Review for 'Valley longitudinal profiles record the fluvial landscape evolution and geological structure of the Gamburtsev Subglacial Mountains, East Antarctica' by Guy J. G. Paxman, Fiona J. Clubb, Stewart S. R. Jamieson, and Alexander L. Densmore.

This manuscript addresses the geology, geomorphology, and uplift and erosion history of the Gamburtsev Subglacial Mountains (GSM), located under the East Antarctic Ice Sheet (EAIS). This is an ambitious and multidisciplinary study that utilizes a combination of geophysical datasets and quantitative geomorphology approaches to shed light on the history of a poorly understood landscape. Under the assumption that little erosion has occurred on the GSM since the onset of EAIS glaciation nearly 34 Ma, and thus that the landscape preserved is largely fluvial, the authors perform a detailed Khi analysis issued from quantitative fluvial geomorphology techniques to interrogate the coupled erosion-uplift history, identify and investigate possible geological boundaries (lithology, tectonics), and test hypothesis regarding the age of formation of the mountain range.

We certainly appreciated the breadth and depth of the manuscript and we would like to commend the authors on the interdisciplinary approach they applied to a difficult question. Hence, we recommend publication of the work, following a series of revisions that I note below (see also attached document with annotated figure).

We hope that our comments help the authors improve this interesting manuscript, and we invite them to contact us with any queries.

Kindly,

Anna Grau Galofre and Evan Blanc (PhD student)

Major points

We raise three major points we would like to see prior to moving to publication of this work.

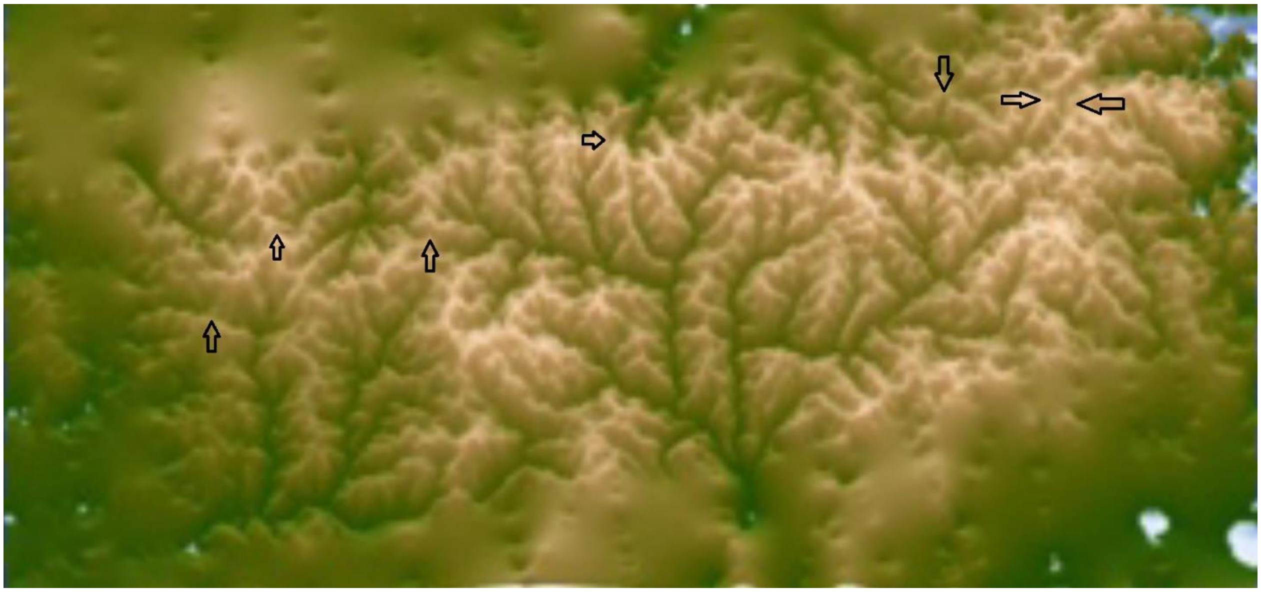
Erosion rates during and after the onset of Eocene glaciation

