Reply to Reviewer 1, Professor Luca C Malatesta

Thank you very much for your thoughtful and constructive comments. We greatly appreciate the time you invested in reviewing our manuscript. Below, we provide point-by-point responses. We will revise the manuscript to incorporate the changes described in this letter.

Comment 1: Grain size data

Reviewer's comments

Some additional information is needed to better understand how grain size data is collected. To my eyes, unfamiliar with the rivers in question, the D50 values of most rivers, between 40 and 70 cm, are very high. Is the necessary bankfull depth to transport these clasts reasonable with observations? A back of the envelope calculation using Shields stress to calculate the critical water depth for incipient motion,

$$\tau * = (hS)/(RD) = \tau^*_{\text{crit}},$$

gives a water depth of ca. 2 m at a slope of 2%, ca. 4m at 1%. Does that seem reasonable given the field context?

Authors' response

We agree that the reported D_{50} values appear unusually coarse at first glance, and we appreciate this opportunity to clarify the field context and our definitions. In our study reaches, clasts in the boulder size range are common on the bed surface, and we interpret bedrock erosion to be dominated by high-discharge events that are capable of mobilizing these coarse tools.

To address your question quantitatively, we estimated the minimum flow depth required for incipient motion using the Shields-type relation $\tau^* = \dfrac{hS}{R_b D_s}$. For example, in Takinosawa, the reach-averaged slope is $S \approx 0.046$ and the reach-averaged representative grain size is $D_s \approx 0.4$ m. Assuming $\tau^* = 0.05$ and $R_b = 1.65$, the corresponding minimum flow depth is $h \approx 0.72$ m. This value is consistent with our field observations during high-flow conditions and is not unrealistically large for these channels.

We have added this clarification to the revised manuscript to explicitly state (i) the assumed Shields parameter and density ratio, (ii) the resulting range of estimated minimum flow depths across tributaries, and (iii) that the relevant flows are high-discharge events rather than mean daily flows. The added text to the new line 370 reads:

This value is reasonable in the field because the estimated minimum flow depths using $\tau^* = \dfrac{hS}{R_b D_s}$ *range from 0.5–1.0 m across the study tributaries under* $\tau^* = 0.05$ *and* $R_b = 1.65$*, and these depths are plausible during high-flow events that dominate bedrock erosion.*

Reviewer's comments

What is the minimum size that the drone survey could consider (I am not very familiar with the method), does that artificially raise the D50 compared to a hand survey down to ca. 2 mm grain size? A quick glance at a few granulometry tables from the data on Zenodo shows that on ca. 1 cm seems to be the smallest grain size. How does that bottom threshold impact the D50 calculation?

Authors' response

Thank you for raising this important point. In our drone-based workflow, the minimum measurable grain size is constrained by point-cloud resolution. Following the method of Steer et al. (2022), we required each grain to be represented by at least 10 points in the 3D point cloud, which corresponds to approximately 1–2 cm in the field of view (depending on flight altitude and image resolution). Therefore, grains finer than ∼1–2 cm are not reliably detected.

This lower detection threshold has limited influence on our representative grain-size metric because we use a volume-weighted statistic (the median of the cumulative *volume* distribution; i.e., CDF = 0.5 with weights proportional to grain volume). This is conceptually similar to mass-weighted statistics from sieve analyses, where coarse fractions dominate the total weight. Consequently, excluding very fine grains has little effect on the volume-weighted median when coarse clasts constitute a large fraction of the bed-surface volume.

We have added the above clarification to the revised manuscript, including the practical detection limit and the rationale for using a volume-weighted metric. The added text reads:

- new Line 293–297: *The grain sizes of the riverbeds were measured from dron-derived 3D point cloud data. In recent years, the methods for automated grain-size analysis have been rapidly developing g (e.g., Soloy* *et al., 2020; Steer et al., 2022; Mair et al., 2024).. Among those studies, the method proposed by Steer et al. (2022) can measure the three axes of the grains from 3D point cloud data obtained by drone photogrammetry, allowing the grain axes lengths and the volume of grains to be measured. In this study, we adopted the volume-based metric for the representative grain size, so that the G3Point method Steer et al. (2022) was employed.*

- new Line 307: *In the G3Point method, 10 points are required to fit an ellipsoid to a grain, so the measurable minimal grain size is 1–2 cm.*

Reviewer's comments

More details about the manual pebble count is also needed. Which method was followed? What was the threshold to “ fine ” ?

Authors' response

We agree and have expanded the description of the manual measurement procedure. Our manual survey was not a classical heel-to-toe Wolman count because many clasts are too large to be feasibly sampled that way. Instead, we delineated a several-square-meter area on a river bar using spray paint and measured the a-, b-, and c-axes of clasts that were (i) visually identifiable from a vertical viewpoint, (ii) had exposed surfaces, and (iii) had a short axis of at least 1 cm (to match the minimum size of the automated measurements). We have added these details to the revised manuscript and clarified that the lower threshold for manual measurements was 1 cm in short-axis length. The added text to new Line 317–318 reads:

First, we delineated a several-square-meter area on a river bar using spray paint. Within this area, we measured the sizes of clasts that were visually identifiable from a vertical viewpoint, had exposed surfaces, and possessed a short axis of at least 1 cm.

Comment 2: Timescales

Reviewer's comments

Towards the end of the introduction, l. 82–91, you go over various constraints for rates of erosion/denudation/exhumation/(rock?) uplift. While all these processes are closely related they are not always identical. And often they are measured over different integration timescales. These two paragraphs need to be revised to be more rigorous and explicit when providing this background.

