

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-81

Anonymous Referee #2

Referee comment on "Sub-seasonal variability of supraglacial ice cliff melt rates and associated processes from time-lapse photogrammetry" by Marin Kneib et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-81-RC2>, 2022

This submission presents measured and modelled melt from four ice cliffs located on two Himalayan glaciers with contrasting geomorphological characteristics. Measured melt is derived from terrestrial timelapse photogrammetry and modelled melt is derived from a surface energy balance model partly developed by the co-authors. The methods are extensively described and largely based on previous work and are sound. There is careful attention given to quantifying uncertainty and I can find no fault in that regard. The results show some interesting spatial and temporal variability in melt patterns and provide a window into the dynamics of the ablation season, which is otherwise all too often obscured by cloud and inaccessible for field observations. Interpretation of the key controls of this variability is convincing.

It is notable that the premise of the work is to be able to quantify and characterise the evolution of these ice cliffs during their most dynamic period, yet the modelling that is to shed insight into the energy balance is static. There is one line of justification for this (373-374) but it could do with much better justification, especially given the most recently published work of the co-authors describes and uses the dynamic model in a similar vein and some of the future work suggested in later sections has already been realised. There is also a great deal of attention given to the important role that albedo plays in controlling melt, and the fact that it is dealt with as a constant in the modelling, but on the other hand the timelapse images are normalised to account for illumination variability and cliff brightness is derived. It strikes me it would be a small step to use these data more explicitly to drive a time-varying albedo parameterisation that would bring the modelled and measured data more closely together. I don't suggest that the authors should implement that as a revision, but maybe the concept (and the challenges that might lie therein) could be included within the discussion of future work. Lastly, the manuscript is very lengthy, and could be streamlined in places. An example is in the description of the SfM setup, which is based on previous published work - a short summary of the departures from this previous work rather than extensive description should suffice. Lines 70-99 could be distilled into a few lines. Lines 618-625 seem to offer little other than some generic thought. A re-read of the manuscript with a critical eye for what is and what is not required, and whether any material (methods mostly) could go into Supplementary, would help to keep the reader's attention and lead them to the take-home message more

efficiently.

The manuscript is otherwise very well written. I have picked up some small ambiguities or points for clarification that follow here:

line20: is that horizontal or vertical uncertainty?

line28: variability across space or through time? On an individual cliff, or between sites?

line40: I prefer not to suggest that ice cliffs enhance melt - rather it's the debris that is suppressing it (unless thin of course). Maybe exceed is a better choice of word.

line43: what is an 'advanced' energy balance model in this context?

line77: are ponds a process? Maybe pond filling and drainage? Similarly for streams. Maybe you mean down-cutting?

line121: directions of the compass don't need capitalising

line138: add 'satellite' for those not familiar with Pleiades

line164-165: this is presumably important for the modelling? Maybe state that if so?

line190: 'identified in the June flight'

line223: 'As an initial estimate, we used the values provided by...'

line225: didn't change by more than five centimetres in which direction? Not sure I follow. Do you mean five degrees?

line428 and elsewhere: why the need to put text in bold? The aim being in bold made sense (perhaps) but not the rest...

Table 1: caption needs attention (repeats that from the figure directly above)

Figure 8: clarify which period the cliff outline relates to (start or end of observation period)?

Figure 16: this is nicely presented, but the integration of it into the text is poor. It also represents one possible pathway of evolution over a discrete (set) period of a month, showing two points in time. This doesn't fit well with the rest of the study that tells the reader there is great spatial and temporal variability in behaviour, and it has been characterised at fine temporal resolution for the first time. The figure either needs better explanation in the text, revising (to really show the new information gleaned from this study), or removing.