The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-239-RC1, 2018

Interactive comment on "Multiannual observations and modelling of seasonal thermal profiles through supraglacial debris in the Central Himalaya" *by* Ann V. Rowan et al.

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 abla-

Printer-friendly version

Discussion paper

tion, 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.

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.

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.

L94-: Did this study find any method to calculate spatial distribution of sub-debris ablation? I do not think so.

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.

L158-: The paragraph looks a part of discussion. It is unclear how this description relates to this study.

L176: Pay respect to the original studies for the Pyramid Observatory data.

L249: Debris pit IM4 did not reach to debris-ice interface so that use of term "debris thickness" is misleading.

L261: The original study showed seven points. Why the only five data were used? Show the data in Figure 3b.

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.

L330: Without description of model structure, the readers are not convinced with "therefore".

L357-: Are descriptions for air temperature necessary?

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

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.

L521: I was confused with this description. How was the thermal conductivity by the Conway's method used? Clarify this discrepancy.

L526: I do not agree with use of "surface lowering" in this context. See comment above (L275)

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.

L641: Only one ablation stake measurement was available in this study. NOT three.

L662: This issue of surface relief is not the sole problem for the DDF method but for the other methods.

L674: This sentence should be moved to the section for DDF method.

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

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.

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.

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".

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".

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

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

L90: Doubled "between"

L106: Provide any reference(s) for the heat flux method.

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

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.

L146: Kayastha et al. (2000) did not conduct their observation in the 1970s but in 1999.

L159-: Provide years.

L201: What is unit of "0.02"?

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

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.

L279/628/714: Why were different values used for ice density? And what is value of density of Eq. 2?

L296: Provide equation(s) for Conway's method.

L323: What is the time interval of calculation? I suppose that the 30 min interval of observation is too long to run the model.

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.

L366: What does this "All $p < 0.05$." mean?

L472: I do not understand what the authors intended to mean.

L477-: Cite figure.

L491: Remove the failed site KH3.

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

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.

L745: Watson et al. (2017 in GPC) is not appropriate but Watson et al. (2017 in Geomorphology) should be.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-239, 2017.