary? Sub- dividing of the season too. Pre-/post-monsoon periods are too short comparing with ordinary impression. Is this definition meaningful?

The sub-division of the monsoon does not affect our debris temperature calculations, which either consider the entire summer or the entire monsoon season. However, the pre- and post-monsoon intervals are important to define the duration of this season and are discussed here in context of similar observations from published literature.

L507: Why was the single lapse rate used? Is this reasonable value? How did the authors get this value? Justify the value, and test its variability on the estimated ablation.
The lapse rate quoted here was used by Rounce et al., (2015) to adjust the Pyramid air temperature measurements to their study site at Imja-Lhotse Shar Glacier. As the difference in elevation between these sites is only 15 m the effect of this value on calculated ablation is minimal.

L521: I was confused with this description. How was the thermal conductivity by the Conway's method used? Clarify this discrepancy.
A detailed description of our application on Conway and Rasmussen's heat flux calculation is given in Section 3.4.2 of the original manuscript. To address this comment we replaced the text in Section 5.2 from line 521 with;
"Non-conductive processes or vertical stratification in debris processes are indicated by non-linearity in the mean debris temperature profiles and large residuals around the best fit line of the scatter plot of temperature derivatives used to estimate apparent thermal diffusivity from which effective conductivity is calculated. For each glacier, the site with the least evidence of non-conductive processes was used to calculate *k*, assuming that this single value is representative at the glacier scale; a realistic assumption if the debris lithology and moisture content are consistent within glaciers. Calculated *k* was 0.98 ± 0.10 W m-1 °C-1 for Khumbu Glacier (KH1), 1.43 ± 0.14 W m-1 °C-1 for Ngozumpa Glacier (NG1), and 1.98 ± 0.20 W m-1 °C-1 for Imja-Lhotse Shar Glacier (IM4). These values were used to compute ablation summed over all days of the defined monsoon season at all available sites, according to equation 3 using the site specific debris thickness, and mean daily surface temperature, and assuming that the ice is isothermal at 0°C."

L526: I do not agree with use of "surface lowering" in this context. See comment above (L275)
Here as above, surface lowering has been replaced with ablation.

L535-: I do not understand why the authors did not calculate ablation at IM13, where the observed ablation data is available though only one point data does not guarantee accuracy/plausibility of model output. In addition, I do not understand the meaning of calculation results for debris pits, which did not reach to the debris-ice interface. This does not provide any useful information.
The thermal diffusion model has now been applied to the sites where ablation stakes were installed on Imja-Lhotse Shar Glacier, and results are presented in the new versions of Table 5 and Figure 10.

L641: Only one ablation stake measurement was available in this study. NOT three.
See response to L108 above.

L662: This issue of surface relief is not the sole problem for the DDF method but for the other methods.
This sentence has been removed as the ms no longer focuses on glacier-wide mass balance but only point estimates.

L674: This sentence should be moved to the section for DDF method.
The DDF section and associated text including this sentence have been removed.

L675: I do not understand why the authors did not use this value. IM13 is the sole site where validation is possible.
See response to L535 above.

L704-: I do not agree with the discussion in this section. Both the satellite based surface lowering and modelled emergence velocity have huge uncertainties. Vincent et al. (2016) could have applied this approach

because they obtained super fine UAV based DEM. In addition, I was so confused with values shown since L722. I list all values make me confusing as below. Note that the following values have all unit of m a-1.
Surface lowering of Khumbu: 1.14 (L722) Total loss of ice thickness of Khumbu: 1.59 (L725) Surface lowering of Ngozumpa: 1.21 (L731) Surface lowering 1.21-1.59 (L741)
For Khumbu, if the remotely sensed surface lowering is surely 1.14, the value of 1.59 should be ablation. But, the same value appears later as "surface lowering". Together with the description in L731, I do not understand how the authors understood the relationship among ablation, emergence velocity and surface lowering because surface lowering derived from remotely sensed DEMs does not require the emergence velocity. This discredit also come up around L757.
Mass balance -3.0 Surface lowering 3.9
Why is the surface lowering greater than mass balance in absolute value? I briefly checked the abstract of Vincent et al. (2016) as:
Surface lowering -0.84 Emergence velocity +0.37 Mass balance -1.21
The relationship among these is convincing. I know that the values above are for debris-covered area and the authors wanted to discuss those on "assumed" debris-free surface. My question is how the authors estimated the surface lowering of "debris-free equivalent surface" as 3.9.
These comments all refer to Section 6.4, which has been removed, see response to Major Issue above.

