Valley longitudinal profiles record the fluvial landscape evolution and geological structure of the Gamburtsev Subglacial Mountains, East Antarctica

Response to reviewers

We thank the reviewers for their constructive comments on our manuscript. Below, we list our responses to the points raised by the reviewers, emphasising the intended changes to the manuscript that we hope will satisfactorily address the comments. Comments made by the reviewers are in *black italics*, and our responses are in blue.

Reviewer 1

The manuscript presents a novel adaptation of existing methodologies. Little is known of the geologic and tectonic history of Antarctica for large swaths. The authors attempt to address this gap for the Gamburtsev Subglacial Mountains under the East Antarctic Ice Sheet. They apply theoretically and empirically well-founded approaches from fluvial geomorphology to recent sub-ice topographic data.

Their approach is rigorous and well-considered and sufficiently documented. Through careful data handling, they have reduced potential spatial biases as far as can be expected until higher resolution topographic data become available. An important result of the study is the implication of preserved foreland basin sediments in the South Pole Basin, which would hold an archive of the geology and tectonic history of this section of Antarctica. My only concern with regards to the approach is the authors' choice of denudation rate ranges for the pre-glaciation mountain range. Their modelled range is significantly lower than the rates measured in modern mountain ranges, with the implication that the response times reported here are too long. This could be easily remedied by running the stream power incision model for an expanded range of erosion. I suspect that, other than timing, the overall conclusions would not change appreciably.

The manuscript itself is well-written, and the figures are well-drafted. The supplemental data are complete and well-presented.

We thank the reviewer for their supportive comments and useful suggestions regarding the choice of denudation rates, which we address in the response to the specific comments below.

Specific Comments:

The authors clearly present their assumptions. However, one assumption in particular is questionable. The authors use Summerfield and Hulton 1994 as a basis for a first estimate of denudation rates. This work measured river loads in some of the world's largest rivers. There are two potential problems with this approach. First, river loads integrate over years, and are prone to producing either high or low outliers (see Kirchner et al., 2001). Second, these large basins integrate both mountainous and more gentle landscapes. As such, they underestimate the erosion occurring in the mountainous areas.

More recent studies/compilations that use longer-term time-averaging approaches such as cosmogenic nuclides suggest significantly faster erosion in active tectonic settings (see Portenga and Bierman, 2011; Wittmann et al., 2016; among others). The Wittmann paper provides a good example of the first problem. From S&H 1994, the Danube is recorded as having a total denudation rate of 52 m/Myr while cosmogenic nuclides suggest that the denudation rate over geological time-scales is 412 m/Myr. From P&B 2011, the average denudation rate from all measured seismically active basins is 367 m/Myr.

In the European Alps, which the authors note resembles the GSM, thermochronometry-derived exhumation rates often approach 1000 m/Myr (Fox et al., 2016; among others) and cosmogenic nuclide-derived denudation rates often exceed 1000 m/Myr (see Delunel et al., 2020; among others). Indeed, the authors also cite Koppes and Montgomery 2009 in support of 100 m/Myr as a fast erosion rate, however as far as I can tell, 100 m/Myr is among the slowest rates reported for active orogens by these authors, with erosion rates in excess of 10000 m/Myr being common (note that K&M 2009 report their rates in mm/yr). As an active orogen, the GSM was likely uplifting significantly faster than 100 m/Myr. Clearly this would lead to faster response times for these fluvial systems and has implications for the interpreted tectonic history. The relationship between erosion rate and relief ratio would also change.

The above three paragraphs relate to a key question we considered during our analysis, namely, "what is an appropriate range of pre-Oligocene fluvial denudation rates to assume for the GSM in our stream power incision modelling?". For clarity, in our study the denudation rate (E) is used to back-calculate the erodibility coefficient (K) according to Eq. (12), which is then fed into the stream power incision model (Eq. 11). Eq. (12) shows that K depends also on k_s; this is well constrained for the Gamburtsevs via chi analysis, whereas we do not have any direct constraints for E in pre-glacial times in East Antarctica from sediment yields or cosmogenic nuclides.

