REBUTTAL - Manuscript Egusphere-2025-2926

Date	12 May 2026
Former manuscript title	Fluvial fans in the Himalayan Terai: A gradual shift of the Karnali River from double to single branch
Revised manuscript title	A gradual shift of channel dominance on the Karnali fluvial fan in the Himalayan Terai
Authors	Kshitiz Gautam et al.

Dear Associate Editor, Dear Prof. Dr. Fan,
We thank you for the opportunity to respond to the reviewer's comments and update our manuscript. We have updated our manuscript accordingly. In this document, we have structured the response in easy-to-follow sequence as recommended:

a) Comments from referees (in **Black**)
b) Author's response (in **Blue**)
c) Author's changes in manuscript (in **Green**)

Additionally, we would like to mention that apart from response to reviewer's comments, we have added and adjusted phrasing and sequence of some texts to enhance the fluency of the manuscript.

REFEREE 1

General comments

The authors present a well-crafted, well-written investigation of recent changes to two fans of the Himalayan Terai region, with specific focus on the evolution of the Karnali River from a two-branch to a single-branch system. The topic is of considerable interest, and the manuscript is in pretty good shape – I found only a very few minor issues to comment on throughout.

Nonetheless, I think that the authors need to do a better job on two aspects of the study. First, their study would be clearer and make more sense to the reader if it was presented as a hypothesis-driven investigation. Second, the available data to test their leading hypothesis (which they present as a "finding" in the discussion) is quite limited, and I think they should explicitly acknowledge that additional work would be desirable to provide a more comprehensive test.

On the first point: the introduction reads as if the authors are going to just look at some data and see if anything interesting emerges. But I am sure that they had some specific ideas about why a fan system could evolve from a 2-channel to a single-channel system, and that these ideas guided their data collection. The paper would be much improved if specific hypotheses were offered at the outset, and the data collection effort was presented as an effort to test these hypotheses. As written, the author's main conclusion is presented first at the end of the Results section, which is really too late to introduce this. Better to see specific hypotheses at the beginning of the paper, and then explain which of these hypotheses are supported by the author's data.

On the second point: the only real support available that I could discern for the idea that one of the channels was "plugged" by coarse bed material in 2009 is the large grain size at the outside of a bend near the head of the fan. This seems like minimal support to me: ideally some evidence of the deposit itself might be available, or some other supporting evidence. I would like the authors to either more clearly summarize the existing evidence (in case I have missed something), and then discuss additional evidence would could (should?) be collected before accepting this working

hypothesis (e.g., before and after topographic surveys, field mapping of the coarse deposit from 2009, and so on). This discussion would strengthen the scientific impact of the author's work.

These suggestions are important, and perhaps constitute a significant revision of the presentation, but should be easily accomplished and really only a minor effort for the authors. I have indicated major revisions, but I think they will be easily accomplished and the present manuscript greatly improved as a result.

Jim Pizzuto
Dept. of Earth Sciences (retired)
University of Delaware, USA

Thank you for your constructive feedback. We have repeated the two main comments below, and our response is listed in blue.

Comment 1.1

On the first point: the introduction reads as if the authors are going to just look at some data and see if anything interesting emerges. But I am sure that they had some specific ideas about why a fan system could evolve from a 2-channel to a single-channel system, and that these ideas guided their data collection. The paper would be much improved if specific hypotheses were offered at the outset, and the data collection effort was presented as an effort to test these hypotheses. As written, the author's main conclusion is presented first at the end of the Results section, which is really too late to introduce this. Better to see specific hypotheses at the beginning of the paper, and then explain which of these hypotheses are supported by the author's data.

Our response

Observations on the gradually declining discharge in the eastern Geruwa branch on the fluvial fan of the Karnali river system have motivated this study. The research question that our study aimed to answer is the following:

> *What are the causes of the flow partitioning at the Chisapani river bifurcation on the Karnali fluvial fan to increasingly disfavor its eastern Geruwa branch since 2009?*

To answer this question, we formulated the following subquestions:

A. How has the flow partitioning at the Chisapani bifurcation on the Karnali fluvial fan developed over the past centuries?
B. How do the hydrogeomorphic characteristics of the Karnali fluvial fan compare to those of other fluvial fans in the Himalayan foothills, in particular the Koshi fluvial fan in eastern Nepal?*
C. Does the monsoon-dominated hydrograph at Chisapani show deviations from its regular variability?
D. What role have water intakes had on the flow partitioning between the two branches?
E. What role have the embankments along the two branches of the fluvial fan had on the flow partitioning?
F. What role has the elevated land to the east of the Karnali River and downstream of the Chisapani bifurcation had on the flow partitioning?
G. What can explain the gradual decline of the flow discharge into the Geruwa branch since the 2009 season?

*We have chosen for a comparison of the Karnali and Koshi fluvial fans as the latter is one of the most studied systems in this region.

Our data collection has been guided by the above questions rather than by one or more hypotheses. We will adjust the Introduction section to include this information.

Related changes to the manuscript
Line 51-57: We have added literature relevant to our analysis.
Line 74-78: We have explained and included our research questions.