Figure 11: This is problematic. Ostrem curve is obtained experimentally, in which the atmospheric condition can be assumed to be same for different debris thickness. However, the plots in Fig. 11 were estimated under different conditions in different years. Annual variability of meteorological conditions and variability of sites should affect the estimated melting rates. These should not be compared as the Ostrem curve. If do so, some standardization (ratio to debris-free ice, for instance) should be processed.
Figure 11 and associated text will be removed from the revised manuscript.

Minor Comments
L54/55: Ragettli et al. (2015) and McCarthy et al. (2017) are for Langtang region though the sentence started as "In the Everest region".
Citation to Ragettli et al. (2015) removed, and sentence citing McCarthy et al. (2017) moved up to start before "In the Everest region…".

L56: The sentence is odd. "trend of glacier mass loss . . . has reduced glacier volume" L65: It would be better to address what is "effective thickness".
A good point, the sentence has been simplified to remove the reference to glacier volume.

L77: Similar rates of mass loss were pointed out between debris-covered and -free not "glacier" but "area".
"glaciers" replaced with "area"

L79: Gardelle et al. (2013) did not analyze ice-cliff ablation. Immerzeel's paper was published in 2014.
Citation to Gardelle et al. (2013) removed, and Immerzeel et al. (2014) corrected.

L90: Doubled "between"
Deleted

L106: Provide any reference(s) for the heat flux method.
Citation to Conway and Rasmussen (2000) added here.

L133: "the accumulation area" should be replaced by "the ice-fall".
Done

L139: If this is a result of this study, provide method and location where the authors excavated debris pits. Otherwise provide reference.
L140: Is this debris distribution based on this study or others? Provide reference if latter.

These results are from a forthcoming paper; citation added here to Gibson et al. (2018) *Earth Surface Processes and Landforms (accepted)*.

L146: Kayastha et al. (2000) did not conduct their observation in the 1970s but in 1999.
L159-: Provide years.
Apologies for this error, the DDF section and associated text including this sentence have been removed.

L201: What is unit of "0.02"?
"m" added here to indicate meters

L207: Accuracy of site location is used in this study? Removable.
GPS accuracy removed.

L227: Which version of RGI? Area of Ngozumpa is 61.1 km2 in the latest RGI6. Area and altitude information of the glaciers can be discarded. It is much better than providing incorrect information.
All glacier areas and altitudes have been corrected to the values given in RGI v6 as cited in the text.

L279/628/714: Why were different values used for ice density? And what is value of density of Eq. 2?
Density of ice was 900 kg m-3 as stated at L628, this has been added to L279. Text at L714 has been removed with Section 6.4

L296: Provide equation(s) for Conway's method.
Equation 2 from Conway and Rasmussen (2000) has been added to this section as Equation 4.

L323: What is the time interval of calculation? I suppose that the 30 min interval of observation is too long to run the model.
The model was forced with the 30-minute temperature data, apart from in Experiment 3 where these data were resampled to match the hourly timestep of the precipitation data. This is stated in the following paragraph (lines 340–250 in the original version).

L336: What do the authors intend to mean with vapour fluxes? Latent heat at the surface? Vertical vapor transfer in the debris layer? If latter, is it different from moisture consideration? Clarify it.
We removed this sentence, since we state earlier in the paragraph that the surface energy balance terms including the latent heat flux were not simulated, as surface temperature is specified in the model.