The authors present and defend the assumption that erosion rates during the built up of the east Antarctic ice sheet were low enough as to not have produced a noticeable imprint into the pre-glacial landscape at the resolution observed. This assumes that glaciation was cold-based right from the onset, and everywhere in the GSM, which I have a hard time believing (and which is later questioned in section 5.1). Is there any supporting evidence to show that erosion rates shut down during the early stages of glaciation, such as sediment records dated from that time? Is there equivalent evidence from elsewhere that at the onset of glaciation erosion rates fall to near zero? Permanent, cold-based glaciation is without considering that punctuate warming events, such as the Late Oligocene Oi2b warming event (27 My BP), which lead to Antarctic wide warming and perhaps ice retreat (Duncan et al., 2022) could have triggered warm-based glacial erosion in the region. Indeed, Creyts et al., 2014, cited in the paper, states the following regarding the mountain range:

"Steep-sided valley walls, lower cirque levels, and overdeepenings along valley floors demonstrate that the entire range has been extensively modified by alpine-style glacial erosion [Rose et al., 2013]. The large-scale hypsometry with a maximum at high elevation and a distribution that tapers from this maximum reflects an alpine morphology and is a characteristic signature of glacier erosion (Figure 1c) [Brozović et al., 1997; Egholm et al., 2009]."

The second assumption I struggle with connects with the first, and it is the lack of discussion as to the effect of glacial erosion in profile morphometry, including concavity, Khi analysis, and knickpoint presence. Glacial erosion also leads to profile concavity (e.g., Headley et al., 2012, Deal and Prasicek, 2020), and hence the glacial overprint of a pre-glacial, dominantly fluvial network, can be expected to take concave-up shapes overall, with the presence of knickpoints and changes in curvature (notably overdeepened sections, as the authors very well point out in section 5.1) moving from glacially-dominated to fluvially-dominated regions (Herman and Braun, 2008, Deal and Prasicek, 2020). This comparison points between rivers in unglaciated and glaciated catchments should be carefully and systematically presented (see next major comment).
Because of the mathematical similarity between the fluvial (SPIM) and glacial sliding (SIIM) equations, it is possible that the χ-driven approach could also lead to meaningful results in a fluvial valley with glacial overprint. Other processes, notably subglacial hydrology and perhaps also cold-based glacial erosion, may have also played a role on the landscape evolution since the Eocene, and should be briefly introduced.

Line 140 – The D8 algorithm's assumption that water is always routed downslope breaks when considering possible subglacial meltwater contributions, which likely were possible in the region (Cretys et al., 2014, Young et al., 2025). I have noted a few cross-divide valleys from the radar data connecting catchments 9-2, 2-1, 10-10, 9-3, 5-6. 5-N unnumbered network (figure included in the attachment). If present, subglacial drainage and sediment erosion contributions would have played a role in the erosion, transport, storage, and deposition in the region.



GSM DEM data, extracted from figure 1b in the paper. Arrows point at valleys that seemingly cross drainage divides.

Line 195 – a good place to discuss that glacial and subglacial erosion may introduce additional signatures in the Khi plot

Lines 312-314: the authors state: "*We also note that not all longitudinal profiles decrease monotonically in elevation moving downslope; several valleys exhibit local minima (i.e., enclosed lows), which often coincide with tributary junctions (e.g., basins 5, 7, and 9), and, less commonly, isolated maxima (e.g., basins 6 and 8).*"

Undulating longitudinal profiles could be a sign of subglacial meltwater drainage utilization of these former river networks, particularly if these systems are consistently oriented towards a possible former ice flow direction (Grau Galofre et al., 2018). This is once again a sign that care should be taken interpreting these profiles solely under a fluvial erosion perspective.

Line 395-397: Knickpoints could also result from glacial action (see e.g., Herman and Braun, 2008 or Deal and Pracisek, 2020).

