Authors' Response to Reviews of

Geomorphological and hydrological controls on sediment export in earthquake-affected catchments in the Nepal Himalaya

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Earth Surface Dynamics,

RC: ***Reviewers' Comment***, AR: Authors' Response, ☐ Manuscript Text

1. Introduction

We are grateful to the three reviewers for their detailed, constructive and encouraging comments. Below, we respond to their comments point by point.

2. Reviewer #1

RC: ***This paper asks a very straightforward question regarding what the legacy of earthquake triggered landslides are on the mobilization of sediment in river channels adjacent to landslides in the years after an earthquake. Recent events in Taiwan, China, and New Zealand all highlight the potential for earthquake-triggered landslide debris to cascade into river systems and dramatically change river morphology. This study exploits the 2015 Gorkha (Nepal) earthquake and examines changes in valley bottom gravel storage for two rivers, the Bhote Kosi and the Melachi-Indrwati, as a proxy for sediment storage.***

RC: ***Notably, the study shows that earthquake-triggered landslides don't influence valley bottom sediment storage in a clear way, despite the fact that many of the landslides are adjacent to river channels. Moreover, a huge flood event in 2021, which did influence the valley bottom sediment storage in a clear and dramatic way on one of the two rivers, transported sediment that was pretty clearly not sourced from earthquake triggered landslides. Rather, very likely, the legacy of previous landslide dams (and importantly the fine sediment they impound) is likely the dominate source of sediment to river channels.***

RC: ***All of this suggests that how earthquakes influence river channels through the landslides they trigger is a bit more nuanced than has been articulated previously. While the legacy of landslides is important, in this example, the importance derives not from the sediment landslides generate directly (which is generally quite coarse) but rather through the sediment that is impounded behind landslide dams (which is by definition mobile in the river and hence finer).***

RC: ***I love this study because it takes a critical and objective look at an issue that I think many have opinions about but few bother to actually explore with data. I just have a one substantial comment below, and then a minor comment. Well done.***

AR: We thank Reviewer #1 for their very encouraging comments.

RC: ***Figure 4-5 (and text pertaining to): why not simply calculate the width of the exposed gravel orthogonal to the river channel as a function of the distance downstream? Plotting area versus distance is confusing***

because it relies on a discretization, and hence will be more difficult to reproduce in the future. I think a more straightforward thing would be to plot width versus distance, which when numerically integrated in the downstream direction would yield area. There is much less ambiguity about this measurement, and it is much more straightforward to interpret. Also, once you have area, you can easily get to volume with an assumption about aggradation depth.

AR: At the beginning of the study, we mapped individual gravel bars, which are 2D objects, intending to identify zones of sediment input – potentially linked to the Gorkha earthquake – through changes in gravel bar area. It is only when we started analysing the data that it became apparent that seasonal changes in water level would make comparison between years impossible and so the decision was made to instead map the "combined" channel area of water and sediment. We believe that redoing the analysis with width instead of area will constitute a large amount of work (as we would have to design a completely new approach and start from scratch) for limited benefit, as we believe width data would show the same trends and signals as are present in the area data.

We could potentially add labels on the right-hand side of the "gravel area" diagrams displaying an "equivalent channel width" to facilitate comparison with published width data. This "equivalent channel width" would be the gravel bar area divided by the width of the window which, in the main text, is 500 m: this would equal the average channel width over that window, should the channel flow straight through the window, and represent a maximum estimate of the true average channel width, as channels tend to not be perfectly straight. A gravel bar area of 0.2 km^2 would therefore be equivalent to an equivalent channel width of 400 m. However, because increasing channel sinuosity will increase the difference between the equivalent channel width and the true average channel width, we are worried that the information will be misleading. We would have to make this clear in the text.

We have tested the effect of changing window size on the output in the appendix, and chose 500 m as the most appropriate to display reach-scale trends in the context of this study. Narrowing the windows led to increasingly noisy data without information gain. The width data would have exhibited the same problem, and would have had to be averaged over a given distance, so the trends would have been similar.

RC: ***Figure 6. again, to me thinking about changes in width is much easier to interpret than changes in the area of segments that have arbitrary lengths.***

AR: Please see response to comment above.

RC: ***338-344: A similar case was documented in California in the Sierra Nevadas in 2018 and summarized in this study: Collins, Brian D., et al. "Linking mesoscale meteorology with extreme landscape response: Effects of narrow cold frontal rainbands (NCFR)." Journal of Geophysical Research: Earth Surface 125.10 (2020): e2020JF005675. In this work there is weather radar available that demonstrates exactly what you are speculating about here - a local zone of extremely intense precipitation. In the case of the Sierras, the culprit is a Narrow Cold Frontal Rainband (NCFR). I don't know if these also occur commonly in the Himalaya, but regardless, this paper provides support for the basic idea you are proposing so I think it's worth citing.***

AR: We thank Reviewer #1 for pointing us to this helpful study. We have modified the text accordingly: "[...] localised over a 6 km wide zone that runs between multiple small catchments along strike at an elevation of approximately 4500 m (Hobley et al. 2012). Similarly, in 2018, a localised band of extreme precipitation in the Californian Sierra Nevada has been shown to initiate hundreds of landslides, increasing fluvial sediment yield to up to 16-fold relative to the local annual sediment yield (Collins et al. 2020)."

3. Reviewer #2

RC: ***This is a very nice manuscript that tackles an important and topical issue in Earth surface processes – the mobility and fate of coseismic landslide debris after large earthquakes. I read it with interest and I think it will make a good contribution to the journal. The manuscript is well-written and illustrated overall and won't require much work to make it suitable for publication. I do have some comments and queries for the authors, which are detailed in an annotated PDF. I won't repeat all of those here, but will just touch on a couple of the more substantial points.***

AR: We thank Reviewer #2 for their thorough and encouraging comments.

RC: ***Most importantly, I don't think the introduction really motivates the specific study that the authors have carried out. The text to line 42 is pretty much a factual summary of the literature around coseismic and postseismic landsliding and the potential issues associated with that sediment. The authors then pivot to state what they will do, but without having articulated any outstanding issue or problem, it's hard to see what specific knowledge gap they want to fill. I can guess at this, but it would be helpful to make it explicit. This will also help to address another wider point, which is that – as currently written – the manuscript feels like two loosely connected pieces of work: one on changes in gravel area in two specific parts of the Gorkha rupture area, and another on documentation of a particular flood event in the Melamchi Khola in 2021. It's not clear, from the introduction or anywhere else, why the authors have chosen to focus on the Melamchi Khola (as opposed to other events in the two catchments – e.g. 2019 and 2020 flows in the Bhote Kosi), or how that links with the first part of the manuscript. Again, I can guess at the link but it would be good to spell it out for the reader. Line 50 introduces the idea of pre-conditioning, but without explaining what that means, and I think that lack of clarity creates an issue later in the manuscript which I'll come to below.***

AR: Reviewer #2 points out that the manuscript appears to consist of two main themes that are currently only loosely connected: changes in gravel area in catchments affected by the Gorkha earthquake, and exploration of cause of the Melamchi flood event. This study initially aimed to document the impact of sediment pulses (related to coseismic landsliding) on bedload transport, under the assumption/expectation that the Gorkha earthquake would result in channel aggradation on a similar scale to that observed following the 2008 Wenchuan earthquake. The two study catchments were chosen as they were both within an area heavily affected by coseismic landsliding, and therefore were expected to exhibit changes in fluvial sediment transport following the earthquake. It quickly became apparent that there were few clear signals in the mapped combined channel area that could be linked to the Gorkha earthquake. The 2021 Melamchi flood event occurred partway through the study and was therefore not included in the original aims. However, the significant impact it has had on the Melamchi-Indrawati catchment merited the event's inclusion in the study and shifted the focus of the manuscript away from merely exploring the impact (or lack thereof) of Gorkha earthquake coseismic landsliding on fluvial sediment transport, to additionally considering the causes of the Melamchi event, potential links to the Gorkha earthquake, and the role of extreme hydro-geomorphic events in fluvial sediment export. As a result, we have been struggling to integrate the Melamchi event within the motivation of the study: bringing it from the onset would make the reader wonder about the link with the coseismic landslides, but omitting it in introduction would make the reader wonder why this large event suddenly appears in the analysis. We are going to alter the introduction to try to clarify the aims and better integrate the different components of the study, possibly through the introduction of the preconditioning.