The exact terminology for the various types of erosion-related rates should be double-checked. Thermochronological ages provide a rate of exhumation which is equal to the negative local erosion rate. But denudation is a little different as it regards the evacuation of material at the scale of a catchment (due in large part to the work of erosion). I think that thermochronology can constrain erosion, but not denudation. And basin-wide cosmogenic studies provide a rate of denudation (at the scale of the catchment) and not of erosion. (It is worth checking these terms against another source than myself.)

Authors' response

Thank you for this careful and very helpful comment. We have thoroughly revised the relevant paragraphs in the Introduction to (i) use consistent and standard terminology and (ii) explicitly state the characteristic spatial and temporal integration scales of each metric.

Specifically, we now distinguish among: (i) *rock uplift rate* (vertical motion of bedrock relative to a reference frame), (ii) *exhumation rate* inferred from thermochronology (long-term removal of overburden, commonly approximating the negative of long-term local erosion rate under steady conditions), and (iii) *catchment-wide denudation rate* inferred from cosmogenic nuclides (spatially averaged evacuation of material over a drainage basin, integrating over 10^3–10^5 yr timescales depending on nuclide system and erosion rate).

We also replaced the ambiguous term "uplift rate" with "rock uplift rate" throughout for clarity. In addition, we revised the text to avoid implying that thermochronology directly measures denudation at the catchment scale. The revised manuscript now explicitly states which reported rates correspond to exhumation versus catchment-wide denudation and notes that these constraints integrate over different timescales.

Comment 3: Display of results

Reviewer's comments

The models rely on a lot of variables that evolve along stream. Some are all strictly proportional: A, Qw, W in SFDM. Some are (partly) independent, S, H, P_c, tau_s, Shields stress. Others come from field and lab surveys $D_s, sigma_t$. When parsing through the results, I wanted to see how they all evolved to understand why a model behaves this or that way. Would it be possible to provide more information side by side on a representative profile? E.g. for the Gohyaku river, where the three models behave very differently.

Figures 9 and 10 can be combined advantageously, and a figure could be dedicated to showing one river in more details.

Authors' response

Thank you for this suggestion. We agree that a side-by-side visualization of key variables along a representative profile would greatly improve interpretability. In the revised manuscript, we (i) combined the former Figs. 9 and 10 into a single figure to directly compare the modeled profiles and fits, and (ii) added additional supplementaly information showing the longitudinal variations of key variables/inputs (including those derived from field data) along a representative river profile, with a focus on the Gohyaku River where model behaviors differ most strongly.

Comment 4: Bankfull width and depth

Reviewer's comments

Given the experience in the field, you have an opportunity to check further values of the models against measurements or at least qualitative impressions. Does the predicted width falls in the range of observed bankfull width. What about the bankfull water depth? It is not explicitly calculated but lies indirectly in Eq. 14. If you compute it, how does it compare with field observations? I don't know if width and depth were surveyed, but even if they were not systematically measured, it would be good to provide an impression in the context of your field experience.

Authors' response

Thank you. We agree that additional ground-truthing is valuable. Although we did not systematically survey bankfull geometry at all sites, we estimated approximate bankfull widths from several 3D point-cloud datasets that capture bankfull indicators. These estimates are broadly consistent with the modeled channel widths.

For example, the estimated bankfull width is approximately $\sim$8 m for Takinosawa, $\sim$10 m for Hisawa, and 15–20 m for the Gohyaku River. The corresponding modeled widths are $\sim$7 m for Takinosawa and Hisawa and $\sim$21 m for the Gohyaku River. Thus, discrepancies are generally within several meters, which is reasonable given (i) uncertainty in bankfull identification from point clouds and (ii) natural width variability along the reaches. We have added this comparison to the revised manuscript and clarified that our field measurements primarily captured active-channel width, while bankfull width was estimated where point-cloud coverage permitted. The added text to the new Line 386–392 reads:

Bankfull widths predicted using Eq. (19) with the optimized values of kw and kSFDM are consistent with field observations and with estimates derived from 3D point-cloud datasets of the riverbeds. For Takinosawa, the predicted bankfull width is approximately 5–9 m, whereas the observed width is about 7 m. Similarly, the predicted widths for Hisawa and the Gohyaku River are approximately 5–7 m and 22–24 m, respectively, which are comparable to the observed widths of about 10 m and 15–20 m. For Fukazawa and Sangasawa, the predicted bankfull widths are approximately 13–16 m and 4–9 m, respectively. Direct measurements of bankfull width were not available for these rivers.

Comment 5: Lithological controls

Reviewer's comment

The SFDM model considers the tool effect, but it does not directly take the tensile strength contrast of tools and bedrock. As far as I understand, it does however indirectly acknowledge it because tool size is part of the equation and should be reasonably expected to increase with rock hardness. Is this correct? If so, you could mention the presence of this indirect role of tool/bedrock strength in the discussion.

Authors' response

Thank you for this important point. We agree with your interpretation: SFDM does not explicitly include a tensile-strength contrast between tools and bedrock, but it can indirectly reflect lithological influence because tool size (D_s) is an explicit control in the formulation, and tool size may covary with basin-scale lithologic and geomorphic conditions that influence hillslope supply and clast production.