First paragraph of section 5.2. All the observations presented by the authors show that the results are consistent with fluvial erosion dominating the landscape signature of these networks. But none in this paragraph the authors show that these results are inconsistent with glacial overprint. For example, it would be useful to contrast the authors' claim with the range of concavities consistent with catchments where glacial erosion has operated. How do Khi plots compare? I would like to see a more systematic presentation of what to expect in rivers with partially glaciated catchments (see also next major comment).

Line 501 – The authors finally admit that the morphology of certain profiles is consistent with glacial erosion above the 1500 m elevation mark, which would have consisted on a former ELA at the location of basin 8. An ELA at 1500 m in basin 8 would have also affected the basins that directly share a drainage divide with it, particularly basin 9 (which routes into the same main stem), and perhaps also basins 7, 5, 4, and the higher elevation basin 2. There are a large number of tributary valleys above the 1500 m mark in figure 4.

On the light of these points, I request the authors revise their 1st conclusion to allow for more flexibility in the glacial fingerprint of the longitudinal profiles (which they themselves agree existed in section 5.1).

Clearer structure, consistent message

There are mixed messages throughout the paper, notably regarding the role of fluvial vs. glacial erosion. The predominance of near steady-state fluvial landscape signatures is established as almost certain throughout methods and results. Coming up to the discussion (section 5.1), where this near certainty is challenged and the authors present a welcomed discussion of this assumption in the light of their results. This structure as is, however, makes it very hard to follow the logic thread of the paper and poses conflicting messages.

I suggest the authors deliver a consistent message by first judging which river profiles are most likely to be dominated by near steady-state fluvial signatures, to then focus their analysis on these – thus avoiding going back and forth with the role of glacial action on the GSM. In particular, I suggest the authors focus their fluvial SPIM model on basin 10 (which they already do!) but justify their choice by stating that other basins were likely affected by glaciation. To this purpose, I suggest the following changes:

1- Introduce a formal set of guidelines targeting the aspects in a longitudinal profile that support fluvial or glacial action in the methods section before discussing the Khi analysis in section 3.2.
2- Resolve whether the profiles in the different basins are consistent with fluvial or/and glacial action after describing longitudinal profile morphology in section 4.1. Use the qualitative guidelines introduced in the methods section to make a choice of the profiles where application of SPIM model is justified because glacial action is minimized. Use this as a justification to focus the analysis on basin 10 as exists in the paper.
3- Rework section 5.1 by incorporating the guidelines for the recognition of fluvial or glacial profile erosion in the methods and results text. Focus instead on discussing the evidence for pre-Eocene alpine glaciation and the possible paleo ELA at 1500 m. That is an important, and currently obscured result of this paper.
4- If pre-Eocene alpine glaciation existed over 1500 m, then one can expect spatially variable loading and perhaps heterogeneous uplift patterns. This is a point to raise in section 5.2.

SPIM and Khi analyses can be greatly summarized to condense the paper – these are well-known tools to the fluvial geomorphology community.

Assumptions and uncertainty

Uncertainty

The uncertainty estimation for the topographic dataset derived from the different radar platforms is vague as to what horizontal and vertical resolutions they recover once they integrate the whole suit of radar data. They state in line 98-99 that this resolution (a distance of 15-30 m between points along-track and 10 m vertical) is enough for the scale of the investigation in this study (>100s of meters) – but both resolutions, namely what is obtained from data (line 302 – 87% below 10 km, 98% below 20 km) and what is required for this study (assuming 1km since this is the length step of the model – line 258) should be made explicit here, and not have them spread between methods and results. The discussion of whether the resolution is enough or not for the evaluation of their diverse claims should be systematically made in each relevant section.

