

Peer Review File

Manuscript Title: Medieval demise of a Himalayan giant summit induced by mega-landslide

Reviewer Comments & Author Rebuttals

Editorial Notes:

Referee: 1: Out of three attached files only the first one has been added into this file as the other two were main text edits. (Please refer page No.7 of this file)

Referee: 2: The attached file was a main manuscript hence has not been added into this file.

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):
please see three files attached

Referee #2 (Remarks to the Author):

Review by Niels Hovius
11.10.2022

All mountains are ephemeral. What, then, brings down those that stand tallest of all? Because high means inaccessible, we have, in fact, little scientific information and insight to answer this question. Lavé and co-authors present an exquisite piece of forensic geomorphology and sedimentology, supported by thorough geochronology, to fill this gap, at least in part. The study documents what may have been one of the largest known collapses of subaerial topography. It substantiates volume, source area, sediment redistribution, and timing of events. In doing so, it builds a powerful case for the role of very large landslides in limiting the height of very tall mountains, raking into the domain where low temperatures slow down or stop most other processes. Lave and colleagues argue that what may be called cryostrengthening suppresses progressive mass shedding, leading to a buildup of metastable topography on a large scale at elevations high above the glacial Equilibrium Line Altitude, and leaving room only for catastrophic adjustments of mountain height. In doing so, they develop a new paradigm to complement influential concepts of a glacial buzzsaw and frost cracking, both of which are thought to reduce and limit mountain topography. The proposed concept is a plausible explanation of the reconstructed history of the Annapurna IV, which may until a fateful day in the 12th century AD have been the world's 15th 8000 m peak. While the authors are careful not to engage in adventurous extrapolation, their concept may well apply to many other eye-catching mountains protruding far beyond the trim line of glaciers. Instead, they speculate about the impact of similar episodes on downstream communities, arguing that effects may well propagate far beyond the mountain front and into heavily settled foreland landscapes. A salutary tale, worth keeping in mind when planning the development of mountain areas.

I find the study technically convincing, conceptually important, and of interest to a wide readership. Moreover, the starting question has an innate appeal. What more is there to desire for those who side with Mallory, who professed to wanting to climb Everest "Because it's there".

That said, I have some comments on this study.

1) The volume reconstruction hinges on the remote identification of a few deposit remnants at the

fringes of the Sabche basin. The authors infer that the entire basin was filled to the brim, with an approximately flat top surface. The alternative of a massive avalanche running up the sides of the basin and leaving deposits above the fill in the central depression is not mentioned. This scenario might have involved significantly smaller rock and debris volumes. I urge an exploration and discussion of this alternative in order to have a better sense of the range of possible volumes.

2) The appear to implicitly reject the glacial buzzsaw concept, because it does not permit the emergence of very high peaks. It may be better to argue that while the buzzsaw operates in glaciated mountain ranges, it evidently does not catch all rising peaks. Ones that make it through the sawing window are home free but for the large-scale collapses that inevitably happen when mountain relief can't be sustained by their rock mass. This requires some rephrasing

3) The descriptive sedimentology for the Sabche basin breccias is not elaborated in the supplementary materials. While the main text is clear, it constitutes a sequence of statements that are not underpinned by photographic material or logs. Can the authors add the best of what they have on this to the supplementary file?

4) Do the authors think that the Sabche case is an example of a mechanism that may apply to many high mountains? They are carefully sitting on the fence on this question, but invite it with the opening paragraph of the main text. I think that a return to that question at the end of the paper will help round the manuscript and instigate a wider investigation of the generality of the concept.

5) Some minor reorganization of materials in the final sections of the manuscript and the removal of a few redundant sentences may help further improve what already is an eminently legible text. In addition to these points, I have left a string of comments in the left margin of the pdf document. I have also left suggestions for minor edits littered across the text. I hope that the authors will find them useful.

I ask that you please disclose my name to the authors. If we don't do double blind, then we do double seeing.