RC: ***The methodology is straightforward and clearly documented, although I wondered why aggradation was represented by changes in bare-sediment area over a 0.5 km2 tile, rather than by changes in width at a set of closely-spaced stations. There is some double-counting that goes on (which the authors do acknowledge***

in the SI) with their chosen method. I also wasn't sure why they then needed to apply a smoothing function; if I understand the method correctly, then aren't the areas already smoothed over c. 10 cells centred on each raster cell? Why smooth it again? I also would have liked to see some consideration of how much change in area they would expect to see in confined reaches like those along the Bhote Kosi, which yields 'valley widths' of <100 m over much of their study section. In a lot of places I'm familiar with, there could be considerable aggradation that would not lead to appreciable widening of the visible water + sediment. It's perhaps not surprising, therefore, that there is a broad correlation between areas where sediment area has increased and areas where valley width is larger in Figs 6 and 7 (especially for the Bhote Kosi, where panel b looks like a stretched version of panel d), and not much change visible in between. Do the authors have a sense of how much aggradation could be hidden from this approach?

AR: Please see response to Reviewer #1 on the use of area vs. width.
Regarding the final smoothing step: this step was designed to focus on reach-scale changes and facilitate comparisons between mapping epochs. Without the final additional smoothing step, the figures contained large local spikes in the area data which made it difficult to distinguish the different mapping epochs in Figs. 6 and 7. This is not as big an issue when a single mapping epoch is shown (as Reviewer #2 points out for Fig. 4). Based on our analysis of the data before smoothing, we do not believe that the smoothing obscured important signals, but are happy to provide the unsmoothed data in Appendix if the reviewer believes they are important to show.
Reviewer # 2 correctly points out this bias: for a given amount of aggradation, confined channel reaches will exhibit less discernible widening of visible water and sediment extent. However, even in the most confined reaches, valley walls are rarely steeper than 45 degrees so we believe that aggradation, while not as clear as in wide valley sections, would still be visible. In addition, where local constrictions occur (e.g., gorge with near-vertical walls where aggradation will not be detectable with our method), aggradation does tend to occur upstream of the constriction rather than in the gorge where shear stress will be the greatest (see for example Turzewski et al. - https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JF004778) except if the gorge itself were dammed by a landslide, which would likely lead to detectable changes at the reach scale. We will add text to better highlight this limitation.

RC: ***The analysis of landslide connectivity to the channel network feels like a bit of an add-on, and it's only after the results are shown (lines 375-377) that the rationale is made somewhat more clear – essentially, to see if 'low connectivity' can explain the lack of direct evidence for aggradation. The authors don't really explain what they're judging that against, though, other than a quick reference to the work by Gen Li and colleagues in Wenchuan. What are they looking for? How low does connectivity have to be to fit with the lack of aggradation that they observe? It's not clear why they need to use two different inventories or to repeat the analysis that's already been done by Roback et al. (2018), and Fig 8 isn't really utilised. I've made some suggestions for how this might be better linked with the rest of the manuscript, perhaps by better signposting up front, by taking a similar approach to Li et al. and looking at connectivity as a function of landslide size (since they are making a direct comparison with Wenchuan), and leaving the inventory and buffer size sensitivity analyses out of the main manuscript.***

AR: Again, this relates to better defining the motivation. When our analysis showed little evidence of sediment transport following Gorkha, our first thought was "are the Gorkha landslides connected in these valleys"? And when the Melamchi event happened, we wanted to know if there was any evidence that showed that landslides were better connected in Melamchi than in the Bhote Koshi. We chose different inventories for completeness.
We will remove Figure 8 that is not very helpful indeed and provide a figure showing how the connectivity of the landslides evolves along our two studied rivers, which will have greater relevance to interpreting the signals we detected. Following on previous comments, we will also tighten the motivation of the study and

better integrate/justify the connectivity analysis.

RC: ***I also struggled a bit to understand the unique contribution that is made by section 6, which largely summarises some reports on the Melamchi event. Part of my confusion might link back to the overall point of investigating that event in detail, and so addressing the motivation may help with this query as well. As I understand it, the main contribution that the authors are trying to make here is that there is evidence in satellite imagery for multiple sediment sources upstream of Bremthang, rather than just the failure of a moraine dam in the Pemdang Khola as argued for by some of the previous work. This is based on Fig 10, which shows bare sediment in a number of tributaries feeding into the Melamchi Khola after the event, and the authors interpret this as evidence of sediment excavation from multiple sources (although there's some confusing wording on that point). I agree with them, although Fig 10 could be enlarged and annotated to make that easier for the reader to see. I've also looked at the evidence for that event, and I struggled to see what they were trying to show in the figure, so a reader could be forgiven for not picking it up. If I've missed the point of this section then it would be good to clarify what the unique contribution is that they are trying to make.***

AR: Making our motivations clearer in the introduction would go some way to addressing this comment. The section does summarise existing reports, but we believe it is necessary to do that so the reader can understand the Melamchi event in greater detail, and the debate regarding the triggers of the event, which is relevant to the discussion on the location of the source of sediments and hazards associated with their transport. Additionally, the images that are shown in this section were not included in any of the reports and therefore they do show new evidence, in particular regarding the depth of aggradation. We will clarify the motivation and better justify the need for the information provided in this section. We will also clarify the importance of Figure 10 and annotate the points of interest for the reader.