We have expanded the Discussion to emphasize that (i) bedrock properties can influence the characteristic tool size produced and supplied from upstream, (ii) downstream fining through abrasion further modifies tool size, and (iii) consequently, local bedrock lithology at a point does not uniquely determine channel gradient, whereas basin-integrated lithologic controls can still shape longitudinal profiles through their influence on tool production and size distributions. We added a paragraph discussing this indirect lithologic role and cited relevant studies (e.g., Sklar et al., 2017; Verdian et al., 2021; Parker, 1991).

Line-by-line comments

Line 44: "incision episodes"

We agree to change to "incision episodes", and revised the manuscript.

Line 57–60: cite the authors of these models right away.

We added citations to these models.

Line 61: It would be useful to read the motivation for picking this site. Surely there are many catchments in Japan with varying lithology where that work could be done. I gather it is due to the availability of studies constraining rates of uplift and erosion.

We agree. We added the following phrase to the manuscript:

, because in this region, studies on uplift and erosion rates have been extensively conducted...

Line 70–80: grammatical comments

We followed your comments on grammar and spelling, and revised the manuscript accordingly.

Line 82, 99: You are talking about rock or surface uplift here? It would be good to specify it especially since you write about exhumation in the same paragraph.

We specified that it is "rock uplift rate" throughout.

Line 84: how did Fujiwara get to that number. I am not able to access the original article in Chikyu Monthly.

We added the following sentence:

The rock uplift rate can be estimated using the T-T method, based on the relative heights of river terrace surfaces formed during the Last Interglacial and the preceding glacial period (Yoshiyama and Yanagida, 1995; Tanaka et al., 1997).

Line 85–90: what is the integration time of the U-Th/He measurement? "most of the results". Is that the "results" of Matsushi? To me the formulation suggests that there are multiple studies, each with their own results. Fukuda et al. produce thermochronological constraints for exhumation rates. It should be mentioned since the last denudation rate is from cosmogenic nuclides.

Thank you. We revised these sentences to (i) explicitly state that U–Th/He thermochronology provides long-term *exhumation* constraints integrated over Myr timescales (depending on cooling history), and (ii) clarify which specific studies provide exhumation versus catchment-wide denudation constraints. We also removed ambiguous phrasing such as "most of the results".

Line 109: the equations are part of the sentences and should be followed by punctuations, comma or period, as needed. Further nitpicking but I believe that subscripts that are not variables themselves, but simply qualifiers of the main variable, should be written upright and not italics. Just as in equation 26.

We added punctuation where sentences end with equations and revised qualifying subscripts to be upright.

Line 124: just a clarification for me, rho_s is the standard density, or the submerged density of the rock?

We clarified that ρ_s is the standard density of quartz (=2650 kg m^{-3}).

Line 130: I enjoy the step by step explanation of the model setup. We seem to jump quite suddenly to Ir without much context compared to all other steps.

We revised the description of I_r to present it in two steps as follows:

I_r *is expressed as:*

$$I_\mathrm{r} = \frac{\rho_\mathrm{s} q_\mathrm{s}}{M_\mathrm{p} L_\mathrm{s}}. \tag{1}$$

Substituting $M_\mathrm{p} = \rho_\mathrm{s} \pi D_\mathrm{s}^3/6$ *into Eq.* (1)*,*

$$I_\mathrm{r} = \frac{6}{\pi D_\mathrm{s}^3} q_\mathrm{s} \frac{1}{L_\mathrm{s}}. \tag{2}$$

Line 154: extra space after opening parentheses

We removed the extra space.

Line 178: in this long expression, W is a function of Qw (itself already present in eq. 19) and a coefficient kw (eq. 18). Wouldn' t it be simpler to insert eq. 18 in eq. 19 and get rid of W to only keep kw a parameter you will optimize for?

We agree and revised the expression so that k_w is explicitly stated as follows:

$$S_{\mathrm{eq,SFDM}} = \left[\frac{R_{\mathrm{b}} g^{1/3} D_{\mathrm{s}} k_{\mathrm{w}}^{2/3}}{C_f^{1/3} Q_{\mathrm{w}}^{1/3}} \left\{ \tau_{\mathrm{c}}^* \right. \right. \tag{3}$$

$$\left. \left. + \left(\frac{\beta_0 q_{\mathrm{s}}^2}{5.7 (R_{\mathrm{b}} g)^{1/2} D_{\mathrm{s}} (\beta_0 D_{\mathrm{s}}^{1/2} q_{\mathrm{s}} - k_{\mathrm{s}}^{1/2} \sigma_{\mathrm{t}}^2 U)} \right)^{2/3} \right\} \right]^{3/2}. \tag{4}$$

Line 182, 241, 244 : bedrock singular

We corrected "bedrocks" to "bedrock".

Line 184: Shobe et al. 2017 (GMD) are the original authors of SPACE and should be cited here.

We added Shobe et al. (2017) when describing the SPACE model.

Line 215: Should there be a reference for Optuna?

We added Akiba et al. (2019) as a reference for Optuna.

Line 220: how are kappa_a and kappa_b set? For ks in equation 10. Your comment was about how set κ_a and κ_b in Eq.10.

We followed Inoue et al. (2014) and set $\kappa_a = 2.5D_s$ and $\kappa_b = 0.0032$. We added these values to the manuscript.

Line 228: It could be clearer to directly refer to the critical drainage area: "The pixels draining more than 0.7 km2 were regarded [···]"

We followed your comment and revised the manuscript.

Line 236: " linearize "

We followed your comment and revised the manuscript.

