Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2021-26

Simon Cook (Referee)

Referee comment on "Climatic controls on mountain glacier basal thermal regimes dictate spatial patterns of glacial erosion" by Jingtao Lai and Alison M. Anders, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-26-RC1, 2021

Thank you for the opportunity to review this manuscript. I very much enjoyed reading it, and I think it provides some food for thought (and hence will be of interest to the glacial geomorphology community), particularly around the implications for the often-suggested relationship between ELA position, maximum glacial erosion rates, and mountain peak elevations (an important component of the glacial 'buzzsaw' hypothesis). Specifically, the authors argue that cirque floor elevations might represent average glacial conditions over a glacial cycle(s) rather than an ELA position related to climate.

I cannot comment authoritatively on the use of the PISM ice sheet model, but the modelling framework seems (in my humble opinion) to be robust, and indeed is grounded in previous peer-reviewed work by the same authors; the results are interesting and clearly presented. The manuscript is well-written. Consequently, most of my comments represent minor alterations and suggestions.

I have been asked to comment specifically on the following items:
1. Does the paper address relevant scientific questions within the scope of ESurf? YES
2. Does the paper present novel concepts, ideas, tools, or data? YES
3. Are substantial conclusions reached? YES
4. Are the scientific methods and assumptions valid and clearly outlined? YES
5. Are the results sufficient to support the interpretations and conclusions? YES
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? YES
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES
8. Does the title clearly reflect the contents of the paper? YES
9. Does the abstract provide a concise and complete summary? YES
10. Is the overall presentation well structured and clear? YES
11. Is the language fluent and precise? YES
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced,

combined, or eliminated? NO
14. Are the number and quality of references appropriate? YES
15. Is the amount and quality of supplementary material appropriate? N/A

L35 – "…than cold-based glaciers"
L36 – need comma before 'consequently'
L44- "frozen-based" seems an odd term to use. Cold-based?
L45 – ELA (singular)
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.
L59 – suggest you re-word this to "will tend to create a cold basal layer that is frozen to the bedrock"
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.
L94 – works
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.
L115 – unrealistically
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.
L141 – range has
L147 – delete "is"
L229 – covered
L303 – not recorded in cirques
L313 – to climate
L321 – cold, high-latitude
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.
L344 – which imply
L355 – suggest you change 'On the contrary' to 'By Contrast'
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.