RC: ***Related to this, the authors then argue that there is no evidence for 'pre-conditioning' of the event by the Gorkha earthquake. Here I think they need to be more explicit about what they mean by pre-conditioning, because this is a term that is used (or mis-used!) in lots of different ways. I think their argument is that the sediment sources visible in their image don't match up with coseismic landslides in the Roback et al. inventory, and therefore the Melamchi event wasn't just caused by remobilisation of coseismic landslide debris. That's fine, and I agree, but they don't actually demonstrate that here in any of the figures; it would be useful to overlay the image with the coseismic inventory just to make that point more clearly. But at the same time, I don't think – on the basis of the evidence that they've shown here – that they can rule out what Maharjan et al. (2021) were referring to as pre-conditioning, as in the weakening of hillslopes by shaking in the Gorkha earthquake, which then failed under heavy rainfall in 2021. The assertion that the hillslopes were weakened by the earthquake and then failed is very hard to test; we looked for that kind of effect in a pair of earthquakes in New Zealand (Parker et al. 2016) and it's really difficult to identify unequivocally. Equally, however, I don't see any evidence from the authors that that has NOT happened. So I'd suggest that this section, and the conclusions, be reframed in places to link more closely with the evidence that they've shown. Similarly, the assertion in line 449 that Melamchi-type events pose a greater risk to populations than coseismic landslides certainly could be true, but it's not been demonstrated by anything in the manuscript, and I'd also suggest that that is removed or modified.***

AR: We agree with the reviewer's interpretation of what we mean when we say we don't see evidence of the Gorkha earthquake pre-conditioning the landscape for the Melamchi event. We agree that we should add the Roback landslides to Figure 10 so it becomes clear that the landslides could not have acted as sediment sources for the event (we will do so). Similarly, we agree that we can't say for sure that hillslope weakening from the earthquake didn't play any role in facilitating the Melamchi event, and we can't say that it did. We will include the Parker et al. (2016) study with a brief explanation of what sort of evidence we would need to come to an ambiguous conclusion, and why it is difficult to do. We also agree that we should modify the final statement of that section about the risk posed by Melamchi-type events being higher than that of coseismic landslides, to “we have shown that in addition to coseismic landslides, such sediment stores can pose a risk to populations in a region increasingly exposed to extreme hydro-meteorological events.”

RC: ***The figures are generally clear and have good, informative captions. The photos in Fig 2, while definitely compelling, don’t really add much to the focus of the manuscript. I also wasn’t sure why the steepness (long profile or k_sn) and width data needed to be shown in both Fig 6 and Fig 9, so a clearer rationale for why both figures are necessary would be helpful.***

AR: Thank you for your comment. While we understand that Figure 2 brings too much focus on the Melamchi event, we have to recognize that it is the main driver of sediment export in our study area over the period studied, and therefore needs to be described in detail. We will work on the introduction to better motivate our study and integrate the Melamchi event. The reason why we added Figure 2 and would like to keep it is that all the reports show the extent of the damage based on aerial images. Because we had pictures of the area from the ground, we thought that this figure was important in conveying the extent of the damage to readers who are not familiar with the area. The aerial images do show widespread devastation, but it is only when we started piecing together the images with landmarks from our pre-event survey (including the brand new Hotel Roj that we used as a “stable” location for our base station in the field) that we understood the extent of the damage. It was truly heartbreaking. Long profile in Figures 6 and 7 is shown to give better spatial context to the other three panels, and not intended solely to give an indication of channel steepness. The maps of k_{sn} in Figure 9 cover the full catchment, including tributaries, rather than just trunk channel within the extent of mapping. They are intended to allow identification of patterns of channel steepness (and valley width, combined with Panel C).

RC: ***Line 2: Or longer? Centennial +? I guess this relates to the second clause - eventually that sediment must leave the catchment, although not necessarily on annual to decadal time scales***

AR: We will change decadal to "decadal to centennial" here.

RC: ***Line 6: rivers?***

AR: Reviewer #2 is correct. We have changed this to “rivers”.

RC: ***Line 7: Might be clearer to say ’along both river corridors’ - there is coarse sediment accumulation in the catchments, just not along the valley floors***

AR: We changed “in both catchments” to “along both river corridors”.

RC: ***Line 11: helicopter-based***

AR: We changed “helicopter based” to “helicopter-based”.

RC: ***Line 12: You might consider bringing this back to the overall focus of the ms here - otherwise the 2021 event feels like a bit of an afterthought. How does that relate back to the themes in the first half of the abstract?***

AR: As mentioned in our general comments, we will reframe the study accordingly to better define the aims of this study and better integrate the Melamchi event.

RC: ***Line 19: I would add Li et al. (2014) to this***

AR: We have added Li et al. (2014).

RC: ***Line 23: Surely not indefinitely...?***

AR: We could rephrase this to "In the short term, the sediment generated by coseismic landsliding either remains on the hillslopes or is delivered to fluvial systems".

RC: ***Line 33: Probably more accurate to say 'at least three years'***

AR: We changed the text to "at least three years".

RC: ***Line 43: There is a fairly abrupt jump here from the review of past work in the preceding paragraph to your specific focus in this ms. I didn't get a clear sense, however, of what aspect of this overall topic you want to address, or what the problem is. I think a couple of extra lines here might help the reader to understand that better***

AR: This relates back to Reviewer #1's first comment on the manuscript – we need a "bridging" sentence between the background on coseismic landslides and the introduction of our study area, to identify where our study fits in. We will work on this.

RC: ***Line 48: Linked to the preceding comment - I'm not sure that the introduction really sets the scene for this specific aim and focus. Why are valley morphology and hydrological events likely to be important? If you find areas of aggradation, what does that tell you about sediment production and export? If you don't find any, what does that tell you? What do we think we understand about pre-conditioning? Why is it important to understand whether the Melamchi flood (which has only been mentioned in passing) was related to the earthquake? For example, some work led by Fei Zhang pointed to the importance of the hydrological system in transporting sediment and creating an earthquake signal in the Zipingpu reservoir downstream (https://www.science.org/doi/10.1126/sciadv.aav7110). But they were looking only at the upstream and downstream ends of the system - they didn't know what was happening in between. So there is certainly scope to illuminate what happens in between source and (local) sink in terms of temporal changes in storage along the transport pathway***

AR: See comment above, and we will include the Zhang paper to better set the scene - thank you.

RC: ***Line 57: Whatever its other merits, I'm not sure that the Kargel et al. paper is the best source of information on the costs of the earthquake. If you want to go directly to the source then perhaps the PDNA is better to cite here: https://reliefweb.int/report/nepal/nepal-earthquake-2015-post-disaster-needs-assessment-executive-summary***

AR: We will add that citation.

RC: ***Line 67: Suggest removing this reference - it's not an inventory but instead focuses on modelling based on incomplete inventory data***

AR: We thank Reviewer #2 for this suggestion and have removed the Robinson et al. (2017) reference.

RC: ***Line 90: "and sources roughly half of its area from the Tibetan Plateau": Not sure this is needed, and it's not really visible in Fig 1c***

AR: The fact it is not visible (for clarity purpose) is the reason we thought we should mention it. We would like to keep this.

RC: ***Line 96: You could cite Nepal et al. (2019) on landslide susceptibility along the Arniko Highway: https://www.sciencedirect.com/science/article/pii/S2590061719300377***

AR: We'll add that reference.

RC: ***Figure 1: Perhaps just worth noting here that this is the southern portion of the catchment - you've cut off more than a third of the upstream area***

AR: We clarified the caption to read "Southern portion of the Bhote Koshi catchment".

RC: ***Line 134: The exact source of this sediment is explored further below: That's not quite the same as the aim that was outlined in the introduction (lines 48-51)***

AR: We may remove this sentence in an effort to address the general issue of "clarification of the aims and better integration of the Melamchi event"

RC: ***Line 135: This is all fine and reads well. But, related to my previous comments on the introduction, it's not entirely clear at this point in the ms why you have chosen this particular time window 2012-2021, or why therefore these events are important to include and describe here. What do you mean 'taking into account their impact'? Impact on what? I think once the introduction makes a clearer case for what you're trying to achieve, this can be clarified as well. For example, there was a large debris flow from the Chakhu Khola in July 2019 that damaged the intake of the Middle Bhotekosi HEP scheme - is that worth including here?***

AR: This will be addressed accordingly, see earlier comments.

RC: ***Figure 2: Include deg symbol and N/E indication***

AR: We have added degree symbols and N/E indication to the coordinates.