Line 257–272: grammatical comments

We followed all your comments and revised the manuscript accordingly.

Line 272–275: Can you provide details about the manual measurement? Was a it a heel-to-toe Wolman count, did you measure all the clasts in the top x-cm of the squares shown in Fig.2? Was it on a river bar, or across bar and channel?

We added the following sentences to clarify the manual measurements:

First, we delineated a several-square-meter area on a river bar using spray paint. Within this area, we measured the sizes of clasts that were visually identifiable from a vertical viewpoint, had exposed surfaces, and possessed a short axis of at least 1 cm.

Line 286: concave up?

We corrected "concave downward" to "concave up".

Line 291: what is "slightly"? An actual distance would be useful.

We revised the text to provide an actual distance. The lithologic boundary is located at approximately 120 m downstream from the knickpoint.

Line 292: "Check dams", no definite article

We followed your comment and revised the manuscript.

Line 292–293: what about Sangasawa? The check dam does not correspond to a break in slope?

We revised the text to clarify that the check dam is not responsible for the observed step in Sangasawa and added the following sentences:

In Sangasawa, a check dam is located 10 m downstream of the confluence, which makes it difficult to distinguish the effects of slope changes caused by variations in drainage area. However, the step observed in the topography within the granite occurs upstream of the dam, indicating that the dam is not responsible for this feature.

Line 294: I'd suggest to consider removing "typical". I do not know what is a "typical" knickpoint in this context.

We removed the word "typical".

Line 307: pick one of the adverbs “closely” or “well”. I think the reference to Fig. 6 is missing earlier

We followed your comments and revised the manuscript.

Line 308: “in the Gohyaku RIver.” “in the Sakura River”

We added the definite article where appropriate and corrected capitalization/typing.

Line 317: this is very very coarse, I’m surprised! Gravel is a size category from 2 to 64 mm. 400 and 700 mm are boulders. A D50 in boulder category is truly massive and suggests debris flow control rather than fluvial processes (from my experience). Are you sure there is not a misplaced decimal here? Alternatively, does the remote sampling of grain size introduce a bias by missing grains below a certain threshold? The examples shown in Figure 2 look much finer than the D50 listed in the text.

Thank you for this careful check. We confirm that the decimal point is not misplaced. The large values arise because boulder-size clasts ($>$ 256 mm) are present at most sites and strongly influence our *volume-weighted* representative grain size metric. In addition, Fig. 2 shows locations that appear locally finer than the reach-scale statistics because clast sizes vary spatially and the photo frames capture only part of each reach.

Regarding potential bias from the remote method, we acknowledge that grains finer than $\sim$1–2 cm are not reliably detected, as explained above. However, because we use a volume-weighted statistic, the omission of very fine grains has limited influence on the representative grain size when coarse clasts constitute a large fraction of the bed-surface volume. We have clarified these points in the revised manuscript and explicitly state that the modeled “dominant” grain size corresponds to the coarse tools relevant for bedrock abrasion during high flows.

Line 322: Provide a reference to Figure 9

We added a reference to Fig. 9.

Line 324–325: I realize now that channel width was not surveyed in the field or remotely. Width being an important component of the calculations, can you provide a ground truth for these values? Are they reasonable?

Using several 3D point-cloud datasets, we estimated approximate bankfull widths. The bankfull width in Takinosawa and Hisawa is about $\sim$10 m, and that in the Gohyaku River is about 15–20 m. These values are broadly consistent with the modeled widths.

Line 328: Is the Sangasawa knickpoint at ca. 3000 m at the transition granodiorite to sandstone? It is difficult to see in Fig. 9 d. I suggest to add a label on the figure.

We added a label on Fig. 9d to indicate the location of the knickpoint in Sangasawa.

Line 348: " Sediment-cover effect "

We followed your comment and revised the manuscript.

Line 357: Given the importance of the cover ratio. Would it be possible to plot the ratio along the profile of Fig. 10 (maybe on a shared y-axis) so that we can see how it changes along slope and lithology? Fig. 12 provides the theoretical expectations. It would be helpful to see it applied to a river. And/or, as suggested at the beginning, provide a separate detailed display of the different variables modeled for one of the rivers.

We added a figure showing the longitudinal distribution of the cover ratio in SFDM, and we also expanded the revised figure set to better visualize how key variables evolve along a representative river profile.

Line 385: Could you provide some references here? I imagine that the sediment-stripped channels of out-of-equilibrium systems are meant to be only for those that react to an increase in rock uplift rate, or precipitation. A river flowing across a mountain that that halved its rock uplift rate will also be out-of-equilibrium but wouldn ’ t it be going through a phase of alluviation, at least downstream?

Thank you for this suggestion. We agree that out-of-equilibrium conditions can arise from both increases and decreases in forcing (e.g., rock uplift rate or discharge). We revised the manuscript to clarify the statement and added references to general frameworks on transient river response and channel adjustment, including cases where decreased uplift can lead to downstream aggradation and alluviation during adjustment.

Line 399 : In-text citation format where needed.

We corrected the citation format.

Line 416: Gohyaku

We corrected the typo.

Figures

Figure 1: Missing the label (a) for the inset. In (b) could you leave the hillshade as a semi transparent layer on top of the DEM colors? It ’ s a shame to mask it. (c) the legend of the geological map is incomplete. I distinguish two shades of yellow/orange, one blue, and three (?) greens that are not documented.

We revised Fig. 1 following your comment.