Thank you for asking me to review this manuscript. I do hope it will make it to print in Nature.
Niels Hovius

[please also see one file attached]

Referee #3 (Remarks to the Author):

Summary of study

Lave et al. present a comprehensive forensic analysis of a large rockslide and associated deposits in the Annapurna range in Nepal. Using field observation, a suite of geochronological techniques, and slope stability analysis, they argue for the catastrophic failure of a paleo-Annapurna IV peak that likely exceeded 8000 m elevation. Recognizing that the height of this paleo-peak and other >8 km giants in the Himalaya lie well above the equilibrium line attitude, they argue that the denudation of these high peaks is caused by infrequent, high-magnitude mountain collapse. This research also shows that in the aftermath of this event, the downstream fluvial system for >150 km (maybe as far as 300 km downstream) became saturated with the easily eroded mass-wasting detritus for more than a century. Further, the authors speculate that permafrost and muted chemical weathering at these high altitudes effectively strength, or at least not weaken, the material strength needed to maintain such high, steep mountains that intermittently fail.

Major comments

This is an interesting, well-documented, and thorough investigation of a high-magnitude rockslide and its downstream impacts. While rapid, pulsed deposition of the downstream Pokhara Formation during the medieval period was documented (Schwanghart et al., 2016), the source of this material and precise mechanisms remained speculative. This study provides the first evidence that

the catastrophic failure of a mountain peak altered the dynamics of the catchment, resulting in the deposition of the Pokhara Formation affecting the river much farther downstream, possibly to the Gandak fan ~300 km downstream of where the slope failure took place.

This study is of wide interest for several reasons. First, it documents and describes an underappreciated mechanism of denudation in tectonically active mountains, challenging aspects of the so-called glacial buzzsaw hypothesis. Second, it highlights the impact of such events on the downstream system. There is a growing body of literature and recognition of the impact of these rare, high-magnitude events on downstream river systems (e.g., Scherler et al., 2016; Bruni et al., 2021), but nothing of this scale. Third, the results emphasize a rare but important hazard for communities in the Himalaya. This is not only in terms of the impact of the slope failure itself, but the potentially disruptive impact of the ensuing valley aggradation. Finally, this study is exciting in highlighting the demise of a likely 8 km peak in the Himalaya.

While I do think that this study advances our understanding of mountain denudation in tectonically active settings, I do have some comments and suggestions I would like the authors to address prior to publication. Further, I provide line-by-line comments to streamline the revision process that I hope the authors find helpful. I think that what I am requesting amounts to minor to moderate revisions.

Upon reading this study, I went back and reread Schwanghart et al. (2016). It is very clear that this study is distinct from Schwanghart et al., but I was hoping for some more discussion of the implications of this study on Schwanghart et al.'s results. Specifically, I found evidence for pulse aggradation of the Pokhara formation presented in Schwanghart et al. to be convincing. From this study, pulsed aggradation documented by Schwanghart et al. is genetically linked to the rockslide deposit such that sediment evacuation in the aftermath of the slope failure is unsteady in time. I do not think that the authors need to dive into this in detail, but I think it is worth acknowledging this presumably unsteady catchment response to the high-magnitude perturbation because it is consistent with the results of Scherler et al. (2016) and Bruni et al. (2021) and has implications for the ensuing hazard cascade that can affect downstream communities for years to perhaps centuries following the initial event.

I felt that there was a missed opportunity in this study to highlight the importance of this event (and possibly others) to civilization in the Himalaya. I realize that if such an event were to happen today, that Pokhara would be "completely wiped off the map" as stated in the manuscript; however, I kept thinking that Pokhara, the second largest city in Nepal, wouldn't be there if it were not for this event! The deposit clearly provides flat surfaces for infrastructure, impounded lakes in backfilled tributaries, and, owing to the rapid development of fertile soils, allowed for expansive agriculture. Absent this event and the associated deposition of the Pokhara formation, I doubt a population of nearly half-a-million people could thrive in this valley. This seems a critical and important implication of this work that is perhaps as relevant and headline-grabbing (maybe more so) as the hazard implications. I find the idea that this event set the stage for the second-largest population center in Nepal exciting and thought provoking.