Comment 1.2
On the second point: the only real support available that I could discern for the idea that one of the channels was "plugged" by coarse bed material in 2009 is the large grain size at the outside of a bend near the head of the fan. This seems like minimal support to me: ideally some evidence of the deposit itself might be available, or some other supporting evidence. I would like the authors to either more clearly summarize the existing evidence (in case I have missed something), and then discuss additional evidence would could (should?) be collected before accepting this working hypothesis (e.g., before and after topographic surveys, field mapping of the coarse deposit from 2009, and so on). This discussion would strengthen the scientific impact of the author's work.

Our response
Unfortunately, our domain of interest is a protected and data-scarce area, and no in-situ topographic surveys or historical surface sediment samples are available. Following your suggestion, we explored remotely sensed data in more detail. Specifically, we analyzed satellite imagery and global DEM datasets from before and after 2009. We used the SRTM 30 m Global DEM (representing surface elevations around 2000) and the Copernicus 30 m Global DEM (composite data from 2011–2014, with most coverage for the Karnali fluvial fan dating from 2011). To our knowledge, these are the only elevation datasets available for this area.

In addition to DEM data, we have analyzed Landsat images from 2000, 2009 (before the monsoon season), and 2011 to extract land cover and stream network information, both under low discharge conditions at Chisapani (Figure 1 in this document).

In 2000, the Geruwa branch carried a significant portion of the discharge, and under low flow conditions at least two channels supplied water to the eastern Geruwa branch from an outer bend at Dolphin Point in the upstream Karnali River (Figure 1A). By 2009 (yet before the double monsoon peak), a new eastern channel had formed near the eastern fan boundary (Figure 1B). In 2011, this eastern channel supplying water to the Geruwa branch had ceased to exist (Figure 1C). Under low flow conditions, only one channel supplied water to the Geruwa branch, and it had narrowed since 2009. As a result, a large part of the Chisapani discharge was transported through the Kauriala branch.

The disappearance and decline of the eastern channels between 2009 and 2011 reflect the decline of river discharge into the Geruwa branch, which seems to be associated with sediment deposition in the channel taking off from the outer bend at Dolphin point.

We have computed the difference in surface elevation between the composite Copernicus DEM (2011-2014) and the SRTM DEM (2000) (Figure 1D-E). In 2011 the upstream part of the Geruwa branch had become elevated compared to 2000 (Figure 1E). Our domain of interest is unvegetated, which implies that DEM surface elevation data reflecting canopy elevation for vegetated areas does not affect estimates. Sediment deposition between 2011-2014 and 2000 and the resulting elevation difference across the upstream region of the Geruwa branch support our suggestion that sediment deposition has gradually restricted the water discharge into the Geruwa branch.

In addition, we have analyzed grain size distributions of the bed surface sediment from our bed surface images. To this end, we have determined surface grain-size distributions from images taken at various locations along the fluvial fan (Figure 2) using a python-based tool called *Segmenteverygrain* (Sylvester et al., 2025). The results indicate that the surface sediment across the

outer bend at Dolphin Point and the upper part of the Geruwa branch consists of a large amount of boulders compared to other locations. This supports our suggestion that the sediment deposited over the upstream part of the Geruwa branch was relatively coarse.

The above increase in bed level and estimates of surface grain size with the surface fraction content of boulders across the upstream end of the Geruwa branch underline our hypothesis that a self-reinforcing mechanism was triggered

a. where boulders carried during peak flow discharge were deposited right downstream of the Karnali outer bend across the upstream portion of the Geruwa branch;
b. deposited boulders, unable to move further downstream under these peak discharges, increased river bed level across this upstream part of the Geruwa branch;
c. this subsequently gradually reduced the flow entering this branch;
d. this reduced flow enhanced further boulder deposition in subsequent flood seasons.

Related changes to the manuscript
Line 149-161: We have described the methodology associated with these additional analyses.
Line 259-276: Additional analysis on bed level changes (Figure 8 in the revised manuscript) and grain-size distribution (Figure 10 in the revised manuscript).
Line 277-301: We have added a description of the conceptual framework and schematic for the hypothesis on the closure of the Geruwa branch (Figure 11 in the revised manuscript) with relevant literature.

Other comments

Comment 1.3
Line 20. Should probably cite Figure 1 somewhere in this paragraph.
Related changes to the manuscript
Line 22: We have now cited Figure 1.

Comment 1.4
Line 51. In the paragraph above on anthropogenic stresses, land-use is not mentioned. Perhaps it should be included in the paragraph above, or, if it is not important here, not mentioned at all...???
Related changes to the manuscript
Line 65-67: We have now mentioned the topic of land use and refer to associated literature.

Comment 1.5
Line 67. Include: Section 6, which presumably interprets information from the previous sections and leads to the conclusions of Section 7.
Related changes to the manuscript
Line 91: We have now referred to Sections 6 and 7.

Comment 1.6
Introduction, general comment. I think it would be helpful to present some specific hypotheses to test. This would provide context for the methods. Otherwise, why investigate flow characteristics, dynamics, channel properties, and so on? Presumably these investigations are designed to answer specific questions derived from the authors' hypotheses....and the reader should know what these questions and hypotheses are.
We refer to our response to Comment 1.1.
Related changes to the manuscript
We refer to our changes in the manuscript for comment 1.1.