RC: ***Line 140: Why that time range?***

AR: As the initial aim of the study focused on the impact of the Gorkha earthquake on fluvial and sediment dynamics, we chose to extend the mapping back to 2012, under the assumption that three mapping epochs prior to the Gorkha earthquake would establish a baseline of "pre-event" water and sediment area. The end of the mapping epochs was set to December 2021 as the June 2021 Melamchi event occurred partway through the mapping process, and we wished to capture the aftermath of the event. We will clarify in the text.

RC: ***Line 140: Perhaps 'help to constrain them'? If I understand you correctly, you're not actually identifying zones of sediment input, but aggradation must be happening downstream of those major inputs...***

AR: We have changed "these data identify zones of sediment input" to "these data help to constrain zones of sediment input".

RC: ***Line 141: And postseismic? The inventories in the Kincey et al. (2021) paper show that the footprint of landsliding evolves quite a bit following the earthquake***

AR: Currently, we calculate connectivity for coseismic landslides only, using the inventories of Roback et al. (2018) and Gnyawali and Adhikari (2017).

RC: ***Line 142: Of the trunk streams only?***

AR: We calculate channel steepness and valley width within the full catchment. We have clarified the text to read "within the studied catchments" rather than "along the study reach".

RC: ***Line 158: OK, that's useful. At some point it would be good to talk about how confined the valleys are, and therefore how much you would expect aggradation to affect the planform area. In some parts of both valleys, quite a lot of aggradation could occur without much change in planform area; elsewhere they are***

less confined

AR: Reviewer #2 makes a good point here: see response to earlier comment. We will highlight this bias in our methods.

RC: ***Figure 3: It would be useful to annotate these panels with image dates***

AR: We'll edit the figures to add the image dates directly to the panels.

RC: ***Figure 3: This is a minor point, but as you don't have direct measurements of grain size, I think 'sediment' might be a better general term to use throughout the ms - it doesn't imply a particular particle size class***

AR: We will change "gravel" to "sediment" – especially since we know that the deposits associated with the Melamchi event are mostly finer than gravel.

RC: ***Line 164: Presumably the same polyline is used for all mapping epochs, for comparability? Might be worth pointing that out***

AR: Reviewer #2 is correct – we have clarified this in the text by adding "For comparability, the same polyline is used for all mapping epochs.".

RC: ***Line 168: Since this is relatively long in the downstream direction, how much double-counting are you doing? In other words, for a river going through a tight bend you will count additional area in multiple overlapping windows. Presumably you would want to minimise that, no?***

AR: It is true that there will be windows overlapping in sinuous areas. However, we believe the impact is minimal for our study as we are mostly focused on changes between periods rather than estimating a true sediment budget for the area. We have been trying to think of solutions to this problem but realised this was likely the best approach for our purpose. The choice of the window size is deliberate too, as justified by the sensitivity analysis showed in the Appendix. We believe 500 m is the optimum window size to characterize changes at the reach scale we are interested.

RC: ***Line 170: By 'combined' do you mean water + visible sediment?***

AR: That is correct – we have now clarified this in the text.

RC: ***Figure 4: Given that the windowing procedure is already doing some smoothing (by including c. 10 points on either side of the point in question), why add another round of smoothing? To be honest, there is little difference between the dashed and solid lines in panel d.***

AR: Linking back to a comment Reviewer #2 made earlier about this: this does not make a big difference for plotting one mapping epoch by itself, but becomes more pronounced when plotting multiple mapping epochs on the same profile.

RC: ***Line 184: Just to be clear - these are uncertainties on the per-window areas (or per-point areas as plotted in panels a and c, not on the whole area in each epoch. I think this is pretty clear but am just checking that I understand it correctly***

AR: That is correct – the uncertainties are for each window, not for the entire area in each epoch.

RC: ***Line 192: I think it's worth explaining why you have repeated this, and how it links back to your overall aim. What are you looking for in particular? It's not obvious (to me at least) why this analysis is part of the ms***

AR: We will clarify accordingly – see answer to reviewer #1 too. The problem is that we did not initially intend to do this analysis, but when our analysis showed little evidence of sediment transport following Gorkha, our first thought was "are the Gorkha landslides connected in these valleys"? And when the Melamchi event happened, we wanted to know if there was any evidence that showed that landslides were better connected in the Melamchi-Indrawati catchment than in the Bhote Koshi. It didn't feel right to suddenly include a new method in the discussion, so we decided to include it in methods, but have been struggling to integrate it within the aims of the study. We will work on this.

RC: ***Line 205: It's good to include this, but I think it needs a little more explanation. Are you looking for correlations between steepness, width, and areas of aggradation? If they are correlated, what will that tell you? If they aren't what will that tell you? And how does this link back to the aims in lines 48-51?***

AR: We will try to justify better the need for this analysis. It is down to linking observations to valley morphology, as we expect that sediment deposition will be affected by it: deposition is more likely in wide, flat reaches (such as the alluvial reach between Melamchi Bazaar and Dolalghat), whereas steeper and more confined reaches will promote faster export of sediment. Similarly, low gradient aggraded valleys may represent sources of sediment. The aim is to test whether the observations can be (at least partly) explained by the valley morphology.

RC: ***Line 208: This makes it sound as if the analysis is being done on the trunk streams plus major tribs - but the mapping in 4.1.1. seems to be only on the trunk streams? If that's the case then what is gained by looking at width on the tribs? It might also be worth explaining what the valley width routine is actually capturing in this case. There are parts of the Bhote Kosi valley that are quite confined between bedrock walls, and others with low alluvial terraces (e.g., at the inside of bends). What is being included as 'floodplain' in this landscape?***

AR: See answer above. We are trying to characterize the valley morphology as there is a lot of variability between valleys and even within a single valley, and these variations can affect the distribution of sources of sediment and the locus of deposition during flood events. When it became clear that sediment sources could be located upstream of the steep confined valleys that were most affected by post-Gorkha landsliding, we realised we needed to integrate both main stem and tributaries for the analysis. We will try to better motivate this part of the analysis.

In terms of what is considered as a floodplain, the floodplain extraction algorithm assumes floodplains to be low-gradient surfaces adjacent to the channel and identifies these based on local gradient and relief relative to the nearest channel for each DEM pixel. In the case of the Bhote Koshi, this means that low alluvial terraces at the inside of bends are considered floodplains.

RC: ***Line 218: What concavity signal? If the point here is to look at changes over time, then why does it matter that the reference concavity is low?***

AR: Reviewer #2 is correct in that the low reference concavity is not an issue in this case. We have rephrased the text to clarify: "Additionally, Gailleton et al. (2021) suggest that the presence of glaciers as well as spatial variations in tectonic processes can obscure the optimal concavity value. However, as we are merely using k_{sn} as a description of the overall channel steepness within the catchments, the relatively low value of the reference concavity does not present an issue."

RC: ***Line 221: I think it's more accurate to say that the gravel area, not necessarily the changes in gravel area, helps to pick out this transition***

AR: We have changed the manuscript text to read "The gravel area mapped along the Melamchi-Indrawati rivers [...]"

RC: ***Line 226: By 'temporal noise' do you mean 'no clear trend over time'?***

AR: We take "temporal noise" to mean that the rivers are dynamic and gravel area changes over time, although without a clear trend.

RC: ***Line 230: I don't think this is a good citation here -it's not specifically about the Jure landslide***

AR: We have removed the Croissant et al. (2017) reference.