I found the evidence presented to largely support the authors' interpretations. However, there is one additional analysis that I think is needed to fully support the authors' proposed mechanism for what might be termed "climate-related strengthening (or non-weakening) of high-alititude peaks". The authors conduct a nice back-analysis for the apparent cohesion of the Anapurna range and find values that exceed 1 MPa. I found this convincing. The authors then use results from previous studies that show in fluvially-dominated regions, studies with similar experimental design (i.e., back analysis with slope stability equations) find values generally <0.1 MPa. While this is correct, the slope stability equations used here and in those previous studies are different (infinite slope stability equations for Gallen et al., 2015, finite slope stability equations here). Furthermore, the studies were conducted in different settings (Gallen et al. in Longman Shan, here in the Himalaya). This is to say that there are important differences between these studies that might raise

questions about some of the conclusions regarding higher-strength hillslopes at higher elevations in the Himalaya. Because of these lingering questions, I think it would strengthen the authors' arguments if they conducted a similar back-analysis study of a subset of unglaciated hillslopes at lower elevations in the Himalaya similar to the analysis presented in Extended Data Figure 8. I emphasize that I think the results will show hillslopes are generally weaker in the fluvially-dominated, lower elevation landscape (this is because the topography is so much steeper at high elevation), and will, therefore, better support the authors' interpretations. I am only requesting this additional analysis because I think it will make the arguments related to permafrost and lack of chemical weathering stronger as it is more of an "apples to apples" comparison of results.

While somewhat less important, I think the description/presentation of some of the data can be improved. Specifically, the presentation and description of the geochronology in Figure 2 could be better. In looking at the figure and reading the figure caption, it took me a while to decipher each panel (e.g., the shaded gray pdfs are calibrated pdfs for individual samples). I also had trouble with the IRSL panel. I produce and work with luminescence data, but it wasn't obvious to me how this figure support some of what was stated in the manuscript. I'm sure that it does, I just struggled with how to interpret this panel and found the related Extended Data Figure 4 easier to understand. I think with a little more information in the figure caption, the reader would have an easier time with this important figure.

If it isn't clear from what is above, I think that this is an excellent and thorough investigation. It provides new insight into landscape evolution, denudation, sediment transport, and natural hazards in one of the most rapidly deforming mountain ranges on earth. I also think it is eye-catching work given the "death" of an 8 km peak, the resulting hazard implications, and the fact that the second-largest city in Nepal probably wouldn't exist if this event didn't happen.

All the best,

- Sean F. Gallen

Beyond these comments, I provide some line-by-line comments to help during the revision process.

Line-by-line comments:

L43: I think this should be "~23 km³" instead of "~23km³". If I am correct, there are a bunch of spaces needed between numerical values and their associated units throughout the manuscript.

L44: does "likely" work better than "most probably"?

L47: I know this is quickly handled in the introductory part of the main text, but I found the phrase "growing indefinitely" here to be a little dramatic. The concept of threshold hillslopes, as articulated in the introduction, as been around longer than the glacial buzzsaw hypothesis (e.g. Schmidt and Montgomery, 1995), so I found this conceptual argument to be overstated. Can this please be rephrased so that the general reader isn't led to believe that earth scientists think mountains might grow indefinitely?

L51: "farther" rather than "further".

L52-53: and the rise of civilizations? Well, that is perhaps overstated, but I don't think Pokhara, at least as a large population center, would exist if this event had not occurred. This seems an important implication of this work that seems worth highlighting given the readership of this journal.

L63: "occurs" rather than "occur"

L67: "In such a conceptual model".

L70: "Such a model"?

L72: "notion of the glacial buzzsaw".

L76-77: This only considers a component of what we know about landscape evolution, particularly in rapid uplifting and denuding settings. As acknowledged in the next paragraph, mechanical limits to relief are important and have a longer history in our understanding and conceptualization of landscape evolution than "the glacial buzzsaw". I am a little uncomfortable with this framing. I don't think glacial buzzsaw people (perhaps better phrased as proponents of the glacial buzzsaw) think that the glacial buzzsaw is the only denudation mechanism operating at high altitudes. To put this another way, I don't think anyone would argue that mountains grow indefinitely. Please consider rephrasing this to be a little more faithful to how earth scientists think about landscape evolution from a more holistic perspective.

L107-109: Can the reader be pointed to figures that show this?

L115-116: I couldn't find the locations shown in Extended data figure 2 in SI-1.

L124: Extended data figure 5 is presented before ED figures 3 and 4.

L139: maybe "generally consistent" rather than "very consistent"?

L146-148: It wasn't clear to me how to pull this information from the inspection of the IRSL data shown in Fig. 2a. I think some more detail is needed in the figure caption.

L149: "7 plant debris samples"?

L167: Why 82%? I realize this can be added to the methods or simply rephrased here. My confusion is that I didn't understand if this was a result (i.e., 82-100% of slopes are stable when using 1-2 MPa) or if 82% was a lower threshold used to determine that 1-2 MPa was a reasonable value for apparent cohesion.

