

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 #2

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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.

Specific comments:

L101: Perhaps you can mention how many locations you measured at here?

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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.

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.

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.

L160-168: It seems this comparison would fit better in a discussion section. It is not clear how it is relevant.

L171: This section also describes the calculation and modeling methods. Please add them to this introductory sentence.

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”

L266-268: These are results and I suggest you move them to that section.

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?

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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.

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.

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.

L665: This section also mixes results with discussion.

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.

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

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