RC: ***Line 232: I think I agree... but do we actually know this? This depends very much on how the coseismic landslides are distributed relative to your mapping area. There's no question that there was some direct input (Wang et al. 2015 Geology showed that really nicely with the suspended sediment records after Wenchuan)...***

AR: Reviewer #2 correctly points out that we do not know at which time coseismic landslide sediment may have reached the channels. We have rephrased the text to clearly state that this is an assumption we made when choosing our mapping epochs: "Although the mainshock of the Gorkha earthquake series pre-dates May 2015 imagery, the time interval is short enough that we assume the majority of coseismic landslide sediment has not yet been delivered to the river network, or been transported a significant distance".

RC: ***Line 235: What's the date range on that change? And what is the evidence that it was a debris flow in the trib?***

AR: The change occurs between May 2015 and January/February 2016. We have changed the manuscript text to reflect this. We visited the area in October 2019 and confirmed that the deposits at Timbu are linked to a debris flow.

RC: ***Line 241: It might just be worth reminding the reader that these quoted numbers are per 0.5 km2 window***

AR: We thank Reviewer #2 for pointing this out and have clarified it in the text.

RC: ***Line 242: I'm not sure what is meant by 'continuous signal'***

AR: We meant the continuous increase in gravel area between mapping epochs and have updated the text accordingly.

RC: ***Line 244: downstream***

AR: We have changed the text to "along the remaining study reach downstream until Dolalghat".

RC: ***Line 251: I'm not sure what this is referring to - are the two clauses in this sentence talking about the same increase? It is much more than a few km from Chanaute Bazaar to the MCT. If not, then what July 2021 peak, and what is the evidence that it is being translated? I don't see that in Fig 6. This could be due to the colour scheme, which makes it difficult to identify an individual interval, so it might be worth annotating the features that you wish to highlight***

AR: We meant Melamchi Bazaar instead of Chanaute Bazaar - thank you for spotting that. We will experiment with different colour schemes for Figures 6 and 7 (see also comment below), and will annotate features if needed.

RC: ***Figure 6: The colours don't come out clearly in the PDF, making it hard to see the trend over time in these plots. I wonder about a blue to red scheme instead?***

AR: As we have 15 and 14 mapping epochs for the Melamchi-Indrawati and Bhote Koshi rivers, respectively, finding a colour scheme which allows the reader to distinguish between individual mapping epochs while still preventing visual distortion of the data and exclusion of readers with colour vision deficiencies is challenging. We will try with a red-blue colour scheme, as suggested.

RC: ***Figure 6c: You could sum these up for each epoch to get a total change in km2 per epoch over the whole reach, and then plot those over time - that might be a way to document both the lack of a clear trend and the step change in summer 2021***

AR: We agree that a figure like that would be more helpful and accessible than the one we currently have. However, we tried making this plot before but realised it wouldn't work because not all mapping epochs cover the same length of river (for example, the 2013 Melamchi-Indrawati mapping only goes ca. 20 km upstream from Dolalghat).

RC: ***Figure 6: Again it might be worth reminding the reader that this is per 0.5 km2 window***

AR: We have edited the text to reflect this.

RC: ***Line 259: Is it necessary to show both inventories, given that they show similar patterns? Why not combine them, or move one to the SI if it needs to be shown?***

AR: This is a fair point. We wanted to use both inventories for completeness but could keep the Roback figure in here and move the Gnyawali and Adhikari figures to the SI, just to show that there is no big difference between the two.

RC: ***Line 261: This links to my earlier query in the methods - what's the purpose of repeating their analysis, if only to report that you've confirmed their finding? And, given that this ms is only focused on these two catchments, what's the significance of this finding? Are you arguing that there are similar patterns of connectivity between the two valleys? In that case I'd suggest stating that more directly. A more effective way to show this might be to show connectivity as a function of landslide area for the two valleys, as Li et al. did, because that helps to also communicate the role of the area-freq distribution. It's not surprising that connectivity increases with buffer size; that, plus the two inventories, mean that this figure takes up a lot of space but doesn't actually communicate that much information (and doesn't actually communicate what the highlighted sentence seems to suggest - that landslides in these two valleys are similar to each other but different from the study area as a whole). That would allow the buffer size sensitivity and other inventory (if needed) to be moved to the SI***

AR: See answer to previous comments. We are going to add more information through our own analysis of connectivity along the study rivers, to make it more relevant to our specific problem (and likely remove Fig. 8). In particular, we would like to demonstrate that the Melamchi event is not a result of greater connectivity of co-seismic landslide in the Melamchi catchment. We will expand the connectivity analysis in the text.

RC: ***Line 265: That could be done with the inventories in Kincey et al. (2021), which are freely available...***

AR: We checked the supplementary data for the Kincey et al. paper and they appear to only include landslide density. We are not sure we can get to connectivity from there.

RC: ***Line 266: True... but this brings me back to the point of doing the connectivity analysis in the first place!***

AR: Yes, see earlier responses to comments.

RC: ***Line 276: That's visible in the long profile in Fig 6 - does it need to be shown again in Fig 9? Do we need the k_sn plot?***

AR: As said in response to an earlier comment on this topic, the long profile in Figure 6 is intended to provide spatial context for the mapped reach, whereas the map of channel steepness in Figure 9 allows us to see the spatial patterns along the main stem and tributaries and give overall context for the location of the different reaches.

RC: ***Line 281: Perhaps label in Fig 9***

AR: We will label the Balephi River.

RC: ***Line 283: Why is this not visible in Fig 6? And the width in Fig 6 is never <50 m, so I'm not sure how to square that with the text here. Apart from this detail, I'm not really sure what Fig 9 adds that Fig 6 doesn't already communicate... there doesn't seem to be any use of the k_sn values or the trunk stream/tributary information***

AR: The long profiles in Figures 6 and 7 only extend as far as the mapped reaches of each river, and therefore they do not show the sections discussed here. While we do not use the k_{sn} values themselves, we use the channel steepness and valley width data to identify patterns in width and steepness across both catchments, as demonstrated in the manuscript shortly after this comment.

RC: ***Figure 9: It might be worth decreasing the contrast on the hillshade image, to allow the underlying colours to come through more clearly?***

AR: We will experiment with different opacity for the hillshade.

RC: ***Line 294: This is a slightly different wording than in the introduction. I'm not sure that you can determine whether or not this was a 'necessary' pre-condition - but you might assess whether it was a pre-condition***

AR: See earlier comments, in particular responses to reviewer #1: we are going to reframe the study and better introduce the concept of pre-conditioning. We will rephrase accordingly.

RC: ***Line 316: Melamchigaon***

AR: We have changed the text to "Melamchigaon".

RC: ***Line 322: and postseismic?***

AR: We added "and postseismic".

RC: ***Line 322: strikethrough: during the earthquake***

AR: We removed "during the earthquake".

RC: ***Line 324: I'm not sure I agree with this statement - in the preceding section you have already described the collapse of a glacial moraine, incision into a pre-Gorkha landslide dam and entrainment of part of the fill behind the dam, and the role of a pre-Gorkha landslide that may have been reactivated in 2015. How are any of those sediment sources linked to the earthquake?***

AR: That is a fair point. However, there is still debate over where the sediment came from (and for example we believe a lot of the sediment came from sources that were not identified in the report, following our analysis). Also, sediment stores such as co-seismic landslides could have been remobilised by the event if well connected to the river, and could have therefore contributed to the disaster. We will rephrase accordingly to highlight the purpose of our work here.