L170: "the" rather than "these"?

L172: What are the results for 1 MPa? This analysis assumes the stronger value of 2 MPa, correct?

L185: "after" rather than "posterior" might be easier for the reader.

L201: What defines "oversaturated"? I recommend using a different word to avoid ambiguity.

L209: "farther" rather than "further".

L231-232: See my major comments above. I think this argument is fine, but it could be much stronger with an independent analysis of lower elevation, fluvially-dominated parts of the Nepal using the same methodology. Doing such an analysis would make the arguments here more persuasive.

L255: see the comment above about "oversaturated".

L258: delete "have"

L262-264: This is probably correct, but would Pokhara even exist without this event? It seems

relevant to highlight that the depositional products from this event are critical to the thriving population and economy there. That seems like an attention-grabbing implication of this study that is worth emphasizing.

L273: note the citation style.

L618: "dominate" rather than "dominates"

L646-647: delete "will" and replace "rely" with "relies".

L 683: add "the" between with and previous.

References cited in the review that are not in the manuscript:

Bruni, E.T., Ott, R.F., Picotti, V., Haghipour, N., Wegmann, K.W. and Gallen, S.F., 2021. Stochastic alluvial fan and terrace formation triggered by a high-magnitude Holocene landslide in the Klados Gorge, Crete. *Earth Surface Dynamics*, 9(4), pp.771-793.

Scherler, D., Lamb, M.P., Rhodes, E.J. and Avouac, J.P., 2016. Climate-change versus landslide origin of fill terraces in a rapidly eroding bedrock landscape: San Gabriel River, California. *GSA Bulletin*, 128(7-8), pp.1228-1248.

Schmidt, K.M. and Montgomery, D.R., 1995. Limits to relief. *Science*, 270(5236), pp.617-620.

Review of Lave et al., "Demise of a Himalayan giant and the dramatic fate of the highest peaks on Earth"

This submission provides one of the first (or perhaps the first) rather thorough, quantitative, and convincing analyses of what processes set limits on the heights of Earth's highest mountains. For decades, alpine geologists and geophysicists have recognized that a competition between rock uplift and erosion must modulate the height of mountains in active orogens, such as the Himalaya. But, a largely unresolved conundrum has existed, because, whereas tectonically driven rock uplift persists irrespective of ambient climate, the character of erosion changes as a function of that same climate. At lower altitudes on these high mountains, glacial scour, rivers, landslides, and rainfall erode the bedrock and can, in theory and likely in reality in my opinion, balance the long-term rate of rock uplift. However, in very high mountains, such as for the 8000-m-high peaks of the Himalaya, the temperatures above 7500 m altitude almost never rise above freezing, such that freeze-thaw erosive cycles are suppressed. Because frigid air at high altitude holds little moisture, snowfall and ice accumulation are minimal. Erosion by glacial sliding or wind scour is negligible, because glacial ice is frozen to the bedrock, such that little to no glacial sliding or ensuing erosion occurs, whereas the thin air makes wind scour less effective. So, why don't peaks keep growing higher and higher? In this paper, Lavé et al. propose an appealing possibility (and, in my opinion, the most likely candidate) for sustaining this "rock uplift vs erosion" balance by a mechanism that has been rarely been proposed or documented: episodic, large-scale collapse of elevated, frozen summit pyramids. Documenting the episodic and abrupt collapse of unstable summit "towers" in Nepal's Annapurna Range is the focus of Lavé et al's paper). The authors argue that wholesale failure of unstable, perennially frozen summit "towers" is a key, but largely ignored, component of sustaining a long-term topographic balance between rock uplift and erosion. extending into the perennially frozen high elevation occurring due to the finite strength of the rocks that form the "vertically extruded" mountain edifice rising above ~8000 m.

