

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-81

Anonymous Referee #1

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-RC1>, 2022

In this study, the authors monitored the evolution of parts of the debris covered tongue of two glaciers (Langtang Glacier and 24K Glacier) during the monsoon season of the year 2019. They developed a very innovative setup to track the 3D surface changes of the surface at a weekly resolution. They use these weekly digital elevation models (DEMs), and some additional knowledge on the ice dynamics, to calculate the melt occurring from ice cliffs at very high spatial and temporal resolution. They compare their observed cliff melt rates to model simulations to decipher the controls on ice cliff melt and evolution.

I commend the authors for the impressive amount of work behind this manuscript. I think it is a very good addition to the literature, but I have some semi-major comments, and a number of specific ones. Addressing them might help clarifying some aspects of the manuscript.

Major comments:

The uncertainty assessment (section 3.5) is generally done in a careful way. They authors did their best to evaluate honestly the uncertainty associated with their newly developed technique. However, I have some concerns with equation 4, which basically assumes that the errors are uncorrelated (independent) for each pixel. In this study, the errors should be largely correlated because they can originate from e.g. a non-perfect adjustment of the camera, or from the ice flow correction. I therefore suggest to revise this equation, and the text afterwards (L344-346). It is also not so clear, whether these uncertainties are re-used after in the text and figures, because for the figures, they refer to the standard deviation, and not to the uncertainty analysis.

My second major comment is about the model used in the study. I don't fully understand why the authors used the static version of the model instead of the dynamic one. The

dynamic model would be a great way to assess the share of each process (surface energy balance vs. 'geomorphic' processes). As it stands the manuscript is a bit frustrating, because the description of processes related to the redistribution of debris remains extremely descriptive. I am also curious about the model calibration, if there is any, because no details are provided about it.

My third comment is about some sentences in the discussion and conclusion that I find slightly misleading, or not well supported by the data. For instance, in L253-254, L400-403 and L631-633, it is written that time and space integrated methods lead to an underestimation of melt rates of 50%. This is not really correct, it is just that the methods are looking at measuring different targets: previous methods measured integrated losses that include reburial and expansion, and thus also melt beneath debris, while here the authors focus on shorter time scale and on melt rates along cliff transects. The same comment applies for the comparison between the melt estimates calculated for the ice cliffs vs. the sub-debris melt (e.g. L23-25 and L394-399). The calculation of this ratio also lacks details, because little is said about the uncertainties and the representativeness of each end-member.

I found the paper slightly lengthy in some places. For instance, the introduction could be more concise, or the line 361-370 about the surface energy balance model are not extremely useful. While this is not a major issue, it would streamline the manuscript to check for each sentence whether it is relevant for the general paper scope. Similarly, the results section 4.2 that presents each cliff's evolution is difficult to follow. The authors do not link very well the morphologic and meteorological changes happening to changes in the melt rates, and instead follow a more descriptive approach.

Specific comments:

In some places, there are some sentences in bold font. They should be in normal font.

L18: "Tibet" -> should be "China" for consistency with "Nepal", no?

L20-21: the uncertainty is given at pixel of cliff scale?

L27: I didn't find elements in the text that supports this statement in a quantitative way.

L36: it's the ice and not the "ice cliff" that is directly exposed. The sentence should be re-phrased.

L45: consider adding "for specific locations".

L115: which part of the glacier section is included in the calculation of slope?

L130: is it relevant to quote the price in a scientific publication? I suspect anyone interested by the setup will contact the authors.

L191: maybe a slightly more quantitative assessment of the quality of the co-registration would be good

L193-195: the mean elevation difference on stable ground is very large (larger than the absolute value of the emergence for 24K!), and would imply a potential systematic bias. Is the median at zero? The authors should consider putting the mean or median off-glacier elevation at zero.

L292-293: why not calculating all the uncertainties within the normal intersection framework?

L330: how does the 2° uncertainty in the slope angle translates into the sigma_flow? Also the units of the different components of sigma_flow are never explicitly written.

L413: missing figure number

L423-432: what is the impact on the melt rate?

L539-554: here I would expect a quantitative analysis, which remains too descriptive

L571: a bit in contradiction with L571

L603-605: then why not testing the dynamical parametrization of the model?

Section 5.3: it would be worth acknowledging the limitations of the study (very small area

surveyed, only one full ablation season, north facing cliffs only...)

L627: "bridged a crucial gap" -> this sounds a bit like overselling the study

L627-648: the conclusion is mostly based on very general sentences, and does not do a good job in highlighting the specific findings of the study. It should be re-written to better highlight the novel aspects of this study. I would recommend not to use too many bullet points in the conclusion.

Fig. 7 a and b: if I read the caption well, it says that the shading area represents the standard deviation, but what standard deviation? The spatial one? Why not using the uncertainties calculated from the uncertainty analysis for the observed melt rates?

Fig. 7 c and d: there is a unit problem for the energy fluxes. They are not the right order of magnitude.

Fig. 8: is there an influence from the large boulder on top of the cliff edge? It is never mentioned in the text I think?

Fig. 9 and all the following ones with the same design:

- I find it very confusing to represent the cumulative precipitation for overlapping periods, it suggests a huge amount of precipitation, because most periods are counted twice. I suggest expressing the precipitation in mm/day instead.
- On the panel d: at which time resolution are the fluxes plotted? When two model runs overlap, which one is used for the flux plot? Also fluxes should be in $W m^{-2}$ and not melt contribution, because they can be negative.