Comment 1.7
Line 208. The hypothesis to be tested should be presented in the introduction, and specific methods available to test it should be outlined in the methods. Evidence from grain size only is somewhat weak, it seems to me. Is the hypothesis supported by topography analyses, or are these data insufficient or unavailable?
We refer to our response to Comments 1.1 and 1.2.
Related changes to the manuscript
We refer to our changes in the manuscript for comment 1.1 and 1.2.

Comment 1.8
Line 215. But little direct evidence besides grain size has been presented for this. I think it remains a "working hypothesis", rather than a "finding". This may be semantics, but nonetheless important.
We refer to our response to Comment 1.2.
Related changes to the manuscript
We refer to our changes in the manuscript for comment 1.2.

REFEREE 2

General comments
This manuscript investigates the gradual transition of the Karnali River fluvial fan system from a long-lived double-branch configuration to a single dominant channel. The study addresses an important and timely topic in fluvial geomorphology and river–fan dynamics, particularly in large Himalayan rivers where natural processes still dominate over engineering control. Overall, the manuscript is well written, and presents a possible explanation for Karnali River based on multiple datasets. However, I still have two major questions that need to be addressed:

1. The paragraphs are very short and lack clear logical flow, making the manuscript feel like a compilation of disconnected information. I suggest restructuring the paper around a coherent framework, such as scientific hypothesis → evidence → discussion → conclusion. Meanwhile, the Discussion section should be strengthened with more mechanistic explanations, as the current presentation of information is somewhat confusing.

2. The authors suggest that the river shift may be related to the 2009 monsoon season. Such as heavy 2009 monsoon season formed a sediment ‘plug’ in the upstream reach of the Geruwa branch. I think that this evidence appears relatively weak. Could the authors provide additional evidence to support this hypothesis?"

In addition to the above, there are some minor issues that I also suggest the authors consider.
Line 16-25. Could more recent literature be added here to highlight the significance of your study?
Line 40-51. At interannual timescales, human activities and climate change are the dominant drivers of river change; however, in certain regions or during extreme events, abrupt tectonic activity (such as earthquakes) and short-term sea-level fluctuations can also significantly influence river systems.
Line 61. Which natural factors are included here? These have not been specified earlier in the manuscript.
Line 143-144. This sentence should appear in the Discussion section.
Line 156-159 This is just a simple description; I would rather know more about the underlying mechanisms.
Line 187 “Sinuosity and Braiding Indices and Sinuosity Dynamics for”. Should the first letter be capitalized?

Line 206-208 This is a good hypothesis, but how can it be tested? Is there any new evidence to support it?
Line 210-249 the Discussion section is overall too simplistic, merely ruling out some possible factors. I hope you can build on your results to provide more mechanistic insights.
Line 250-260. Could this be written as one or two paragraphs? There's no need for so many separate paragraphs.

Thank you for your constructive feedback. We have repeated these comments below and addressed them separately.

Comment 2.1
The paragraphs are very short and lack clear logical flow, making the manuscript feel like a compilation of disconnected information. I suggest restructuring the paper around a coherent framework, such as scientific hypothesis → evidence → discussion → conclusion. Meanwhile, the Discussion section should be strengthened with more mechanistic explanations, as the current presentation of information is somewhat confusing.
Regarding the reviewer's suggestions on manuscript structure and hypothesis, we refer to our response to Comment 1.1. We have included more mechanistic explanations in the Discussion section.
Related changes to the manuscript
We refer to changes in the manuscript for comment 1.1.
Line 303-353: We have updated the discussion section.

Comment 2.2
The authors suggest that the river shift may be related to the 2009 monsoon season. Such as heavy 2009 monsoon season formed a sediment 'plug' in the upstream reach of the Geruwa branch. I think that this evidence appears relatively weak. Could the authors provide additional evidence to support this hypothesis?"
Following both this reviewer and Reviewer 1's comments, we have added some analyses that provide more clarity and support for our concluding hypothesis. For this, we refer to our response to Comment 1.2.
Related changes to the manuscript
We refer to changes in the manuscript for comment 1.2.

Comment 2.3
Line 16-25. Could more recent literature be added here to highlight the significance of your study?
Related changes to the manuscript
Line 51-57: We have added literature highlighting the relevance of the study.
Line 42-50: We have rearranged information to highlight relevance .

Comment 2.4
Line 40-51. At interannual timescales, human activities and climate change are the dominant drivers of river change; however, in certain regions or during extreme events, abrupt tectonic activity (such as earthquakes) and short-term sea-level fluctuations can also significantly influence river systems.
Thank you for your comment.
Related changes to the manuscript
Line 54-57: We have included the relevant information and literature on extreme events such as flood events and earthquakes.

Comment 2.5
Line 61. Which natural factors are included here? These have not been specified earlier in the manuscript.
These natural factors include hydrology, topography, and geology.
Related changes to the manuscript
Line 51-57: We have specified what the relevant natural factors are.

Comment 2.6
Line 143-144. This sentence should appear in the Discussion section.
Related changes to the manuscript
Line 330-331: We have moved the information to the Discussion section.

Comment 2.7
Line 156-159 This is just a simple description; I would rather know more about the underlying mechanisms.