In this submission, Lavé et al provide the first robust documentation (at least, that I know) of a massive collapse of a Himalayan summit that previously rose above 8000 m and, thereby, into this zone of perennially frozen conditions where effective physical and chemical erosion at local scales is largely absent. Instead Lavé et al that a mega-landslide with a volume exceeding 20 km³ removed the former >8000-m-high summit pinnacle of Annapurna 4 about 1000 years ago. The combination of (1) the distinctive chaotic composition of the resulting rockfall deposits, (2) the atypical erosional landscape (numerous penitents) flanking the high valley walls, (3) the downstream stratigraphy resulting from subsequent erosion of the landslide mass and resultant pulsed, rapid downstream aggradation, and (4) the quite high-resolution digital topography of the region together enable a remarkably well constrained reconstruction of the initial landslide emplacement, its aerial extent and lateral margins, its initial volume, the subsequently eroded volume, and the likely landslide source area. Each of these characteristics are remarkably well estimated. The authors are quite upfront about where the uncertainties lie, and as a consequence, they are (in my opinion) conservative in their estimates of volume, reconstructed source areas, and landslide dynamics. On the other hand, the (1) local stratigraphy, such as the chaotic and highly eroded landslide deposits, (2) the presence of some obvious candidates for

large-scale sliding surfaces in the preserved bedrock, (3) localized downstream regions of very rapid sediment accumulation with debris eroded from the initial landslide deposits, (4) a suite of self-consistent radiometric dates, and (4) a convincing reconstruction both the sequence and effects of this mammoth, high-altitude rockfall and its subsequent erosion and re-distribution underpin a convincing reconstruction of the main characteristics and character of this unusual (or at least, rarely observed) landslide/summit-collapse event.

The rather ubiquitous presence of a perennially frozen zone in the Himalaya above 7500-8000 meters, the persistent convergence of the Indian subcontinent with the Himalaya with the sustained rock uplift that this convergence generates, and the fact that these very high peaks exist in a frozen zone where typical alpine erosion processes (glacial scour, freeze-thaw cycles) are largely ineffective seems to dictate that large-scale (catastrophic) collapse of high summits is a likely (and, it seems to me, the most viable) explanation for the topographic evolution of the High Himalaya. I predict that this well-documented, but provocative paper will be well cited and will set the stage for innovative reconstructions and re-interpretations of collisional mountain belts through earth history.

Reviewer's note: Because this paper is written by a non-native English speaker, I have spent some time editing the text (using "track changes") to conform with more standard (and slightly more parsimonious) English usage. Hopefully, these suggested changes will be of use to J Lavé et al in subsequent revisions prior to publication in Nature.

Author Rebuttals to Initial Comments:

Referees' comments:

Referee #1 (Remarks to the Author):

please see three files attached

Doug Burbank's review :

We answer below to the most important comments written directly in the margin of the pdf by the reviewer. In our revised manuscript, we fully considered and implemented most text suggestions and syntax improvements, for which we thank him warmly, as well as for his very supportive review.

- Line 103 (Line 259 in the revised version including the revision tracking): It might be useful to have a supplementary figure depicting mean slope angles via a color code that would illustrate the unusual steepness of these source areas.

The Extended data figure 8 provides some hint about the location of the steep slopes. Obviously this does not directly correspond to the slope angle but it provides some combined information on steepness and length of the landforms. Given that we are already presenting many extra figures in the extended data section and in the Supp. Information, we prefer not to add a map that is not essential to our argument, although it is probably didactic.

- Fig. 1:

I recommend putting the ABCD labels in white boxes in the upper left of each figure to which they pertain.

Done.

I would also add some arrows and a label « penitents » pointing to the relevant topography in the 1D image.

The penitents are numerous and visible on the panel 1d and we fear to overload the figure with extra arrows.

I would also enlarge the arrow indicating « south » for readers in 1D.

Done.

In 1C, what does the label « fig. 1D » indicate ? (the actual 1D panel covers a much larger area – so it's confusing). I now recognize that it is supposed to represent the « eye » of the viewer for fig. 1D. But, it is too small to see clearly.

Ok, done - we also slightly modified the symbol.

Make it bigger and more explicit. Is it possible to outline and label the « whitish breccia » in 1D ?

Again, we fear to overload the figure and loose the “aesthetics” of the picture, but we put, however, a discrete outline if it can help the reader to figure out the breccia extent.

Make the 2-km scale a bit larger and thereby easier to see.

Done.

- Fig. 3:

3a. My sense is that that text in the legend of 3a is too small. There's space to make in 50% larger (and more legible).

The symbols and text for the summits of Annapurna 3 & 4 are difficult to see and should be enlarged. These are important landmarks, so should be clearly visible. Also abbreviations such as MCT or STD need to be defined for readers. Also, what do « S » and « K » represent ?

Done.