RC: ***Line 328: Can you explain how this differs from the investigations that have just been summarised in the***

previous section?

AR: The main difference is that we are using imagery that was not available at the time the reports were written, and we are also analysing coseismic landslide connectivity to assess their potential contribution. We agree that was not clear and will try to highlight the novelty of our approach better.

RC: ***Line 329: Should this be Bremthang?***

AR: Reviewer #2 is correct – we have changed this to “Bremthang” in the manuscript.

RC: ***Line 332: This is confusingly worded - the preceding sentence is focused on excavation, but here the figure is being cited to show evidence of accumulation in the tributaries. Which is it?***

AR: We will rephrase accordingly. We agree this is confusing, and will redraw Figure 10 to better link the text to observations on the figure.

RC: ***Line 333: I’m not sure what you mean by ’reactivated’***

AR: We will change “were reactivated" by "exhibited sediment remobilisation”.

RC: ***Line 334: Between which valleys?***

AR: We will rephrase and link better with observations on Figure 10.

RC: ***Line 335: I agree... but it would be good to show evidence of this. Perhaps overlay the Roback et al. inventory on your Planet imagery?***

AR: Excellent idea. We will do so.

RC: ***Line 340: Pemdang?***

AR: We have changed this to “Pemdang” in the manuscript.

RC: ***Line 341: Bremthang?***

AR: We have changed this to “Bremthang” in the manuscript.

RC: ***Line 343: I’m not sure these details are needed - along strike of what?***

AR: This has been shortened for clarity.

RC: ***Line 344: In fairness, Maharjan et al. are similarly circumspect and don’t really say anything concrete about a link. I suspect that you are correct, but I also don’t see that you can rule out a link (or what you’re calling pre-conditioning) based on what you have presented here. You could, if you showed coseismic and/or post-seismic landslide inventories, document whether or not there is a spatial relationship between visible sediment sources and those landslides. Like you, I suspect that there isn’t a simple correlation. But I don’t see how you can rule out what Maharjan et al. mention - that hillslope material was weakened by shaking and therefore was more prone to failure than it otherwise would have been. That’s a really weak assertion because it’s untestable (and even when we tried to test that assertion in Parker et al. 2016, the signal turned out to be really small...), so I’m certainly not defending what they’ve written. Equally, however, I don’t see any evidence here to say that that hasn’t happened. So I’d suggest that you re-frame this to be a little more careful***

AR: The reviewer makes a good point here. We cannot rule out that the hillslopes in the upstream source of the Melamchi event were not weakened during the earthquake, and so more vulnerable to collapse. But the lack of correlation between the Melamchi source and the co-seismic landslides implies a distinct trigger for the Melamchi event. We will rephrase this to avoid ambiguity over the use of the word “pre-conditioning”.

RC: ***Figure 10: Might be worth pointing these out with arrows - the images cover a large area and the features that you want to draw attention to (I think) are pretty small***

AR: This figure will be redrafted with arrows to highlight the key features.

RC: ***Line 352: This is another reason for taking a similar approach to Li et al. (2016) - would be useful to quantify the comparison with Wenchuan***

AR: We will revise Figures 6 and 7 to plot connected landslide area as a function of downstream distance (similar to Li et al (2016)), which will allow us to demonstrate from Wenchuan to demonstrate the connectivity of landslides as a function of downstream distance on the river long profile. See also our response to Reviewer #3’s suggestion to calculate landslide area connected to the studied reaches with the method used to calculate sediment area along profile.

RC: ***Line 355: Now I am confused, because that’s not how I interpreted the text on p. 18 - I thought you were suggesting that these two catchments were similar, relative to the overall study area (and thus distinct from it). Can you clarify?***

AR: As the reviewer indicates, Figure 8 is designed to show that these two catchments are similar in terms of their connectivity between the landslides and the river channel.

RC: ***Line 366: I’m not sure what the distinction is here***

AR: We agree that there is some uncertainty about the cited interpretations, and so have deleted the last sentence of that paragraph.

RC: ***Line 375: OK, good - this is a clear summary statement for the reader***

AR: Thank you, that is good to hear.

RC: ***Line 385: I would replace this by ’the prevalence of’***

AR: We have changed “periods of” to “the prevalence of”.

RC: ***Line 385: strikethrough such as tropical cyclones***

AR: We have removed “such as tropical cyclones” from the text.

RC: ***Line 392: For these reasons, I’m not sure that this is a very meaningful comparison***

AR: We will delete this statement.

RC: ***Line 398: that***

AR: We changed “than” to “that”.

RC: ***Line 398: Because you have mentioned this several times, I really think it would be helpful to show the Gorkha landslide inventory on the Planet imagery, to make this point more clearly***

AR: We agree, this will be resolved by adding the Roback landslides to Figure 10.

RC: ***Line 399: see comment above***

RC: ***Line 402: The text here seems to suggest that you have a basis for distinguishing between (1) volume sourced from the Pemdang Khola and (2) volume sourced from other tributaries - and that the latter was greater than the former. You've not shown any evidence on this, so I suggest rephrasing this to avoid giving that impression***

AR: We rephrase this sentence to highlight that the area of remobilised sediment in the 4500-4800 elevation range of the catchment is extensive with the Pemdang Khola being only a portion of the mobilised sediment, but clearly contributed. The areal extent would make it more difficult to argue that the Pemdang Khola was volumetrically more significant.

RC: ***Line 415: This is referred to just as 'Bremthang' in the preceding section***

AR: We have removed "the" to read just "Bremthang".

RC: ***Line 440: I'm not sure that you've really done either of these things. It's hard to demonstrate the reasons for the absence of a signal, and other than connectivity being high you haven't really said much about why sediment doesn't appear to have accumulated along the Bhote Kosi. Given the small widths and confined nature of the Bhote Kosi, I'd expect to see some assessment of how much aggradation you could resolve - there just isn't much accommodation to fill outside of the river channel. And the latter point I think needs a more explicit comparison between Gorkha landslide locations and sediment sources to be clear to the reader***

AR: We broadly agree with the reviewer, and will rephrase this part of the conclusions in terms of the the distribution of the Gorkha landslides and the source of the Melamchi event. However, we do speculate on the reasons for the lack of a signal in terms of sediment mobility, but agree that any relationship between the Gorkha earthquake and the mobilisation of sediment in the upper reaches is unclear in terms of "pre-conditioning". This will also be clearer as we rephrase parts of the INtroduction as outlined previously.

RC: ***Line 444: Can you say this? I suspect that you're right, but I don't think that you've shown any evidence of volumes from different sources***

AR: We only refer to a "majority" rather than be specific about volumes. We can replace "was sourced" with "appears to have been sourced" to dilute the strength of the statement.

RC: ***Line 446: Again, as noted above - "remobilisation of 2015 landslide sediment" and "hillslope pre-conditioning" are potentially two very different things!***

AR: Our statement refers to remobilisation of the coseismic landslides, which we are pretty sure did not happen, given that the sediment involved in the Melamchi event was sourced from higher up in the catchment than the landslides.

RC: ***Line 448: This might be overstating what has been demonstrated here, which is to show two areas that have large valley widths and fairly low slopes. That is certainly something to look for more widely, and I think it's very useful to point this out as a potential area for future study...***

AR: We will rephrase to something along the lines of "We have demonstrated that maps of normalised channel steepness and valley width can help to identify such sediment stores".