We have expanded on this topic and now discuss remaining challenges:

1. *Nonlinear morphodynamic feedbacks*

The stability of river bifurcations is controlled by strongly nonlinear feedbacks between flow partitioning, sediment transport, and channel bed level. Perturbations in discharge or sediment supply can trigger self-reinforcing adjustments (e.g. change in channel dominance), making bifurcation behavior sensitive to boundary conditions and initial states (Blom et al., 2024; Bolla Pittaluga et al., 2003; Wang et al., 1995).

2. *Scale dependence and long adjustment times*

Bifurcation dynamics emerge from processes acting across multiple spatial and temporal scales, from bar-scale sediment sorting to basin-scale sediment supply and base-level change. It remains difficult to distinguish transient responses from long-term stable configurations as bed level adjustments often span decades to centuries (Bolla Pittaluga et al., 2015; Kleinhans et al., 2008).

3. *Anthropogenic modification of bifurcation dynamics*

Engineering interventions such as dams, groynes, dredging, and bank fixation can modify flow and sediment regimes at bifurcations. Consequently, observed stability or instability may reflect active management rather than intrinsic morphodynamic behavior, complicating the interpretation of (in)stability in both natural and regulated river systems (Blom et al., 2024, Kleinhans et al., 2013; Mendoza et al., 2019).

Related changes to the manuscript
Line 336-344: We have included the above explanations in the manuscript.
Line 303-353: Relevant mechanisms and supporting literature are now addressed in the Discussion section.

Comment 2.8
Line 187 “Sinuosity and Braiding Indices and Sinuosity Dynamics for”. Should the first letter be capitalized?
Not necessarily, yet this is done to clarify that we use the first letters of the indices also as acronyms.
Related changes to the manuscript
We would prefer to keep capitalization.

Comment 2.9
Line 206-208 This is a good hypothesis, but how can it be tested? Is there any new evidence to support it?
Thank you for your comment. We refer to our response to Comment 1.2 for added analyses. We expect that future research will shed more light on the mechanisms and the validity of the hypothesis through numerical modelling.
Related changes to the manuscript
We refer to changes in the manuscript for comment 1.2 for additional evidence to support our hypothesis.
Line 352-353, 377-380: Outlook for future testing of the hypothesis.

Comment 2.10
Line 210-249 the Discussion section is overall too simplistic, merely ruling out some possible factors. I hope you can build on your results to provide more mechanistic insights.
We refer to our response to Comment 1.2: we have added some analyses.
Related changes to the manuscript
We refer to changes in the manuscript for comment 1.2 for additional analysis.
Line 303-353: We have updated the Discussion section.

Comment 2.11
Line 250-260. Could this be written as one or two paragraphs? There's no need for so many separate paragraphs.
Related changes to the manuscript
We have adjusted the Conclusions section accordingly.

REFEREE 3 (COMMUNITY COMMENT)

General Comments
The article investigates the evolution of the Karnali fan over the past decades using data from historical surveys and remote sensing imagery. The authors discuss the implications of the fan evolution on ecosystems, which is a novel and important aspect. Furthermore, the authors present the first article (that I am aware of), which uses historical maps of the Karnali fan dating back to the 18th century, and hence adds to our understanding of the fan.

Main criticism
I have one major concern regarding the claim that the Karnali River transitioned from a multi-branch river to a single-branch river following the 2009 monsoon season. This statement is repeated from the title to the conclusion, but I cannot see sufficient evidence to support this transition. I am not convinced that the Karnali is a single-branch river for several reasons:

- Satellite images after 2009 show that the Eastern branch still carries water and is, thus, still an active channel. Please see the attached images of Oct. 2014-2021 as an example.
- Figure 6a shows that the Western branch had a lower water surface area between 2000-2009 than the Eastern branch between 2010-2020. It is not logical to argue that it transitioned to a single-branch river after 2009, when the low-flow partitioning is more balanced after 2009 than before.
- The analysis is based on imagery during low-flow conditions. Dingle (https://doi.org/10.1130/G46909.1 supplements) shows that normal to high flows overflow the plug, and that the Eastern branch still receives water during low flows despite the plug. Hence, the Eastern branch is active, and low flow conditions are insufficient to determine the activity of a branch.

Minor aspects

- Branch characteristics: The Eastern and Western branches have different channel characteristics, whereas the Eastern branch is composed of more but narrower braid channels. These narrower channels are more difficult to classify from 30m satellite imagery, which likely leads to an underestimation of the water surface area in the Eastern branch. It may be worth addressing this limitation in the discussion.
- Potential future evolution: The authors argue that with human interventions, the likelihood of future channel switches decreases (lines 245ff). I believe that this section needs more context. Dingle, 2017 (doi:10.1038/nature22039) shows that the gravel supply to the fan originates from the Siwaliks. The fan will not be blocked from its gravel supply unless a hydropower station is constructed at the mountain gauge. Upstream stations may reduce the flow rate (and hence transport capacity), which may lead to a decrease in gravel supply. However, increasing frequency and magnitude of intense rainfall may counteract and lead to increasing gravel supply (e.g. https://doi.org/10.3126/jalawaayu.v1i1.36448)

Additional thoughts

- 2009 monsoon season: There is a book chapter on bifurcation switches in the Karnali fan by C. Cload called monsoon-driven changes to river bifurcations in Nepal (DOI: 10.1201/9781003475378-6), which may be of interest.
- It may be worth assessing the variation of water surface area with flow (e.g. comparing multiple images during a year). This may be beyond the scope of the study, but I believe it would add depth to the article because it provides evidence about how sensitive medium to high flows are to bifurcation changes. Without any consideration of medium to high flows in the analysis, it should be clearly stated that the findings relate to low-flow conditions only.