Figure 9–10: In my opinion Figures 9 and 10 should be combined. Seeing the three fitted models together — SFDM, A-SPM, and SPACEM — would show the quality of fit even better. And it would save you one figure in an already long article. It would be good to indicate with a symbol (arrows, vertical lines) where tributaries join the main stem to provide information that is otherwise found on Fig. 4 and hard to read by going back and forth. Fig. 10: the symbol for A-SPM is a dashed line in the legend but a dash-dot pattern in the figure.

We combined the former Figs. 9 and 10 as suggested. We fixed the line-style inconsistency for A-SPM between the legend and the figure.

Reply to Reviewer 2, Professor Fritz Schlunegger

Thank you very much for your thoughtful and detailed comments. We greatly appreciate the time and effort you invested in reviewing our manuscript. Below we provide point-by-point responses and describe the corresponding revisions made in the manuscript.

Comment 1: Model uncertainties

I wonder to what extent the model results depend on the uncertainties associated with these variables. This issue is not discussed in the current version and should be further explored.

Authors' response

We agree that the influence of parameter uncertainties on model predictions is an important issue and was insufficiently addressed in the original manuscript. To address this concern, we conducted a sensitivity analysis to evaluate how uncertainties in key model parameters affect the outputs of each model in Supplement.

Specifically, we varied representative parameters (including those controlling sediment supply, transport capacity, and erodibility) within their plausible ranges reported in the literature and examined the resulting variations in predicted river profiles and erosion rates. The results show that, within these ranges, the predicted longitudinal profiles and relative contrasts among lithologies remain stable and do not exhibit qualitative changes. This indicates that the main conclusions of this study are robust to reasonable parameter uncertainties.

We added a new supplement to the revised manuscript describing the sensitivity analysis, its methodology, and its implications for model reliability. We also refer to these results when discussing model performance and limitations. We added the following text to the manuscript (new Line 413–422):

We performed a sensitivity analysis of parameters from the literature by repeatedly re-optimizing the model to minimize the RMS error between observed and calculated topography for the Gohyaku River using Bayesian optimization with Optuna. The results showed that in the SFDM, variations in the nondimensional critical shear stress, friction coefficient, and bedload ratio have only minor effects on river profiles, whereas changes in erosional efficiency produce slight gradient variations associated with lithological contrasts *but overall stable results (Figs. S1–S8). In contrast, the ASPM exhibited strong parameter sensitivity: river profiles deviate markedly from observations when the slope exponent n exceeds 1.5 or when the drainage area exponent m is below 0.5, indicating that the adopted values ($n = 1.0$, $m = 0.5$) lie within a stable range (Figs. S9–S12). In the SPACEM, channel gradients were less sensitive to n than in the ASPM and remained stable for $n \geq 0.7$, while variations in sediment erodibility K_{sed} within reported ranges caused only minor changes in gradients and RMS (Figs. S13–S18). Overall, the sensitivity analysis confirms that the parameter values used in this study fall within stable regimes and yield results consistent with the observed topography.*

Line-by-line comments

Thank you for your detailed comments on clarity, wording, and grammar. We revised the manuscript accordingly. Specific responses are provided below.

Line 13: It is not obvious how a reduction of the slope enhances the sediment cover etc. This needs to be explained more clearly.

Thank you for pointing this out. We clarified the causal relationship between slope, sediment transport capacity, and sediment cover. The revised text now reads:

Higher bedrock erodibility reduces channel slope and sediment transport capacity, promoting sediment cover. The resulting sediment cover suppresses further erosion and offsets the effect of bedrock strength.

Line 16: what is missing in the introduction is a broader presentation of a list of papers where the controls of bedrock on surface erosion and landscape shape has been demonstrated quantiatively.

Thank you for this suggestion. We expanded the Introduction to better situate our study within the existing literature. We added the following text:

In hillslope regions, several studies have demonstrated that bedrock properties, including rock strength and structural orientation, exert primary controls on erosion rates, dominant processes, and mountain topography (Cruz Nunes et al., 2015; Korup and Schlunegger, 2009; Kühni and Pfiffner, 2001). For example, in the Alpine region, Korup and Schlunegger (2009) exhibited that catchment erosion rates in harder crystalline rocks are approximately $\sim$ 0.7 mm yr $^{-1}$, whereas rates in weaker lithologies such as schist and flysch reach $\sim$ 4 mm yr^{-1}. In addition, average hillslope gradients differ by up to

0.08 between gneiss and softer rocks (Korup and Schlunegger, 2009). These studies primarily focus on hillslope regions (slopes of $\sim$ 10–60 %), where gravitational mass transport and weathering dominate sediment production and transport. However, the effects of bedrock properties on river incision and channel morphology in alluvial-bedrock regions remain less well quantified.

Line 18: not clear why weathering.

We revised the sentence to clarify the role of weathering and removed ambiguous phrasing. The revised text now explicitly links weathering to hillslope sediment production rather than to fluvial incision processes.

Line 20: as key controls of what? Please specify.

We clarified that this refers to key controls on *river incision*.

Line 28: elevation? relief? or any other metrics? Please specify.

We clarified that the metric refers to *topographic steepness*.

Line 37: In his 2001 Geology paper, Molnar argued that stream power has to exceed a threshold to entrain the gravel cover on bedrock before incision can start. You may refer here to Molnar's work. There are also other papers that use this criticality for predicting sediment transport.