Given this limitation, the aim of our stream power incision modelling was not to estimate a single value for the timing of Gamburtsev uplift, but instead to place bounds on this age and in turn evaluate competing scenarios for GSM uplift. In this context, constraining the upper (i.e., oldest) age limit for commencement of GSM uplift and valley incision is a more important than the lower (i.e., youngest) age limit. This means we focussed primarily on identifying the lowest plausible erosion rate. For this, we selected 10 m/Myr for the low erosion rate scenario based on values recorded by detrital thermochronology data from Prydz Bay (Thomson et al., 2013). However, we concur with the reviewer that this most likely underestimates true denudation rates for the GSM as these data integrate a large catchment including mountainous and low-relief landscapes and are long-term temporal averages. Rates of 10 m/Myr would also be anomalously low (in the global context) for a steep mountainous area.

We selected 100 m/Myr for the high erosion rate scenario, but we accept that this value is low when compared to some active orogens on Earth today, as the reviewer points out. However, there are multiple reasons why we believe a value of 1000 m/Myr is likely an overestimation of the denudation rate for basin 10 in the GSM (the basin we selected for our stream power incision modelling).

These reasons are as follows:

1. While incision rates of order 1000 m/Myr may be appropriate for highly tectonically active mountain ranges such as the Himalayas or Taiwan, the modern GSM are an intraplate mountain range and not an active collisional orogen at a plate boundary. More appropriate analogues may therefore be less tectonically active extensional orogens such as the Italian Apennines or Basin and Range province in the western USA. In these ranges, erosion rates vary along strike but are consistently on the order of 10s to 100s m/Myr (cf. Koppes and Montgomery, 2009 [their Fig. 2b]; Densmore et al., 2009).
2. The climate of interior East Antarctica in the Late Cretaceous and Palaeogene was likely cooler and more arid than in the modern-day Alps, Himalayas, and Taiwan due to its high latitude and continentality (Zhang et al., 2019). Moreover, the Palaeogene fossil plant record in Antarctica supports the presence of *Nothofagus* (southern beech) and conifer forests, implying a cool-temperate climate akin to modern-day Patagonia (e.g., Francis et al., 2008). Such a climate would be associated with a lower erodibility coefficient (K) and thus a lower denudation rate (E) than the warm, sub-tropical, and monsoon-dominated climates that characterise the Himalayas and Taiwan (cf. Eq. 2).

3. Maximum slopes in the GSM are ~20-25°, equivalent to ~400 m/km (Rose et al., 2013; their Fig. 3b). Comparing this value to the empirical global compilation from Zondervan et al. (2023; their Extended Data Fig. 6b), this slope corresponds to a 50th percentile ^{10}Be denudation rate of ~100 m/Myr (mm/kyr) and a 95th percentile denudation rate of ~500 m/Myr in metamorphic rocks (likely composition of the GSM). This implies that while erosion rates above 100 m/Myr may indeed apply to the steepest parts of the GSM catchments, choosing such a value for the entirety of basin 10, which contains much gentler slopes in its lower course, may be unrepresentative.
4. Despite the limitations of detrital thermochronology in terms of spatial and temporal averaging of erosion signals, there is no indication from these data of very high erosion rates (>1000 m/Myr) having occurred in East Antarctica during the Mesozoic or early Cenozoic. The northern GSM comprise ~20% of the Prydz Bay catchment, so simple weighted averaging implies that if E = 100 m/Myr in this area and E = 10 m/Myr in the lower-relief areas, catchment-averaged values would be ~20 m/Myr, in agreement with thermochronology data (Thomson et al., 2013). However, values of E = 1000 m/Myr in the GSM would result in catchment-averaged rates of >100 m/Myr to be observed in Prydz Bay sediments, which is not the case.
5. Offshore sediment volumes in Prydz Bay do not show evidence for Himalayan or Taiwan level fluvial sedimentation rates (and thus onshore erosion rates) prior to 34 Ma (Jamieson et al., 2005).