3b. Add a label for today's Annapurna 4 (and 3 ?) Also I am guessing that the grayed region above the more obvious topography represents the hypothesized reconstructed height & profile prior to the landslide. If so, label it. If not, what is this gray area ?

We specify it now in the figure caption.

The aim of this figure was to give an idea of the average geometry of the paleo-summit but also of the average eroded volume by making the failure surface visible through transparency.

Given the scale shown here, it's hard for me to envision that the gray area by itself could encompass 23 km^3 . I am guessing that the authors interpret the slide to have extended laterally to the south where the orange-red arrow is now. Is there an assumption that there was no major cirque present in this location prior to the collapse, such that the volume of the current cirque/basin is added to the extra height removed in the summit collapse ?

The greyed summit corresponds indeed to $\sim 23 \text{ km}^3$. A rough calculation can be done easily from the figure: on fig 3a or b, the max length of the failure is 6.5 km; on fig. 3c, the failed volume presents a section of $\sim 4.5 \text{ km}^2$. Multiplying the two estimates gives a volume of 29 km^3 . Obviously this value overestimates the real volume because the sections at the two ends of the failed volume present a lower area, but it shows that we have roughly the correct order of magnitude from visual inspection of figure 3.

What does the line of black dots represent ?

The perspective figure was made with Matlab by adding a transparent topography of the paleo-summit on top of the initial simplified topography in the cirque (with the actual topography as an approximation for the rupture surface). We don't really control what is going on at the junction between the two surfaces: the line of black dots is just an artefact, we were not able to remove with Matlab. However, it corresponds to the trace of the intersection of the failure surface with the mean palaeo-topography. We now highlight this trace with a pink line, keeping the same colour code as in Fig. 3a

The cross-section location (in a and c) should also be depicted in (b).

Added as suggested.

I don't understand the black scale bars in (b) and how they relate to the spacing of vertical gray lines in the background of « b ». That is to say that « 2 km » horizontally seems to be different in the background on the left versus on the right (where the vertical gray lines are more closely spaced). It seems to me that there should just be scale bars for 1 km x 1 km on back left and back right of the panel because one scale doesn't fit both sides, in my opinion.. (Maybe I am missing something, but this figure is unclear.

The grid behind the 3-D topography has a resolution of 2km (horizontal) by 1km (vertical).

We now draw scale bars in all 3 directions to avoid ambiguity.

Is the hummocky topography on the valley floor (to which the orange arrow points) a landslide remnant ? If so, perhaps add a label and arrow.

This “hummocky topography” corresponds to a few rocky outcrops in the middle of the cirque (see Extended data fig. 6a), on the edges of which the mesh of the interpolation procedure is based (under ArcGis by building a triangulated irregular network). At the level of these outcrops we have high frequency information, whereas outside, within the interpolated area, the topography is much smoother.

3c. This figure is quite clear and useful with respect to present topography. But, you hypothesize the pre-collapse landscape to have extended upwards to >8000m. Wouldn't it be useful to sketch in and label a dashed outline of this proposed collapsed region in this cross section ? I know it's speculative.

We are not fully sure to understand what the reviewer would like to see. Another cross section than the one presented here, but that would go through the paleo-summit? If so, it was our initial idea, but there are 2 difficulties, which made us to prefer this section:

1. The paleo-summit was farther to the SE and we preferred a section that goes through the middle of the failure and as far as possible through the middle of the deposit.
2. As our random generation of palaeo-topographies produces a peak with a (slightly) different location for each topography, when we average the various acceptable topographies, this smoothes out the ridge and the peak elevation of this average topography, which is then lower than the average of the palaeo-topography maximum. We could have chosen one acceptable topography instead of the mean one but it would be arbitrary.

We now display the trace of the cross section AA' to inform the reader on the fact that the profile does not go directly through the paleo-summit.

But the 23 km³ has to come from somewhere and the volume suggested in this cross section seems too small to me.

See our answer above.

3d. This figure is confusing to me. Is there any vertical scale ? Does the apparent absence of rockfall volume below 7900 m mean that no significant rockfall contribution came from (or occurred) below this altitude ? Or, are these vertical gray bars intended to extend to the bottom of the diagram (behind the other data) ? Is there a vertical scale ? or does the height of each bar of different color represent a different volume ? I don't know what a volume of

« hillslopes stability » means. Is this quantity somehow depicted in this figure ? What does « hillslope stability » mean in this context ? Is « hillslopes stability » intended to indicate « stable hillslopes » ?