Thank you for this suggestion. We added Molnar (2001) and additional relevant studies discussing critical stream power and threshold conditions for entraining sediment cover on bedrock to the Introduction and Discussion.

Line 42, 183, 191: What is a sediment cover rate?

We unified the terminology by replacing "cover rate" with "cover ratio" throughout the manuscript and clarified its definition at first use.

Line 48: see above some suggestions for further references about the hiding and protrusion effects, the sediment cover and grains with a critical size that have to be removed before further downcutting or gravel bar alteration can start.

Thank you. We added a brief summary of relevant literature on hiding–exposure effects, sediment cover, and critical grain-size thresholds, and cited representative studies that address these processes in bedrock and mixed alluvial–bedrock channels.

Line 51, 53: Grain size data is also needed. Please mention this here.

We revised the text to explicitly include grain-size data as one of the key field inputs, alongside bedrock tensile strength.

Line 73: pyroclastics are actually already volcanic rocks. So what is the difference here?

We clarified our terminology following standard petrological definitions (e.g., Le Bas and Streckeisen, 1991). In the revised manuscript, "volcanic rock" refers specifically to extrusive igneous rocks, whereas "pyroclastic rock" refers to fragmental deposits produced by explosive eruptions. We revised the text accordingly (e.g., "Late Miocene pyroclastic and volcanic rocks").

Line 93: mechanisms of a profile -¿ mechanisms resulting in the formation of a profile. Please rephrase.

We rephrased the sentence following your suggestion.

Line 102: same comment as above regarding study

We added citations to previous studies for each model.

Line 103: to.... to.... Sentence has a complicated structure. Please rephrase.

We simplified the sentence as follows:

to assess how well they capture the actual responses of bedrock rivers to variations in bedrock strength.

Line 104, 184, 191: A model does not focus on something. Please reformulate.

We reformulated these sentences to avoid anthropomorphic wording. For example:

- new Line 129: *In SFDM (Sklar and Dietrich, 2004; Whipple and Tucker, 2002), abrasion–saltation processes are explicitly represented as ...*

- new Line 73: *SPACEM incorporates both lithologic strength and sediment cover ratio ...*
- new Line 224: *SPACEM considers lithology and sediment cover ratio but does not explicitly include sediment tool effects.*

Line 178: sensitivity to empirical parameters

This concern is addressed through the new sensitivity analysis described above (Comment 1) and in the added supplementary information.

Line 209: bias due to different levels of empiricism

Thank you for this insightful comment. We clarified that the apparent simplicity of SPM and SPACEM results from lumping multiple empirical assumptions into a single erodibility parameter, whereas SFDM represents these processes with separate, physically motivated parameters. We emphasized that this does not imply greater sensitivity of SFDM to parameter uncertainty and that our new sensitivity analysis confirms the robustness of the results.

Line 228: A pixel does not flow

We rephrased the sentence as follows:

The Deterministic 8 algorithm, in which water at each pixel is assumed to flow in the direction of maximum downslope gradient, ...

Line 229: channel width issue

We rephrased the sentence to avoid ambiguity:

Flow paths draining more than 7×10^4 km^2 *were regarded as river channels.*

Line 234: concavity index justification

We corrected an error and revised the text as follows (new Line 271):

show that concavity indices typically range from 0.3 to 0.7 (Tucker and Whipple, 2002; Goren and Shelef, 2024). In this study, we adopted a value of 0.36, which was optimized for the study area.

Line 237: justification of the chi metric

We added the following clarification in the new Line 273:

Chi plots are commonly used to assess whether uplift rate and bedrock erodibility are spatially uniform (Perron and Royden, 2013). Under steady-state conditions with uniform erodibility, river profiles are linear in chi space; deviations from linearity indicate spatial variations in uplift rate or erodibility.

Line 241: naked eye

We removed this wording and clarified that lithologic boundaries were mapped based on field observations of rock properties.

Line 253: gneiss and schistosity

We rephrased the sentence to begin as follows:

The schistosity planes in gneiss can result in large variability ...

Line 257, 312: automated grain-size measurements

We added a brief review of automated grain-size measurement approaches and justified our choice of the G3Point method, emphasizing its ability to estimate grain volumes relevant for sediment-cover calculations.

Line 269: additional references

Where appropriate, we added additional references documenting previous applications of similar approaches.

Line 285, 304: influence of faults and folds

We examined channel orientation and morphology near mapped faults and folds and found no systematic changes. We added the following sentence to the manuscript:

The river course does not show significant changes at fault or fold locations, suggesting that tectonic structures exert limited control on channel morphology in this area.

Line 320: wording

We replaced “consider” with “reflect”.

Line 326: slopes exhibit a profile

We rephrased the sentence as follows:

Both the observed and modeled river profiles exhibited consistently smooth longitudinal trends, ...

Line 330: step-like structures

We clarified that these refer to step-like structures of the river profile.

Line 331: sensitivity analysis

This issue is addressed by the new sensitivity analysis described above (Comment 1).

Line 334: wording

We rephrased the sentence as follows:

Their predictions clearly reflect the influence of bedrock strength on channel slope.

Line 337: wording

We rephrased the sentence as follows:

The ASPM predicts that upstream steepness in granodiorite is approximately nine times greater than downstream steepness in sandstone.

Line 350: sediment cover evidence

We added a new figure (new Fig. 10) showing the spatial distribution of sediment cover ratio calculated using Eq. 11. This figure demonstrates that sediment cover ratio systematically varies with bedrock strength.