We accept that the above rationale was not clear in the original manuscript, and the explicit comparisons that we make between the GSM and the Alps in the manuscript make the upper erosion rate limit of 100 m/Myr appear low in that context. We will therefore modify the manuscript in the following ways:

- We will change the framing of the Methods so that we use 100 m/Myr as the most likely order of magnitude for E, with sensitivity tests run using 10 m/Myr and 1000 m/Myr representing plausible lower and upper limits for denudation rates. In the results, we will quote results corresponding to E = 100 m/Myr first, followed by the lower/upper limits corresponding to 10 and 1000 m/Myr.
- We will augment the Discussion section to explain we believe the lower and higher values to be unlikely for the GSM (building in the components described above).
- We will better emphasise in the Methods and Discussion section that, given our objectives, the minimum value of E is more important than the maximum in constraining the timing and mechanisms of GSM uplift. To illustrate this, the difference between E = 100 m/Myr vs. 1000 m/Myr is that uplift occurred ~22 Myr vs. ~2.2 Myr prior to glaciation. Ultimately, both scenarios give the same essential

message – that the modern GSM landscape cannot be 100s of Myr old and that uplift occurred relatively recently prior to glaciation.

Overall, this is a sound study adopting fluvial geomorphic analysis to provide some constraints on the timing and style of tectonics in a little-investigated region.

We thank the reviewer for this summary, and we hope the above response helps address their concerns.

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Reviewer 2 (Anna Grau Galofre and Evan Blanc)

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 Chi analysis issued from quantitative fluvial geomorphology techniques to interrogate the coupled erosion-uplift history, identify and investigate possible geological boundaries (lithology, tectonics), and test hypotheses 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 major revisions that we enclose to the authors in the attached pdf.

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

We thank the reviewers for their supportive comments and appreciation of the multidisciplinary nature of our study. We also thank the reviewers for their thorough and constructive suggestions on how to improve the manuscript. We address these below.

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 build-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 floor 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]."

We concur with the reviewers that the GSM have been modified by local alpine-style glacial erosion, as noted by Creyts et al. (2014) and Rose et al. (2013). We did not mean to give the impression that the transition to cold-based, non-erosive glaciation was immediate or that the GSM constitutes a fully fluvial landscape with zero glacial signature. We apologise for this miscommunication in section 1 and appreciate that not bringing in discussion of glacial erosion until section 5.1 is confusing. We will modify lines 33-37 to clarify that previous studies have shown that the Gamburtsev landscape is consistent with an originally fluvial terrain that was: (i) modified by (local) mountain-scale ice caps and valley glaciers prior to or at ca. 34 Ma, and (ii) preserved beneath cold-based ice thereafter. Indeed, our finding that parts of the longitudinal profiles coinciding with tributary junctions are overdeepened by ~200 m (Fig. 8) is strongly indicative of erosion by warm-based ice (valley glaciers) over multiple glacial-interglacial cycles (as discussed in section 5.1).

There is strong evidence to suggest that this phase of erosive modification by warm-based ice occurred underneath mountain-scale ice caps and valley glaciers that preceded the growth of a continental-scale ice sheet, rather than during a subsequent retreat phase of the ice sheet. First, the geomorphology suggests that glaciation was local in pattern (i.e., ice was confined to the high elevations of the Gamburtsevs) (Rose et al., 2013). Second, global mean sea level reconstructions show that after the EAIS first grew at ca. 34 Ma, sea levels fluctuated through the Oligocene and Miocene, but (a) the amplitudes of these fluctuations are ~20 m, and (b) sea levels did not return to pre-34 Ma values signifying an ice-free world (Rohling et al., 2022; Miller et al., 2025). These variations in sea level are not large enough to signify that, at any stage after 34 Ma, Antarctica was nearly ice-free or that ice was once again confined to local bodies over the GSM. Although appreciable ice-sheet retreat during warmer intervals such as Oi-2b and the Miocene Climatic Optimum is likely, a substantial amount of ice was retained, and the core (i.e., the GSM area) would have been cold-based, with warm-based, erosive ice located at the margins. Third, this is supported by ice-sheet modelling, which suggests that cold-based ice was established over the GSM as soon as the ice sheet began expanding beyond a locally-glaciated system, and that this cold-based, non-erosive core persisted over the GSM throughout the Oligocene and Miocene (Jamieson et al., 2010). Fourth, detrital thermochronology data from Prydz Bay indicate that, after a spike coinciding with initial EAIS growth at ca. 34 Ma, low erosion rates have persisted in central East Antarctica to the present day (Thomson et al., 2013).