Line 293-294 – the authors state that "*Tributaries are largely concave-up and smoothly and systematically join the trunk valley in a downslope direction without 'hanging' above the trunk*". But I don't believe the resolution of the study is sufficient to make such claim. Two uncertainties combine in figure 4: that of each individual point, and more importantly, the spacing between individual points. The second uncertainty does not allow us to judge whether the tributaries are graded or hanging. For example, in figure 4, basin 3, the terminal elevation point of the tributary is some 50-100 m above the upslope elevation point of the main stem (trunk), and 100-200 m above the downslope point of the main stem. With the information available to us, this could very well be a valley hanging 100 m over the main stem. Similar cases occur in figure 4, basins 4, 5, 8, and 9 (can't evaluate 10).

A similar concern arises from the methodology presented in section 3.4. The authors should quantify what is the uncertainty that arises from their combined reconstruction of the subglacial depocenters and subglacial undulations so that we can evaluate whether it is sufficient or not for their interpretations in the discussion.

Assumptions

The paper presents us with many assumptions, some interdependent, which makes it hard to evaluate the robustness and generality of the results presented. While some of these assumptions are justified (i.e., the contribution to erosion of the cold-based EAIS is indeed likely very small), and others are given sufficient consideration (i.e., the range of erosion rates calculated from detrital thermochronology is expanded by a factor 10, a range of m and n values is considered for a given m/n), some others are entirely dismissed (assumption that fluvial erosion is the sole contribution to longitudinal profile erosion, assumption that changes in the effective coefficient of erosion are controlled only by rock strength regional variations, assumption that none of the tributaries are hanging over the main stem, assumption that uplift rates are spatially homogenous, etc.). The authors should highlight each of these assumptions and later discuss how relaxing them would affect their interpretation.

Minor comments

Line 259 – The authors state that the initial topography was set to the furthest downslope point in the profile – the base level? I am not sure this is correct, or that I understood correctly.

Line 261 – The authors state that "*We set the total uplift to match the elevation of the highest point on the longitudinal profile*" but uplift rate cannot equal an elevation. Clarify.

Line 357 – Why can't slopes be compared directly? They are a non-dimensional property (H/L), and is better defined that drainage areas in this context. Slope uncertainty scales like Delta (S) ~ S/L Delta (L) – Delta(H)/L, where H and L are elevation and length scales, and Delta (H) and Delta (L) their associated uncertainties. Drainage area is a dimensional property (cannot be directly compared), and its uncertainty scales as:
Delta(A) ~ h L^(h-1) Delta (L) (assuming A~L^h, where h is Hack's exponent)

Line 384 – 386 The authors state that "*The computed K values were 1.3 or 13 x 10-8 m-0.2 / yr for segment 2 and 385 0.52 or 5.2 x 10-8 m-0.2 / yr for segment 3. When adjusted for covariance with m, these K estimates are in close agreement with the range derived for granitoids and metasedimentary rocks (Stock and Montgomery, 1999)*," however, in the SPIM model, the effective coefficient of erosion K depends on a suite of parameters in addition to the rock quality, including the sediment flux characteristics, the hydraulic geometry of the flow, the basin geometry scaling, etc. (Whipple and Tucker, 1999).

On this same sentence – could the authors elaborate as to whether their interpretation of rock type(s) is consistent with the magnetic signature?

Lines 447-448: This may have been consistent with base level for this network at the time of fluvial incision, I would argue that nowadays this is probably not the case as the ice thickness increases in that direction. If there was subglacial drainage in this network since the time of glacial cover, it may have drained away from this basin.

Lines 599 and 600 state that erosion rates on the order of 10 m/Myr are highly unlikely given what is said in section 3.3. (lines 234-237). However, I am not convinced by the arguments in this section, as I find they to hinge on their hypothesis that the GSM had a regime comparable to purely fluvial systems, which is later questioned in section 5.1. Therefore I do not think the authors can truly dismiss this hypothesis, and I think they should relax the wording of this statement.

We hope that these comments help to improve this creative and interesting study, and we welcome any questions or comments the authors may have.

Kindly,

Anna Grau Galofre and Evan Blanc (PhD student)