The histogram represents some kind of probability density function on the maximum peak elevation (or summit point) of the reconstructed paleosummit.

The gray histogram corresponds to the conditional probability with the only condition that the palaeosummit respects the volume $V=20-26.5\text{km}^3$.

The pink one corresponds to the conditional probability with the two conditions that the paleosummit respects the volume $V=20-26.5\text{km}^3$ and that the slope flanks are stable (or would not erode by small to medium size landslides before the massive rockslide) .

So the fact that there is an almost absence of values below 7900 m means only that the probability that the paleosummit was below 7900m is quite low, which is not surprising since its height must be larger than the present peak elevation of the Annapurna IV at 7525m.

We tried to clarify both the main text and the figure 3d caption.

Referee #2 (Remarks to the Author):
Review by Niels Hovius
11.10.2022

All mountains are ephemeral. What, then, brings down those that stand tallest of all? Because high means inaccessible, we have, in fact, little scientific information and insight to answer this question. Lavé and co-authors present an exquisite piece of forensic geomorphology and sedimentology, supported by thorough geochronology, to fill this gap, at least in part. The study documents what may have been one of the largest known collapses of subaerial topography. It substantiates volume, source area, sediment redistribution, and timing of events. In doing so, it builds a powerful case for the role of very large landslides in limiting the height of very tall mountains, raking into the domain where low temperatures slow down or stop most other processes. Lave and colleagues argue that what may be called cryostrengthening suppresses progressive mass shedding, leading to a buildup of metastable topography on a large scale at elevations high above the glacial Equilibrium Line Altitude, and leaving room only for catastrophic adjustments of mountain height. In doing so, they develop a new paradigm to complement influential concepts of a glacial buzzsaw and frost cracking, both of which are thought to reduce and limit mountain topography. The proposed concept is a plausible explanation of the reconstructed history of the Annapurna IV, which may until a fateful day in the 12th century AD have been the world's 15th 8000 m peak. While the authors are careful not to engage in adventurous extrapolation, their concept may well apply to many other eye-catching mountains protruding far beyond the trim line of glaciers. Instead, they speculate about the impact of similar episodes on downstream communities, arguing that effects may well propagate far beyond the mountain front and into heavily settled foreland landscapes. A salutary tale, worth keeping in mind when planning the development of mountain areas.

I find the study technically convincing, conceptually important, and of interest to a wide readership. Moreover, the starting question has an innate appeal. What more is there to desire for those who side with Mallory, who professed to wanting to climb Everest "Because it's there".

We thank the reviewer for his very positive and constructive review, and have carefully considered the raised points below with manuscript revisions accordingly.

That said, I have some comments on this study.

1) The volume reconstruction hinges on the remote identification of a few deposit remnants at the fringes of the Sabche basin. The authors infer that the entire basin was filled to the brim, with an approximately flat top surface. The alternative of a massive avalanche running up the sides of the basin and leaving deposits above the fill in the central depression is not mentioned. This scenario might have involved significantly smaller rock and debris volumes. I urge an exploration and discussion of this alternative in order to have a better sense of the range of possible volumes.

We acknowledge that the justification for the choice of a sub-horizontal surface was minimal in the initial manuscript version. We have made an effort to further justify this choice by formalising a number of observations (edits in the main text plus one table and one figure with 2 panels in the Supp. Information SI-4) . To summarise, although the diversity of natural phenomena (and here the rock avalanche confinement by a closed glacier cirque is quite

uncommon) means that one can rarely rule out a scenario 100%, we are convinced that the surface of the centre of the deposit was not affected by a depression:

1. Examples from the literature show that the centre of the rock-avalanche deposit is either higher than the downstream part of the deposit (low kinetic energy of the avalanche) or higher than the upstream part of the deposits (high kinetic energy leading to a runup of the mass on the opposite flank). Although our search for examples in the literature is far from being exhaustive, we did not find any examples that actually show a U-shaped deposit (i.e. a centre lower than both the upstream and downstream part).
2. The path of the glacier descending from Annapurna III and running along the entire western edge of the cirque shows that if a central depression had existed, the glacier would have flowed towards the centre of the cirque instead. Its geometry suggests that it was rather deflected by the presence of a convex depositional surface.