RC: ***Line 449: I'm not sure what evidence there is for this statement. You might be right, but I don't think it follows from what you've shown here***

AR: We will rephrase this to state we can't say that these sediment stores pose a greater risk than coseismic landsliding, but we can point out that they do pose a risk and should be taken into account.

4. Reviewer #3

RC: ***In this interesting and thought-provoking study Graf et al track the evolution of gravel storage and export in 2 large catchments in the Nepal Himalaya. Using a unique dataset of gravel area through time and across catchment length, they determine the 2015 Gorkha Earthquake has had a minimal effect on the channel morphology and that only the largest hydro-meteorological events can cause significant alterations of the channel network. I believe this is a significant finding and would make an excellent addition to literature concerning the impact of extreme events on landscape evolution. As a result, this manuscript would make an excellent contribution to the journal subject to several revisions.***

AR: We thank Reviewer #3 for their helpful and encouraging comments.

RC: ***The revisions I propose are focused on more closely aligning the 3 main analyses (the analysis of the gravel area, landslide connectivity, and the morphology of the catchments) towards the stated objectives of the manuscript. I will discuss the main points of these revisions here and provide more specific line by line comments afterwards.***

RC: ***Firstly, the manuscript is lacking a clear statement of the research gap the study is trying to fill. The introduction provides a good overview of the study sites and analysis completed by the study does not clearly demonstrate the motivation of the study.***

AR: This is very similar to what Reviewer #2 has rightly pointed out. We will restructure the introduction to clarify that this study provides an opportunity to compare the impact of a potential source of sediment supplied by co-seismic landslides and an extreme hydrological event on the morphology and altered sediment flux of a Himalayan catchment.

RC: ***Secondly, the analyses of landslide connectivity and valley morphology feel underdeveloped and lack connection to the main gravel area dataset. In the results and discussion, the authors highlight the ineffectiveness of landslide connectivity in its current definition for predicting changes in the sediment storage in the valley. I feel that this is an important result which requires more analysis and exploration. I suggest that the authors conduct another analysis investigating landslide connectivity along the length of the study catchments. This could potentially be done using the boxes used to analyse the gravel area distribution. This study would allow for the correlation (or lack thereof) between the change in gravel area and landslide connectivity following the earthquake. I also feel a similar additional analysis is required for the valley morphology. Currently it is not clear what the objective of the steepness and width analysis is or how it connects to the gravel area dataset. By explicitly comparing the gravel area dataset with the morphology studies the influence of valley shape on sediment residence time will become more obvious.***

AR: We think this is an excellent idea, and is quite similar to a point raised by Reviewer #2. We have therefore chosen to add a component to Figures 6 and 7 where we show the area of co-seismic landslides in relation to their long river profiles, and mapped gravel area. As indicated by the reviewer, this enables a clearer consideration of the link between these metrics.

RC: ***Finally, I think the influence of the other hydrological events identified in section 3.2 needs to be discussed as well. Currently the manuscript only focuses on the 2021 Melamchi disaster and as a result the wider picture of the impact of these types of events is lacking. By discussing the complete timeline of events that affected both catchments it will be easier to interpret figures 6&7 and provide an estimate of the magnitude an event needs to be before it affects the catchment.***

AR: This is a good point, and so we will expand the discussion around the Bhote Koshi GLOF and debris flow.

RC: ***Lines 28&29: Earthquakes are followed by increased landsliding due to 1) remobilisation of coseismic landslide deposits and 2) weakening on the substrate by shaking (damaging the bedrock and breaking cohesive bounds in the soil profile).***

AR: We will be sure to distinguish these processes in the text.

RC: ***Line 33: Should consider the findings of (Jones et al., 2021) as they directly discuss how the earthquake affected landsliding rates in the Nepal Himalaya.***

AR: We thank Reviewer #3 for pointing us towards this helpful paper and will integrate it in the text.

RC: ***Lines 48 – 52: Currently this statement of intent is not well situated in the literature review discussed in the previous paragraphs. While these paragraphs offer a good coverage of the impact coseismic landslides have on channel evolution they do not discuss sediment export which this statement suggests is the main purpose of the manuscript. I suggest an additional paragraph on post earthquake sediment budgets and the role of meterological and hydrological events is required (these exist for the Chi-Chi (Chen et al., 2011; Dadson et al., 2004; Hovius et al., 2011), Wenchuan (Fan et al., 2019; Francis et al., 2022; F. Zhang et al., 2019; S. Zhang et al., 2016) and various New Zealand (Howarth et al., 2012; Parker et al., 2015; Wilkinson et al., 2022) earthquakes) to root this statement in the research gap that is of interest.***

AR: This point was also raised by Reviewer #2, and we have chosen to add an extra paragraph on sediment budgets following earthquakes with a bridging sentence to this study.

RC: ***Line 60: (Marc et al., 2016) would be considered a more up to date reference here.***

AR: We added Marc et al. (2016).

RC: ***Line 80/figure 1: Kathmandu could be labelled on the figure inset to provide better context.***

AR: We will label Kathmandu on the figure inset.

RC: ***Lines 81-87: Referencing the figure throughout this paragraph would help the reader to follow the description of the rivers. i.e. "Elevations in the Indrawati catchment range from around 600 m at Dolalghat (D in Fig. 1b) to >5000 m, and the climate spans temperate to polar tundra environments."***

AR: Good point, we will edit the text accordingly.

RC: ***Section 3.2: This section currently reads as a series of events that are not well versed into the context of sediment generation, transport and storage. In particular the observed, or expected (if not recorded) impacts on the sedimentary system, are not discussed.***

AR: This is a good point. We will work to place this more in the context of sediment generation, transport and storage.

RC: ***Line 125: The location of figure 2 should be labelled on figure 1b.***

AR: Melamchi Bazaar is labelled on Figure 1b. We have edited the caption of Figure 1 to specify that Melamchi Bazaar is the location of Figure 2, and the caption of Figure 2 to state that Melamchi Bazaar is labelled as MB in Figure 1b.

RC: ***Figure 2 c&d: The viewpoints and directions of a&b should be labelled to help the reader recognise the locations.***

AR: We will label the viewpoints and directions on Figure 2 to improve legibility.

RC: ***Line 152: Figure 3 should be referenced here.***

AR: We added a reference to Figure 3, changing the text to: "We used sub-metre resolution imagery from Maxar Technologies and CNES/Airbus, available in Google Earth Pro, for the time period between 2012 and 2019 inclusive, and 3 m resolution Planetscope imagery, obtained through Planet's Education and Research Program, for 2020 onward (Fig. 3)(see Appendix A for the full breakdown of imagery dates and sources)".

RC: ***Lines 160-163: It is not clear why this procedure is being used rather than a direct comparison of the shapefiles through time.***

AR: We attempted a direct comparison of the shapefiles through time at the outset of the study, but found that the results were misleading since the rivers are not straight in x-y coordinate space. As we wanted to show spatial as well as temporal changes in sediment area along both rivers, calculating area within a moving window along a common downstream distance was then the most straightforward approach.