Reply to Reviewer 3, Professor Ellen Chamberlin

Thank you very much for your thoughtful and constructive comments. We greatly appreciate the time you invested in reviewing our manuscript. Below we provide point-by-point responses and describe the corresponding revisions made in the manuscript.

Comment 1: Lithologic boundaries and river profiles

Reviewer's comments

The authors state in lines 284–285 and the figure 4 caption that there is not a relationship between river profile and lithologic boundaries, but there are several places in figure 4 where it appears to me that changes in lithology coincide with knickpoint locations. Specifically the following locations: a) Takinosawa—between siltstone and sandstone, and between sandstone and conglomerate with an intervening tuff; c) Fukazawa— between sandstone and gneiss, and between breccia and tuff; d) Sangasawa—between granodiorite and sandstone (explanation is given about drainage area increasing, but this also coincides with the lithologic boundary quite closely···).

Certainly there are changes in slope in each profile that do not coincide with lithologic change, but these numerous examples make me seriously question the assertion that there is no relationship between river profile and lithology. I think a more detailed evaluation of this claim is warranted, and ideally the connection between knickpoint location and lithologic boundary change should be evaluated statistically.

Authors' response

Thank you for this important comment. We agree that, based on visual inspection alone, some knickpoints appear to coincide with mapped lithologic boundaries. In the revised manuscript, we therefore strengthened the analysis and discussion to more rigorously evaluate whether lithology is a *dominant* control on local channel gradients.

First, rather than comparing "rock types" qualitatively, we quantitatively examined the relationship between channel steepness and measured rock tensile strength (i.e., a continuous proxy for rock strength) and added the corresponding results figure and text in the revised manuscript (figure and line numbers to be updated to match the final layout). The analysis indicates that the correlation between normalized steepness and tensile strength exists but is very weak.

Second, we explicitly acknowledge in the revised text that apparent coincidences between lithologic boundaries and knickpoints can occur, but our results suggest that these coincidences are not systematically associated with large changes in slope. For example, in Takinosawa, there is a minor knickpoint between sandstone and conglomerate with an intervening tuff; however, the reach-averaged slope changes only slightly across this boundary (by approximately 0.004), which is much smaller than lithology-related slope contrasts reported in other geomorphic settings. We added the following text to the manuscript (new Line 339):

Several knickpoints are associated with lithological boundaries. In Takinosawa, a knickpoint occurs at the boundary between sandstone and conglomerate, with a thin tuff layer in between. The average channel slope in the conglomerate reach is 0.056, whereas that in the upstream sandstone reach is 0.052, yielding a slope difference of only 0.004 across the lithologic boundary. Other minor knickpoints that coincide with lithological boundaries exhibit a trend opposite to the commonly expected pattern, in which channel gradients are steeper in harder bedrock and gentler in weaker lithologies. In Fukazawa, knickpoints occur at both the sandstone-gneiss and breccia-tuff boundaries. However, in both cases, softer rocks (sandstone and breccia) are located upstream where channel slopes are steeper, whereas harder rocks (gneiss and lapilli tuff) occur downstream where slopes are lower.

Third, we revised the Discussion to clarify that some knickpoints may reflect local transient (non-equilibrium) conditions. In Fukazawa, for instance, knickpoints occur at the sandstone–gneiss and breccia–tuff boundaries, but the sense of the slope contrast is inconsistent with the expectation that slopes steepen in harder bedrock: softer rocks (sandstone and breccia) occur upstream where slopes are steeper, whereas harder rocks (gneiss and lapilli tuff) occur downstream where slopes are lower. This pattern does not support a simple lithology-controlled steepening and is more consistent with a locally transient state. We added the following text to the manuscript (new Line 467–476):

It should be noted that several small-scale knickpoints are present in the rivers of the study area (Fig. 5) that cannot be explained by any of the models considered in this study. Most of these knickpoints occur independently of bedrock strength, and some display trends opposite to those expected from bedrock strength, such as occurring in relatively hard rock reaches with gentle channel gradients (Fig. 4). All models used in this study assume that rivers are in a steady-state balance between rock uplift and erosion. The presence of these minor knickpoints therefore suggests that parts of the river profiles *may locally deviate from equilibrium. Knickpoints generated by the processes such as past sea-level fluctuations or localized fault activity in bedrock rivers are known to migrate upstream over time and eventually dissipate in upstream reaches (Whipple and Tucker, 1999). Such transient processes are not represented in equilibrium-based models. Despite these limitations, the models employed here—particularly the SFDM—successfully reproduce the overall longitudinal profile characteristics of the rivers. This indicates that, at a macroscopic scale, the river systems in the study area can reasonably be regarded as being close to equilibrium.*

Finally, for Sangasawa, we clarified that the knickpoint coincides with a large change in drainage area, while the normalized steepness remains nearly constant. We revised the manuscript text to make this interpretation explicit (new Line 346–348):

There are also knickpoints that do not correspond to lithologic boundaries. The knickpoint in the middle Sangasawa corresponds to a significant change in the drainage area of the river. The lithologic boundary between the granodiorite and sandstone is located about 120 m downstream of this knickpoint.

Comment 2: Knickpoint genesis

Reviewer's comments

Although this paper isn ' t about the geomorphic history of the Abukuma basin (and that is fine!), some explanation is needed about the knickpoint origins in these profiles, especially since the authors are asserting that they are not controlled by differences in bedrock strength. The chi-plot analysis does not show corresponding knickpoints in different tributaries (lines 294-296), so the knickpoints are inconsistent with a watershed-wide base level control. Are there upstream controls? Or is it indeed lithologic variation? This point needs to be explained in the discussion section.