I disagree with the authors that the Karnali transitioned to a single-branch channel and feel that the narrative of the article needs to be adjusted. Nonetheless, I believe that it is a valuable article because it improves our understanding of the channel evolution during low flow conditions, which prevails for most of the year and is important for ecosystems and communities. I would further like to highlight that the figure presentation is excellent.
Ivo Pink

We appreciate the feedback and thoughtful suggestions of Reviewer 3. We have addressed the reviewer's concerns below.

Comment 3.1

I have one major concern regarding the claim that the Karnali River transitioned from a multi-branch river to a single-branch river following the 2009 monsoon season. This statement is repeated from the title to the conclusion, but I cannot see sufficient evidence to support this transition. I am not convinced that the Karnali is a single-branch river for several reasons:

- Satellite images after 2009 show that the Eastern branch still carries water and is, thus, still an active channel. Please see the attached images of Oct. 2014-2021 as an example.
- Figure 6a shows that the Western branch had a lower water surface area between 2000-2009 than the Eastern branch between 2010-2020. It is not logical to argue that it transitioned to a single-branch river after 2009, when the low-flow partitioning is more balanced after 2009 than before.
- The analysis is based on imagery during low-flow conditions. Dingle (https://doi.org/10.1130/G46909.1 supplements) shows that normal to high flows overflow the plug, and that the Eastern branch still receives water during low flows despite the plug. Hence, the Eastern branch is active, and low flow conditions are insufficient to determine the activity of a branch.

Our response

We would like to emphasize that we have not intended to claim that the Karnali River over its fan has turned into a single-channel system. Rather, we argue that the system shows a gradual shift toward a system where one channel is dominant and that this shift is still ongoing. Please also note this nuance from the second part of the manuscript title: "A gradual shift of the Karnali River from double to single branch".

Our remote sensing analysis indeed considers low-flow conditions only, which is due to cloud cover during monsoon conditions. We are aware of and agree with the point that such analysis under low flow conditions has limitations and that extension to high-flow conditions needs to be done with care. We have analyzed newly available measured discharge data that allows us to extend our study to monsoon conditions.

In 2023, we installed two water-level monitoring stations in the downstream reaches of the Kauriala and Geruwa branches, approximately 30 km downstream of Chisapani. We have used these data to determine the daily discharge in the two branches over the period between November 2023 and March 2025. In addition, we analyzed Karnali discharge data at Chisapani (located right upstream of the Karnali bifurcation) over the same period. These discharge data at Chisapani were made available to us through the Department of Hydrology and Meteorology, Nepal. We analyzed the ratio of the discharge in the two branches as a function of the Chisapani discharge. We found that the share of discharge in the Geruwa branch ranges from 5% under low flow conditions to 20% under high flow conditions (Figure 3). These data cover high flow as well as low flow conditions.

In August 2017 the Geruwa branch received approximately 50% of the Chisapani discharge during the moderate-monsoon discharge of about 4500 m^3/s at Chisapani (Dingle et al., 2020). The fact that, for similar discharges, the Geruwa discharge has decreased from about 50% to 20% of the Chisapani discharge over the period between 2017 and 2023-2025 is an indication that the temporal decrease of the Geruwa branch is not restricted to low-flow conditions but also holds under monsoon conditions.

This supports the fact that the Geruwa discharge is declining not only under low flow but also under moderate-monsoon flows. We emphasize in the revised manuscript that we have no information on temporal changes in discharge partitioning between the branches under more extreme monsoon conditions. Another argument is of a qualitative nature, and relate to data on vegetation cover for the Geruwa floodplain. Bijlmakers et al. (2023) show that, in the Geruwa floodplain, higher peak discharges at Chisapani correspond to increased vegetation removal. This correlation between floodplain reworking with water discharge indicates that grassland cover may be used as a proxy for discharge. Since 2009, grassland cover in the Geruwa floodplain has increased with time at the expense of bare surface sediment and water cover. We attribute this increase of grassland cover to the temporal reduction of hydro-morphodynamic activity or surface sediment reworking in the Geruwa floodplain. We have observed this temporal increase in grassland cover during our field monitoring campaigns, and these observations have been confirmed in interviews with employees of the Bardia National Park authority and residents. They have reported a temporal decline in flooded areas and increase of vegetation cover in the Geruwa floodplain.

We have added the above extra analyses to the manuscript, and have expanded the Discussion section accordingly.

Related changes to the manuscript

We have rephrased texts, including the title, throughout the manuscript to clarify the gradual nature of the shift toward a single-channel system.

Line 199-226 and Appendix A: Provides explanation on decreasing high and low flows in the Geruwa.