RC: ***Lines 160 - 173: The procedure for choosing the parameter values described in A2 should be moved to here as currently it is not clear how these values were chosen in the main text. I also feel the relationship between ds and dp and the overall area recorded needs further exploration as there is significant overlap between the windows.***

AR: While we agree with the reviewer that the parameter choices need to be clarified in the main text, we do not think that the full procedure from A2 should be moved to the main text as it is long and detailed. We will clarify the parameter choices in the main text and refer to A2 for the full procedure. We will additionally expand A2 to include a more in depth analysis of the relationship between d_s and d_p.

RC: ***Line 178: Does the change in resolution of the satellite images have an impact on the area recorded?***

AR: This was a concern when we switched the mapping to Planetscope imagery. Any changes in area due to the different resolution are included in the uncertainty added to the area within each 0.5 km^2 window, which was calculated based on repeatedly mapping imagery from the same dates using both Google Earth and Planetscope images (Fig. 5).

RC: ***Line 183: Not sure what is meant by "The 75th percentile of the collection of standard deviations"***

AR: For each point along the river, we have ten separate area values (as each river was mapped five times using Google Earth imagery, and five times using the Planetscope imagery). We calculate the standard deviation of these ten values, which gives us a standard deviation for each point along the river (e.g. 500 standard deviations assuming we have 500 points). The 75th percentile of these 500 standard deviations would then be selected as the uncertainty value.

RC: ***Line 184: Is this the total uncertainty of the entire profile or per box?***

AR: This is the uncertainty per box. We have clarified the text to reflect this.

RC: ***Section 4.2: Connectivity is not discussed in detail prior to this section.***

AR: Reviewer #3 is correct – we will add text on this in the introduction.

RC: ***Line 188: Is the location of the landslide defined by its origin/scar location or its deposit?***

AR: For our analysis, we look at the landslide polygons as combined scar and deposit. If a polygon intersects the buffer zone, we consider it as connected. We have added "In this analysis, as in Roback et al. (2018)'s study, the landslide polygons consist of the landslide scar and deposit" to clarify this.

RC: ***Line 193: This is the first discussion of why connectivity is being analysed in this manuscript and it does not seem to be linked to the overall stated aim of investigating sediment transportation in the Nepal Himalaya.***

AR: This is similar to what Reviewer #2 mentioned before. Connectivity and its application to this study will be included in the introductory text. We will also improve the linkage between the different analysis components and to the aims of the manuscript.

RC: ***Section 4.3: It is not clear how these metrics are used to identify areas of sediment storage.***

AR: This is a fair comment. Devrani et al (2015) demonstrated that during a similar event in Uttarakhand, sediment was stored in reaches of low normalised steepness (k_{sn}), but incision occurred in reached with high k_{sn} – in other words, the event enhanced the steepness characteristics. By combining this with valley width, we give it a more 3D context. We will highlight this more at the beginning as motivation for the section.

RC: ***Section 5.1. Currently it is difficult to follow the section as the reader is required to flick repeatedly between figures 6&7. I think this section should focus first on figure 6 and the Melamchi-Indrawati rivers before discussing the Bhote Koshi. The comparison between the 2 rivers can be done at the end or in the discussion section.***

AR: We agree that this is a good idea and will restructure Section 5.1 to focus first on Fig. 6 and the Melamchi-Indrawati before moving on to Fig. 7 and the Bhote Koshi.

RC: ***Figure 6&7a are not referenced in the text, perhaps should be relabelled.***

AR: We believe that these panels provide spatial context to Figures 6 and 7 and should therefore remain at the top of each figure. However, we will edit the text in Section 5.1 to ensure that we refer to these panels.

RC: ***Figures 6&7. Currently there is too much information on this figure and the key dates (the earthquake and the hydrological events) are not immediately clear. Perhaps the yearly surveys can be combined to bins based upon the timeline of the events discussed in section 3.2. Another panel which may be useful is a figure showing the absolute cumulative change through time, this would have the potential to draw out the most dynamic areas for further discussion.***

AR: We believe that these figures are key integrative figures in this manuscript and do not contain superfluous information. We agree that replacing panel C in Figures 6 and 7 with absolute cumulative change through time would make the figures easier to interpret. This will present some difficulties as the mapping epochs do not all cover the same distance (see also A1, and our response to a similar comment raised by Reviewer #2). We will look into this.
Regarding the difficulty of extracting key dates, we can add symbols to the legend that identify the mapping epoch corresponding to each event.

RC: ***Line 237: It is not clear which panel of figure 3 is being referred to here.***

AR: We have edited the text to clarify that panel B is being referred to.

RC: ***Lines 241 – 257: A lot of this section seems better fitted in the discussion rather than the results section. Most of the description of the Melamchi disaster has been discussed in the introduction and is not relevant here.***

AR: We believe that this section merits its place in the results section rather than the discussion, as it links the observed trends in Figure 6 with the known timing and location of the Melamchi event.

RC: ***Line 260: Is the connected area/volume the total area of any landslide identified as connected or just the area within the buffer?***

AR: This is the total area of any landslide identified as connected. If a landslide intersects with the buffer, it is deemed connected to the river and its full area is included.

RC: ***Line 266 - 270: This section would be better placed in the discussion rather than here. The points raised in this section are important, particularly when combined with the lack of a clear signal of gravel mobilisation in the river profile. A further analysis comparing the volume of connected landslides to the change in gravel area along the river length would allow for a conclusion to be drawn about the usefulness of this definition of connectivity in the study area.***

AR: We agree that this is better placed in the discussion, and will move it. This will also be improved once we have added the connectivity analysis along the mapped river reaches.

RC: ***Section 5.3: I think this section needs to be more explicitly linked to the gravel area data. I.e. is there a correlation between the channel slope and width with the channel area? This can then be compared with the connectivity of coseismic landslide volume to offer a further opportunity to explore whether the morphology of the channels sets the location of sediment storage or if it is the location of the sediment source.***

AR: We think Reviewer #3 is right. The correlation is already visible in our data but we will spell this out more clearly. The wider the channel, the higher the channel area (makes sense), and the quicker we see any changes in area due to aggradation (in a confined reach, we may never see a change in area even if there is significant aggradation).

RC: ***Lines 305-320: It is not clear from this reporting how much time there is between the cascading events. Is this a continuous event or the result of a series of unfortunate events spanning several days or longer?***

AR: We believe this can be viewed as one continuous event as the landslide dam only blocked the river for a couple of hours. We will double check the timings.

RC: ***Line 344: (Jones et al., 2021) also identifies similar events causing widespread sediment mobilisation.***

AR: Thank you for this suggestion – we will add this reference to the relevant section.

RC: ***Section 7.2: This section should also include analysis of the history of events identified in section 3.2 to provide a better understanding of how frequent large events are and their influence on sediment storage.***

AR: We agree that expanding Section 7.2 to include the full history of events described in Section 3.2 is a good idea and will edit the manuscript accordingly.

RC: ***Lines 392-395: Why can you not calculate the connected volume of landslides in the area affected by the Melamchi disaster? This seems like an important piece of data for this study.***

AR: Following an earlier comment from Reviewer #3, we will shift our connectivity analysis to calculate landslide area along profile, and will be able to estimate connected landslide volume from there.

RC: ***Section 7.3: This section only focuses on how the morphology of the valleys may have influenced the probability of the 2021 disaster occurring. It does not discuss in detail how morphology controls the storage and export of sediment. If this extreme event had not happened would there be any significant differences in the evolution of the gravel area of the 2 catchments?***

AR: We will expand this section to explore the link between channel steepness, valley width, and gravel area in more detail. From there, we can then discuss how valley morphology controls the storage and export of sediment.