We will add parts of the text above to the manuscript Introduction and Methods, so that the assumption and justification of negligible erosion after the initial alpine phase prior to or at ca. 34 Ma is made more clearly.

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, Chi 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).

We agree that more discussion of this is needed earlier in the manuscript than section 5.1. We will revise the manuscript to include an explicit description of the features in the longitudinal profiles that are indicative of fluvial vs. glacial erosion at the beginning of the Results section (4.1). This will enable us to justify where the imprint of glacial erosion does not appear to be significant (i.e., basin 10) and therefore where stream power incision modelling can be confidently applied, as the reviewers suggest in their next major point.

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.

We will make a note of this point in our revisions relating to the above comment. We note, however, that the high relief, low width-to-depth ratios, and V-shaped geometries of the valleys in the GSM are not consistent with substantial erosion by subglacial water or cold-based ice (Rose et al., 2013). Valley morphologies indicate that the erosive effects of these processes were minimal, if not negligible. Moreover, subglacial meltwater channels tend to form in more erodible substrata rather than the crystalline bedrock that likely characterises the GSM (van de Vegt et al., 2012). See also our response to the next comment.

Line 140 – The D8 algorithm's assumption that water is always routed downslope breaks when considering possible subglacial meltwater contributions, where likely were possible in the region (Creyts 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.

This is a good point, but we emphasise that in Creyts et al. (2014) the authors suggest that where localised meltwater in the valleys is pushed uphill, it encounters colder ice and refreezes to the bed ('glaciohydraulic supercooling'), facilitating the preservation of the Gamburtsevs rather than their erosion. There is no suggestion in the Creyts et al. (2014) paper that a subglacial drainage system is actively eroding the Gamburtsev landscape. We will make this point clearly in the vicinity of line 140.

The cross-divide valleys picked out by the reviewers may reflect small-scale breaches that have been noted previously (Rose et al., 2013), and which (as in other glaciated mountains) likely reflect areas where once-localised ice caps began to join, resulting in a change of ice flow direction across drainage divides (Brocklehurst and Whipple, 2002). We also caution that the presence of interpolation artefacts in the Bedmap3 DEM and the survey line spacing makes it hard to be certain if such features are truly 'real' cross-divide valleys. They will often only be sampled by one or two survey lines 5 km apart (the reviewers highlight the uncertainty that this line spacing can give rise to in their later comment concerning hanging valleys). Since this point is tangential to our approach, we do not explore it in detail. However, we will note the possible presence of breaches amongst the evidence for high-altitude glacial modification in the Introduction.

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

We agree and we will flag here that the chi plots may also contain non-glacial signatures. This will then be discussed in more detail in our revisions to section 4.1 (see response to earlier comment).

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.

This is indeed something we discuss in section 5.1, but we will add an additional signpost to this in our revisions to section 4.1, to make clear that there are signatures of glacial erosion within the longitudinal profiles. The undulations noted coincide with valley junctions and are a few hundred metres deep in places. Indeed, elsewhere on Earth the locations of glacial overdeepenings correlate strongly with the confluences of glacial valleys, which causes ice flow to accelerate (Lloyd et al., 2023). We therefore believe the undulations are more consistent with overdeepening by warm-based valley glaciers prior to or at ca. 34 Ma (as discussed above) rather than subsequent erosion via subglacial meltwater.