Comment 3.2

- Branch characteristics: The Eastern and Western branches have different channel characteristics, whereas the Eastern branch is composed of more but narrower braid channels. These narrower channels are more difficult to classify from 30m satellite imagery, which likely leads to an underestimation of the water surface area in the Eastern branch. It may be worth addressing this limitation in the discussion.
- Potential future evolution: The authors argue that with human interventions, the likelihood of future channel switches decreases (lines 245ff). I believe that this section needs more context. Dingle, 2017 (doi:10.1038/nature22039) shows that the gravel supply to the fan originates from the Siwaliks. The fan will not be blocked from its gravel supply unless a hydropower station is constructed at the mountain gauge. Upstream stations may reduce the flow rate (and hence transport capacity), which may lead to a decrease in gravel supply. However, increasing frequency and magnitude of intense rainfall may counteract and lead to increasing gravel supply (e.g. https://doi.org/10.3126/jalawaayu.v1i1.36448)

Our response

We have now mentioned the difficulties in the interpretation of water cover from satellite images for narrow channels, and we now address the role of human interventions more clearly.

Related changes to the manuscript

Lines 125-127: Difficulties in the interpretation of water cover from satellite images for narrow channels.
Line 332-335: Potential future evolution under increased hydrological extremes
Figure 4 sheds light on the potential hydropower

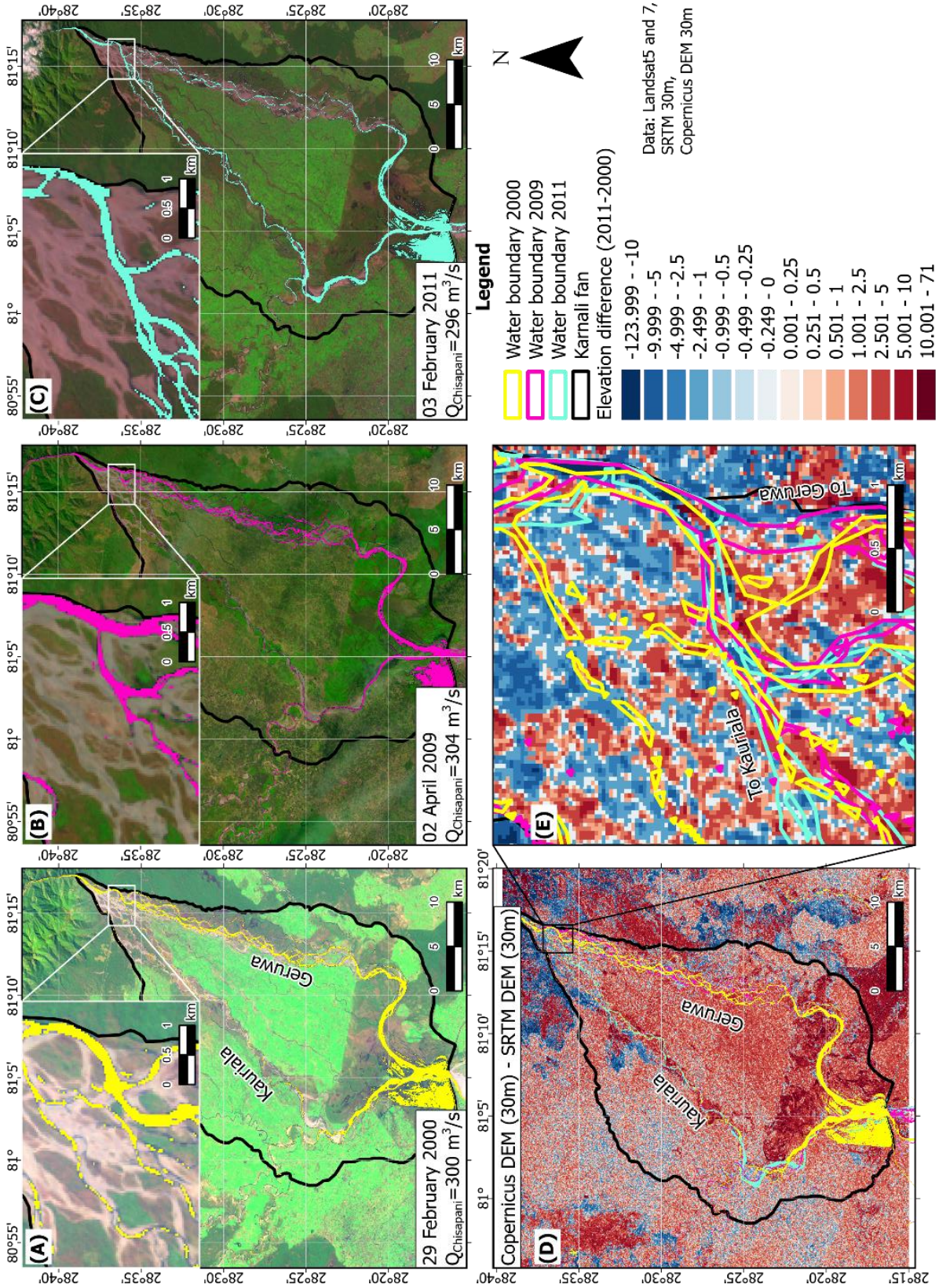


Figure 1: Wetted surface area indicating river channels across the Karnali fluvial fan in the years (A) 2000, (B) 2009, and (C) 2011. Data relate to low-flow conditions: 300 m^3/s in 2000, 304 m^3/s in 2009, and 296 m^3/s in 2011. The inset at the top-left corner of each map shows the Dolphin Point bend right upstream of the Geruwa branch, where the flow partitions between the Geruwa and Kauriala branches. (D) Difference in surface elevation across the Karnali fan between 2011-2014 (Copernicus DEM) and 2000 (SRTM DEM); (E) Inset of the elevation difference map at the Dolphin Point region or upstream end of the Geruwa branch with river channel outlines for the years 2000, 2009, and 2011.