L366: What does this "All $p < 0.05$." mean?
This refers to the precision of the correlation coefficients presented in this section, but we agree that this is confusing as stated here and have removed this text.

L472: I do not understand what the authors intended to mean.
The Reviewer refers to this sentence "*In each case, Td at the greatest depth measured was about 4°C cooler than Ts with a slightly greater difference, of about 1°C through thin debris*." in the regional comparison of debris temperatures. This sentence is intended to illustrate the effect of the observed temperature gradient, but as this adds confusion we have removed this sentence and left the stated gradients to stand alone.

L477-: Cite figure.
Citation to Figure 8b added.

L491: Remove the failed site KH3.
Done

L500: Add "of 2016" after "mid-May".
Done

L669: "1-m long stake" is not sufficient information. 1-m varied in ice? 1-m varied from the debris surface? Table 4 suggests the former condition but it should be clarified.
Text added to clarify the use of ablation stakes "Rounce et al. (2015) installed 2-m long plastic stakes were installed to a depth of 1 m below the ice surface and debris was replaced above."

L745: Watson et al. (2017 in GPC) is not appropriate but Watson et al. (2017 in Geomorphology) should be.
This text was part of Section 6.4, which has been removed.

Anonymous Referee #2
Received and published: 12 February 2018

Dear Editor and authors,
Review comments on "Multiannual observations and modelling of seasonal thermal profiles through supraglacial debris in the Central Himalaya" By Rowan et al. Submitted to the Cryosphere Discussion.

Overview and General Comments
This manuscript presents data from three seasons of air temperature, debris temperature observations from the Khumbu glacier in order to compare with data from three other glaciers in the Khumbu region. They also compare three models of sub-debris melt. Is is quite an ambitious paper as it engages spatial and temporal variability of data from multiple glaciers, while also comparing modelling methods. I see this manuscript as attempting to answer:
1) Which melt modelling approach is best for estimating sub-debris melt across debris- covered glaciers? 2) How does the monsoon effect sub-debris melt? 3) How do debris temperature profiles vary between glaciers in the Khumbu region? 4) What are the debris thicknesses, air temperatures, debris temperatures on the Khumbu glacier? 5) What is the contribution of sub-debris melt to glacier-wide mass balance on the Khumbu glacier?
Each of these questions are valuable and important on their own right but by attempting to address all of these questions the readability and continuity of the paper suffers. This occurs to the point where it is difficult to assess the validity of the science.
More data and result synthesis is needed. It is hard to see how certain reported numbers are relevant for the broader paper. By removing details that are not relevant to the question being addressed, the paper will be more easy to follow. The spatial, temporal, and methodological focus changes frequently (from sentence to sentence). Often the data and results presented are for a specific site, season, duration, and glacier which makes it hard for me to connect the reported information to another site in a different season, duration, etc. (section 4.3 as an example).
As it stands now the conclusions are over stated: they lack context, support and discussion. I suggest that the authors limit the scope of the study or break the manuscript into multiple papers. With a narrowed scope the authors will be able to address their questions more completely. Some of the sentences contain extra words and can be contracted for clarity.
We have revised the manuscript to address the Major Issue raised by Reviewer #1 (see response to this point above) and reduced the scope of the study. We do not attempt to ascertain how the monsoon affects melt by comparing our results to glaciers that are not monsoon influenced, but do consider seasonal thermal profiles rather than data collected over a much shorter period, which is where the novelty of our results lie.

Specific comments:
L101: Perhaps you can mention how many locations you measured at here?
"a number of" replaced with "five"

L111: I suggest that Section 2 be re-labeled as Background. There is a lot of good data reporting in this section but I find that it has not been synthesized in a way that makes it clear why it is important to know these numbers.
Done

L133: The accumulation area surface by definition should have snow at the end of the year. Here you note: "clean ice of the accumulation area." Consider rephrasing this sentence.
Replaced with "base of the icefall"

L149-150: I suggest changing from 'ablation' to 'ablation rate' here because ablation refers to the total length of water equivalent loss. See Glossary of glacier mass balance.
Done

L160-168: It seems this comparison would fit better in a discussion section. It is not clear how it is relevant.
The comparison between debris DDFs measured at Khumbu Glacier and those from debris-covered glaciers elsewhere is included in the Background section to provide context to the relatively high values for Khumbu.