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

We will add a clause to section 5.3 to cover this possibility, which we cannot fully discount. However, in our revision we will also note that these particular knickpoints are well below the reconstructed early mountain glacial limit of the Gamburtsevs (Rose et al., 2013; their Fig. 6).

First paragraph of section 5.1. 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 Chi 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).

We will modify this paragraph to explain systematically whether observations are consistent with fluvial erosion and/or glacial modification. We note that this does not influence the conclusions drawn from basin 10's stream power incision modelling since the morphology of this longitudinal profile (and the findings of previous work; Rose et al., 2013) suggests this basin was primarily below the mountain glacial limit and shows minimal evidence for glacial modification compared to higher-elevation basins in the central and northern GSM. We will explain this in our revised manuscript.

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 of 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.

We agree. This links to the previous comment and we will re-structure this section of the Discussion to avoid the unintended confusion of initially analysing the landscape entirely through a fluvial framework and then 'admitting' that some morphologies are indicative of glacial modification.

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).

We agree and will revise this conclusion as suggested.

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.

We appreciate that the logical thread of the competing roles of fluvial and glacial erosion was not as well laid out as it could have been. We agree with the reviewers' suggestion that this structure should be modified. Indeed, this links to many of the comments made above. We will revise elements of the Introduction, Methods, and Discussion to be clearer up-front about the potential role of glacial erosion, so that it does not appear as a ‘surprise’ in section 5.1 as it does currently.

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 Chi 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.*

These are very helpful suggestions, and we will happily adopt suggested changes 1, 2, and 3 in the revised manuscript. Point 4 is less relevant, as the manuscript primarily focusses on uplift that occurred millions of years prior to glaciation, so the minor isostatic effects of heterogenous ice (un)loading over a relatively short interval around 34 Ma are insignificant. Given concerns about manuscript length (see below), we feel this is extraneous detail that does not influence our main conclusions, so we will not incorporate this point.

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

This is a fair suggestion and one that we considered during the writing of the paper. We decided, however, that while this framework is familiar to the fluvial geomorphology community, it is not well known to the glacial dynamics and Antarctic science communities. To maintain the broad appeal and interdisciplinary nature of the manuscript (which the reviewers highlight as a strength of the paper in their summary), we feel it is important to retain this background in the Methods section. However, we will assess the length of the manuscript following the revision process, and some material can readily be moved to the supplementary information if necessary.

Assumptions and 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.

We agree that the difference between the different types of 'resolution' in this context is not made fully clear partly because the discussion is spread between different sections. We will move the information from line 302 forward to join with the information in line 98-99. We will also explicitly point out where the resolution is sufficient (or not) to support conclusions made later in the manuscript.

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.*

Concerning the reviewers' point about hanging valleys, we agree that there is an oversight here, particularly regarding the spacing between flight lines (i.e., individual points on the long profiles). It is indeed not always possible to discern whether some tributaries are truly hanging above the main stem due to the data resolution limitation. We agree that some terminal elevation points of tributaries are above the trunk valley. This could be because either: (a) the tributary is a hanging valley, or (b) there is a horizontal gap (on the ground) of several kilometres between the terminal tributary point and the nearest trunk valley point. However, the data do show that this uncertainty only applies to a minority of the valleys, that vertical offsets are typically <100 m, and that the largest offsets (such as in basins 4, 5, and 8) are found in locations where there is indeed a large data gap. Valley junctions that are well resolved by the radar data do not show signs of hanging valleys.

We will modify the text in lines 293-294 to make this clear. Importantly, the tributaries in basin 10 (used for stream power incision modelling) are well resolved and do not show any clear hanging patterns. We do not see how this issue applies to the analyses in section 3.4, but we will follow the reviewers' suggestion of more clearly quantifying the uncertainty and whether it affects the conclusions.