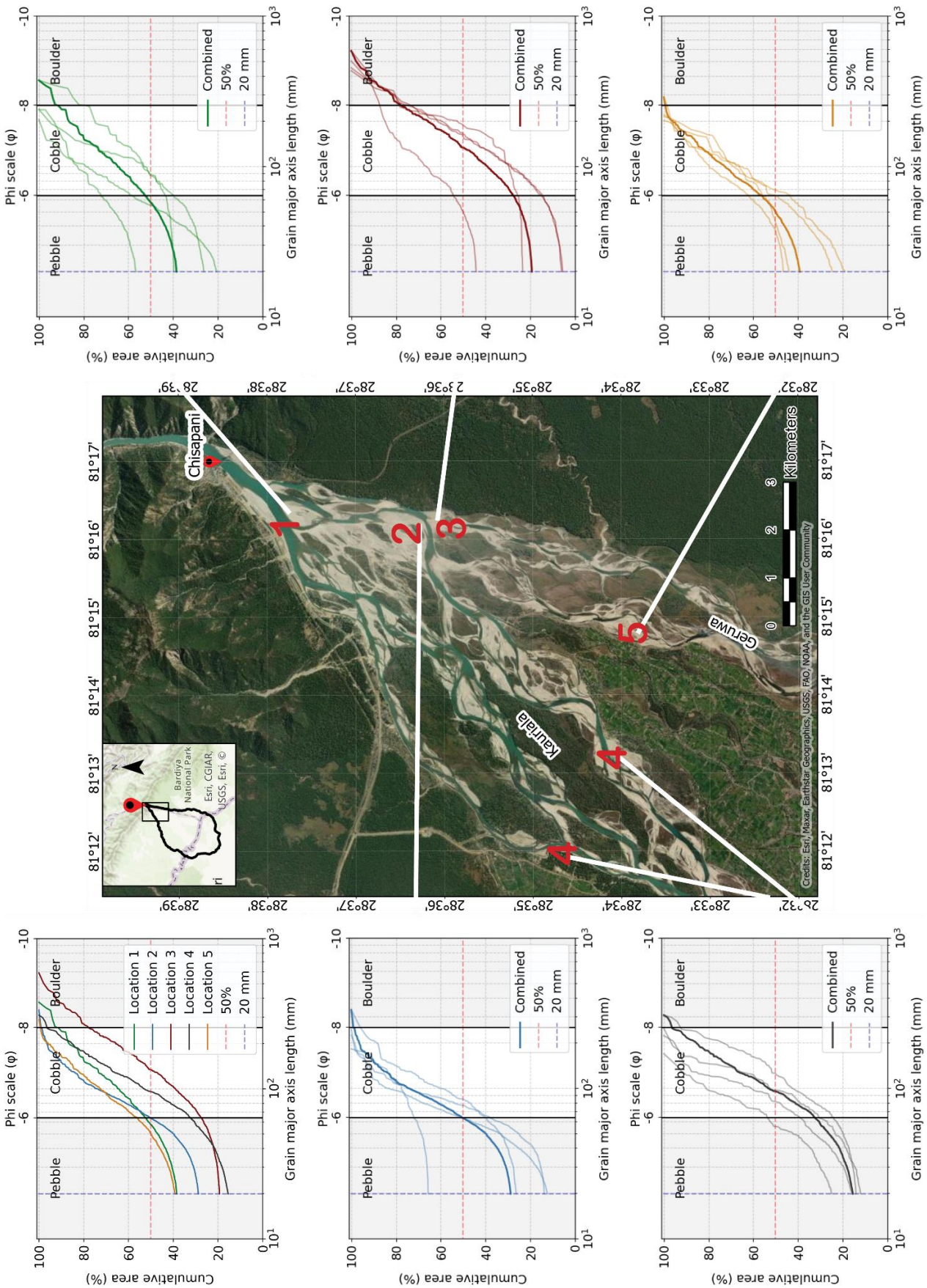


Figure 2: Grain size distributions of surface sediment at various locations across the Karnali fluvial fan, obtained from image analysis. Only grains with a major axis length larger than 20 mm could be distinguished.