Authors' response

Thank you for this suggestion. We agree that a brief explanation of plausible knickpoint origins is necessary, particularly because our main conclusion is that local bedrock strength is not the primary control on channel profile shape. In the revised Discussion, we therefore added a paragraph outlining likely causes of the observed knickpoints.

Because the chi-plot analysis does not indicate systematically corresponding knickpoints among tributaries, a single basin-wide base-level forcing is unlikely to explain all knickpoints. We interpret that (i) some knickpoints may be influenced by upstream controls such as spatial variations in drainage area and sediment supply, and (ii) some reaches may be locally transient (non-equilibrium) due to localized perturbations. For example, the knickpoint in the upper reaches of Takinosawa could not be fully evaluated because that specific upstream site has not been surveyed in detail; however, the presence of a fault cannot be ruled out, which could locally modify rock uplift rate or channel conditions. We added the following sentence to the revised manuscript (new Line 471):

The presence of these minor knickpoints therefore suggests that parts of the river profiles may locally deviate from equilibrium.

We also revised the surrounding text to ensure that the interpretation is framed as plausible mechanisms rather than definitive claims, given the limited constraints on knickpoint age and migration.

Line-by-line comments

Line 21–23 : unclear what you mean here

We revised the sentence as follows:

These erosional processes are recorded in river longitudinal profiles; therefore, researchers have increasingly used river longitudinal profiles to reconstruct signals of past climate change and crustal uplift...

Line 42: wording

We revised the sentence as follows:

Although there are several attempts in the field to measure the sediment cover ratio under fair-weather conditions...

Line 49-50 : citations needed – you say “relatively few studies”, implying there are some which should be cited here

Thank you. We agree that this statement requires supporting citations.In the revised manuscript, we added representative references documenting existing studies.

Line 85: clarify whether there is any evidence for variation in uplift rate within the study area specifically

Thank you. We revised the manuscript to clarify what is known (and unknown) about spatial variations in rock uplift rate within the study area.

Where direct evidence is limited, we explicitly state this limitation and avoid over-interpreting spatial variability.

Comment for figures

Figure 1: show the mapped stream network and location of major faults

We added the mapped stream network and the locations of major faults to Fig. 1 in the revised manuscript.

Figure 5: it would be useful to show the lithology along the channel profile on this chi plot as well, especially considering the discussion point that the transition from sandstone to granodiorite in 2 of the rivers might explain their different lines in this figure.

Thank you. We revised the figure to include lithology information along the chi plot and updated the caption accordingly (Fig. 5).

Reply to Reviewer 4, Professor Gary Parker

Thank you very much for your thoughtful comments. Our replies to your comments are as follows. We will revise the manuscript to incorporate all of these discussions.

Comment 1: Cover factor

Reviewer's comments

This point concerns the SFDM model. This model depends on the value of cover factor Pc, which must be between the values of 0 and 1 for incision (Eq. 10). Cover factor Pc is related to volume bedload transport rate per unit width qs through Eq. 11, and qs is in turn related to erosion rate E(x) via Eq. 16. The predictor for slope at steady state (E = U) is then Eq. 16. I can ' t see through this analysis to determine if indeed Pc is between 0 and 1, and if it shows some consistent pattern of variation downstream. I think that material needs to be added to the manuscript in this regard. The issue could be clarified by plotting predicted Pc versus streamwise distance along with lithology type on diagrams corresponding to Figure 4.

Author's comments

We added a figure (as new Fig. 10) showing the value of the cover ratio at each place. Cover ratio is calculated using Eq. 11. From this figure, it is obvious that cover ratio depends on the bedrock strength.

Line-by-line comments

Thank you for your comments on grammar. The almost grammatical issues you pointed out have been revised accordingly.

Line 42: grammar comments

We revised it as follows:

In new line 56, *Although there are several attempts in the field to measure the sediment cover ratio under fair-weather conditions ....*

In new line 57, *... incision episodes occur ...*

Line 156, 166: Might want to cite a reference

We added a citation of Chatanantavet and Parker (2009).

0.0.1 Line 183: according to type

We revised the manuscript.

Line 195: units?

We add units of $E_r, E_s, K_r, K_{\text{sed}}, q_{\text{w,yr}}, H, H_*$.

Line 271: how obtained?

These parameters D_0 and α_{d} were estimated from the mean of the measured values using the least-squares method. We added

Line 332:The data itself may be insufficient for such a fine resolution.

We agree. In future work, we need to consider a way of measuring grain size in a fine resolution.

Line 336: types

We revised the manuscript.

Line 348: "$\cdots$these smooth river profiles$\cdots$" could be confusing, as it might be thought that the statement refers to the smoothness of the bed. I have suggested alternative wording "$\cdots$the smoothness of the longitudinal profiles of these rivers, in which clear breaks corresponding to a change in lithology are not seen,$\cdots$".

We revised it following your comment.

Line 349: "Through the cover ratio being higher in soft rocks and lower in hard rocks, erosion is suppressed in soft rock areas$\cdots$" This needs to be shown explicitly in terms of plots of Pc, as noted above.

We added the reference to the plots of Pc.

Figure 8: Notation unclear: the -10 should be an exponent on e, correct?

The -10 was the exponent of 10. We modified the expression of the equation in Fig. 8.