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.

We agree that some assumptions were not fully explained. The points concerning fluvial vs. glacial erosion and hanging valleys have been addressed in previous comments. On the erosivity coefficient (K), we do indeed explain that this can be controlled by multiple factors (line 170). We conclude that rock strength variation is most likely because of the strong correlation of K (k_s) with the magnetic anomaly patterns. However, we will rework the discussion (cf. line 520) to more clearly explain why we dismiss other possibilities (e.g., climate, hydraulics). The assumption of spatially uniform uplift rates was made simply to avoid additional, unconstrained, model complexity. We will explain and justify this choice more clearly in the revised manuscript.

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.

By this we simply meant that we began our model runs with a 'flat' landscape whose initial elevation was set to that of the lowermost point on the longitudinal profile for the basin being modelled. This value was also considered to be the 'base level' (below which rivers cannot erode). Although these points are clearly not the 'true' base level since the valley will continue further downstream outside of the surveyed area (with the probable exception of basin 10), this does not matter as the modelled profiles could simply be shifted vertically up/downwards to match. We will revise the manuscript at line 259 to avoid this confusion.

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.

In the SPIM we can prescribe the total magnitude of uplift and a total run time. Assuming the uplift is temporally constant, the uplift rate is simply calculated as the magnitude divided by the time. We constrained the total magnitude of uplift as the difference in elevation between the top and bottom of the long profiles, and the model run time was tuned to best match the observed profile (the model run time was one of the key outputs of our simulations). We will rephrase this sentence accordingly to avoid confusion.

Line 357 – Why can't slopes be compared directly? They are a non-dimensional property (H/L), and is better defined than 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)

We did not mean to imply that slopes cannot be compared between basins because they will have a different dimensionality (as the reviewers point out, slopes are dimensionless). We instead meant that, according to Eq. (1), channel slope scales according to drainage area in a manner dependent on k_s (steepness index) and theta (concavity index). These indices vary between basins, and so therefore does the scaling between slope and drainage area. Therefore, differences between the slopes of valleys in different basins cannot be meaningfully interpreted without first accounting for the difference in this scaling. This is the purpose of chi analysis and comparing k_{sn} (normalised steepness index) values. We will rephrase this sentence to clarify this point (also made in lines 211-214).

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 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).

We agree that K is not solely dependent on bedrock lithology and does incorporate a range of other parameters to represent incision efficiency. Indeed, we already explicitly state this when we present the stream power model in line 169-170. Numerous studies have however found clear links between K and independent measurements of rock strength, suggesting that rock type is a key control on K, such as the classic experiments by Sklar and Dietrich (2001) that found fluvial incision efficiency was inversely proportional to the square of the rock tensile strength.

We believe that the comparison of our computed values with the empirical findings of Stock and Montgomery (1999) is appropriate, as the authors give ranges for each rock type that will reflect the competing influences of other factors, such as those listed by the reviewers above. These values have been used for comparative purposes in recent work (e.g., Hilley et al., 2019). We also note that the differences in K between rock types are several orders of magnitude, so are useful comparators even allowing for variation in other factors that influence K.

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

Yes, the magnetic signature within the Gamburtsevs (with high amplitude 'lows' and 'highs' and a clear E-W structural 'grain') is consistent with gneissic basement rocks that are common in East Antarctica, which is thought to comprise a mosaic of cratonic terrane (Ferraccioli et al., 2011; Nature). We will update this sentence to clarify.

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.

This is a minor phrasing issue. We will update the sentence to '…and therefore likely constituted base level for basin 10 prior to EAIS formation.'

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 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 will relax the wording of this statement as suggested but note that such erosion rates would be anomalously low for a mountainous setting (as noted by reviewer 1). We are considering erosion rates prior to glaciation here, so a regime of fluvial systems is an appropriate assumption (section 5.1 concerns the later modification of the fluvial network, so is later in the timeline).

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

We thank the reviewers for their detailed and constructive input.

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