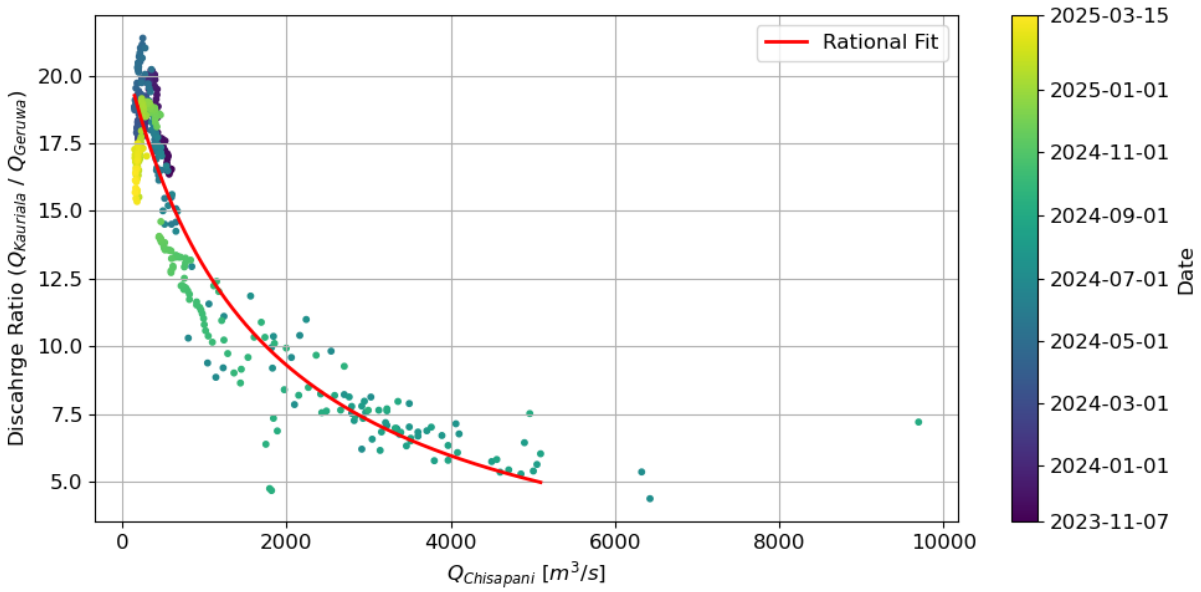


Figure 3: Ratio of Kauriala discharge to Geruwa discharge as a function of the Chisapani discharge. Water discharge in the downstream branches has been computed from time series of measured water level at Sattighat bridge (Kauriala) and Kothiyaghat bridge (Geruwa), both approximately 30 km downstream of Chisapani, measured between 7 Nov 2023 to 15 March 2025. The discharge partitioning between the two branches ranges from about 5% at low flows to about 20% under high flow conditions. Chisapani water discharge over the same period is determined from flow depth data provided by the Department of Hydrology and Meteorology, Nepal. Data includes 2024 monsoon cycle with a peak discharge close to 10,000 m^3/s.

References

Bijlmakers, J., Griffioen, J., and Karssenberg, D.: Environmental drivers of spatio-Temporal dynamics in floodplain vegetation: Grasslands as habitat for megafauna in Bardia National Park (Nepal), Biogeosciences, 20, 1113–1144, https://doi.org/10.5194/bg-20-1113-2023, 2023.

Blom, A., Ylla Arbós, C., Chowdhury, M. K., Doelman, A., Rietkerk, M., and Schielen, R. M. J.: Indications of ongoing noise-tipping of a bifurcating river system, Geophysical Research Letters, 51, e2024GL111846, https://doi.org/10.1029/2024GL111846, 2024.

Bolla Pittaluga, M., Repetto, R., and Tubino, M.: Channel bifurcation in braided rivers: Equilibrium configurations and stability, Water Resources Research, 39, https://doi.org/10.1029/2001WR001112, 2003.

Bolla Pittaluga, M., Coco, G., and Kleinhans, M. G.: A unified framework for stability of channel bifurcations in gravel and sand fluvial systems, Geophysical Research Letters, 42, 7521–7536, https://doi.org/10.1002/2015GL065175, 2015.

Dingle, E. H., Sinclair, H. D., Venditti, J. G., Attal, M., Kinnaird, T. C., Creed, M., Quick, L., Nittrouer, J. A., and Gautam, D.: Sediment dynamics across gravel-sand transitions: Implications for river stability and floodplain recycling, Geology, 48, 468–472, https://doi.org/10.1130/G46909.1, 2020.

Kleinhans, M. G., Jagers, H. R. A., Mosselman, E., and Sloff, C. J.: Bifurcation dynamics and avulsion duration in meandering rivers by one-dimensional and three-dimensional models, Water Resources Research, 44, https://doi.org/10.1029/2007WR005912, 2008.

Kleinhans, M. G., Ferguson, R. I., Lane, S. N., and Hardy, R. J.: Splitting rivers at their seams: bifurcations and avulsion, Earth Surface Processes and Landforms, 38, 47–61, https://doi.org/10.1002/esp.3268, 2013.

Mendoza, A., Soto-Cortes, G., Priego-Hernandez, G., and Rivera-Trejo, F.: Historical description of the morphology and hydraulic behavior of a bifurcation in the lowlands of the Grijalva River Basin, Mexico, CATENA, 176, 343–351, https://doi.org/10.1016/j.catena.2019.01.033, 2019.

Sylvester, Z., Stockli, D. F., Howes, N., Roberts, K., Malkowski, M. A., Poros, Z., Martindale, R. C., and Bai, W.: Segmenteverygrain: A Python module for segmentation of grains in images, Journal of Open Source Software, 10, 7953, https://doi.org/10.21105/joss.07953, 2025.

Wang, Z. B., De Vries, M., Fokkink, R. J., and Langerak, A.: Stability of river bifurcations in ID morphodynamic models, Journal of Hydraulic Research, 33, 739–750, https://doi.org/10.1080/00221689509498549, 1995.