Dear Dr. Cook,

Thank you for your comments on our manuscript. We appreciate your constructive and thoughtful review. The line-by-line comments are very helpful and will be incorporated into the revised manuscript. I'll respond to the major comments below, with the reviewer's comment shown as italic and our response as normal font.

L46 – I'm not sure you can quite say that glacial erosion varies as a function of basal thermal regime because Koppes et al. (2015) used mean annual air temperature rather than basal temperature.

We agree. Glacial erosion varies as a function of the basal thermal regime is not precisely the conclusion of Koppes et al. (2015), but their results do imply this point. We will reword this sentence in the revised manuscript.

L66 – I didn't really understand this sentence. It starts off being about polythermal glaciers, but ends in making the same point you have made several times already about thermal regime needing more study.

Sorry for the confusion. The point of this sentence is that previous work on the impact of thermal regimes on glacial erosion mainly focuses on comparing the thermal regimes between different glaciers, rather than comparing the different portions within a polythermal glacier. For example, people have been classified glaciers as cold vs warm glaciers and have suggested that cold glaciers could protect mountains while warm glaciers destruct mountains. However, polythermal glaciers are more common than purely cold or warm glaciers. Therefore, we need to pay more attention to the variations of thermal regimes within a glacier/icecap. We will rephrase this sentence in the revised version.

L105-117 – This section discusses the glacial erosion rule employed in the modelling effort; the authors use a linear erosion rule and justify that choice with reference to previous studies that also assume erosion rate to be a linear function of sliding velocity. Nonetheless, several papers have been published since those cited here that suggest that the sliding velocity be raised to some exponent (l) which could be <1 (Cook et al., 2020), ~2 (Herman et al., 2015), or >2 (between ~2.3-2.6; Koppes et al., 2015). I wonder if this should be mentioned in the

manuscript. I don't think there is any problem with the approach used by the authors, but the justification of the erosion rule used seems one-sided. Cook et al. (2020) suggested that an exponent of 2 would be suitable for single glaciers and an exponent of 1 or less would be appropriate for ice caps/sheets & mountain ranges comprising multiple glaciers – so their work potentially supports your choice of erosion rule formulation.

Thanks for the comment. This is a good point. We chose this value because our aim is to investigate the spatial pattern of glacial erosion and different values of the exponent have little impact on this spatial pattern. We will provide more references for different choices for the value of the exponent in the revised manuscript.

L123 – do you need to justify (e.g. using citations) why you have selected these values for the constants in the fluvial incision model? There was a lot of justification for the use of a linear glacial erosion rule, but the same detail is not here for the fluvial model.

Thanks for the suggestion and we do need to justify this. We will add a short discussion about our choice of model and model parameters in the revised manuscript.

L327-8 – it's probably a bit self-centred to suggest it, but Cook et al. (2020) provided direct empirical evidence from modern erosion rates and precipitation rates for the influence of precipitation on erosion. We even found that precipitation explained more of the variability in the erosion rate data than did temperature. I wonder if this could/should be mentioned here in your Discussion – certainly, it supports the point you are making here.

Thanks for the suggestion. We did read your work but we must have missed this point you mentioned. We will add this in the revised manuscript.

Fig 9 – I might be misinterpreting (or over-interpreting?) this diagram, but it seems to me that there is a systematic increase in erosion rate with increasing precipitation; there is not the same systematic increase in erosion rate with increasing temperature. We (Cook et al., 2020) found the same relationship (our Figure 3b and 3c). Perhaps this provides empirical support for your results.

Your interpretation is correct. However, it is important to notice that the climate range we explored here is much smaller than your work, especially the temperature range. This is why we did not make any direct comparison with your Fig 3 (and Fig 4) in this manuscript. We've cited your work in the discussion section and we decided not to oversell our results in Fig 9.

Best,
Jingtao Lai