L171: This section also describes the calculation and modeling methods. Please add them to this introductory sentence.
Added "The two methods used to calculate ablation are also described."

L210: the title of this section: "3.3 Comparison with Ngozumpa and Imja-Lhotse Shar Glaciers" does not reflect what is described in it. You do not mention the comparison between the Khumbu, Ngozumpa, and Imja-Lhotse Shar Glaciers rather you describe the Ngozumpa and Imja-Lhotse Shar Glaciers and the data collected from them. Perhaps you can title it "Ngozumpa and Imja-Lhotse Shar Glacier Data"
Subheading changed to "Data collected at Ngozumpa and Imja-Lhotse Shar Glaciers"

L266-268: These are results and I suggest you move them to that section.
The DDFs presented here are not new results for this study but instead are given by the relationship described in Eq. 1 derived from the data presented by Kayastha et al. 2000 and were included here in the text to illustrate the range of values resulting from this calculation. The DDF section and associated text including this sentence have been removed.

L272-318: Section titles: "3.4.2 Downward heat flux vs. 3.4.3 Thermal diffusion model"
The titles are a bit confusing as the "Thermal diffusion model" also incorporates a downward heat flux. Just as the "Downward heat flux approach" is also a "thermal diffusion model." Consider re-naming these two models so there is a clear distinction between them. Analytical and numerical diffusion model maybe?
We chose not to rename these calculations as we consider that they are suitably described in the text where they are introduced here and the terms are appropriate and used consistently throughout the ms.

L368: "4.1 Timing and character of the monsoon" This section needs to be foreshadowed in the introduction and background sections. It is a bit of a surprise to read about precipitation and the monsoon in the results as there is no significant mention of the monsoon or what the authors did with regards to precipitation in the methods section. It is hard to tell how the observations presented are relevant to the study as they are mentioned here with little context.
The data presented in Section 4.1 are introduced in the Methods in Section 3.1 where the data collection is described.

L396: "4.2 Air temperatures" It is not clear why these ranges of air temperatures need to be presented. It could be better to synthesize this in a form that makes it clear how this information connects to the broader paper.
Section 4.2 presents our measurements of on-glacier air temperatures, and the heading has been updated to clarify this. See response to comment on L357 by Reviewer #1 above.

L568: This portion of the discussion section reads more as a methods or results section and I am not sure why it is being discussed here.
Section 6.1 discusses the limitations of our methods for measurement of on-glacier air and debris temperatures and some potential uncertainties in the results that occur because of these limitations, in a similar manner to

Section 6.3.2–6.3.4. This section has been renamed to clarify the purpose of the content "*Limitations of temperature measurements made at Khumbu Glacier*".

L665: This section also mixes results with discussion.
The comparison between calculated ablation and stake measurements has been moved to the Results.

L704: While this is an interesting topic to address I am not sure that context has been provided for this to be discussed in detail here. Are the ablation rates presented here discussed in the methods and results sections? Was ablation rate estimated across the debris-covered area of the Khumbu glacier for all debris thicknesses? Over what period were the modeled ablation rates estimated for 3 years only? Do they cover the full period of the geodetic surface change estimate? All these questions and more need to be addressed to determine the contribution of sub-debris ablation to glacier- wide mass balance.
L769: The lack of a comparison between the melt modeling estimates and measured ablation hampers the usefulness of the modeling exercise. Without a comparison to actual melt from each site I am not sure what to take away from the modeling portion of the paper.
For these two comments, see response to Reviewer #1 Major Issue, above.