In our initial reconstruction, we had attributed a symmetrical error ($\pm 3\text{--}4 \text{ km}^3$) to the reconstruction of the base of the deposit for simplicity (whereas it is disymmetrical, with a greater uncertainty concerning the upper estimate of the volume of the deposit because the position of the substratum under the glaciers in the NE part of the cirque is not well known in the downward direction). We assigned this symmetrical error to implicitly take into account the uncertainty on the top surface reconstruction, and particularly a possible overestimation of the position of the upper surface in the SE part of the cirque at the transition to the Upper Seti valley. We have now made an effort to decompose the error on these two surfaces, even if in the end we retain an error similar to that proposed in the initial manuscript on the final volume of rock.

We also tested the sensitivity of our results with an error bar of 4 km^3 instead of 3 km^3 , but did not see any significant changes in the results ($<3\%$ difference in the % of realisations with a peak $> 8000\text{m}$ for example).

2) They appear to implicitly reject the glacial buzzsaw concept, because it does not permit the emergence of very high peaks. It may be better to argue that while the buzzsaw operates in glaciated mountain ranges, it evidently does not catch all rising peaks. Ones that make it through the sawing window are home free but for the large-scale collapses that inevitably happen when mountain relief can't be sustained by their rock mass. This requires some rephrasing

We agree and have slightly modified the sentences in the introduction, and in addition we have now added a full discussion to precise that the glacial buzzsaw model is likely valid to describe the mean elevation of mountain ranges, but, in contrast with Egloffm et al.'s (2009) view, fails to limit the maximum elevation of mountain range affected by sustained rock uplift. We thank the reviewer for pushing us to clarify this point which was ambiguous in our previous manuscript version.

3) The descriptive sedimentology for the Sabche basin breccias is not elaborated in the supplementary materials. While the main text is clear, it constitutes a sequence of statements that are not underpinned by photographic material or logs. Can the authors add the best of what they have on this to the supplementary file?

It seemed to us that we had previously made a particularly targeted choice of photos in Extended Data Fig. 2 to support the sedimentological description. If we go back to the text we had the following correspondence:

« finely-crushed matrix-supported breccia with centimetric to decimetric angular clasts from various TSS carbonate lithologies » (= Extended Data Fig. 2G).

“ The coarse elements are more abundant in the upper part of the deposit and reach sizes of several meters.” (= Extended Data Fig. 2B and C ; otherwise it is difficult to show a picture with a complete section going from the bottom to the top to really demonstrate)

“ The breccia color is quite uniform (white to grayish) except locally with yellowish breccia units reminiscent of the yellow limestones of the Annapurnas” (= Extended Data Fig. 2C and D)

“without apparent unconformities despite incomplete mixing” (= Extended Data Fig. 2B and C)

“ the presence of internal shear zones” (= Extended Data Fig. 2D and 4a)

“ and of rare jigsaw facies” (= Extended Data Fig. 2E),

“and a reverse granular grading toward the top of the fill” (= Extended Data Fig. 2B and 2C)

For a question of length and cumbersome text, we had not included so many references to each specific figures but only given a mention to the general figure (Extended Data Fig. 2).

We have tried to be more indicative in this new manuscript version, and we also added a few additional figures in the Supplementary Information.

4) Do the authors think that the Sabche case is an example of a mechanism that may apply to many high mountains? They are carefully sitting on the fence on this question, but invite it with the opening paragraph of the main text. I think that a return to that question at the end of the paper will help round the manuscript and instigate a wider investigation of the generality of the concept.

The reviewer is right that if we want to close on the introductory question, it would be good to take a step back and broaden the spectrum of our analysis in the discussion.

For that reason, we modified the order of our sections in the discussion in order to end with a return to the main introductory question. We also expanded the discussion on this topic that we think would be of high interest for the readers.

Nevertheless, a transposition of the mechanism proposed in the Annapurna region to the entire globe would require time and a dedicated study (analysis of slopes, even of the cohesion of the massif, analysis of uplift rates, etc.), especially if we want to take a quantitative approach to really determine the threshold values of tectonic uplift rate, a positive feedback leading to the growth of glaciated peaks as described in our final discussion.

Consequently, we have attempted to answer this question in an essentially qualitative manner: 1. by proposing a conceptual model, also in the form of a new key figure (fig. 5), that makes it easier to explain and illustrate the evolution of mountains towards high peaks protruding largely above the ELA in areas of sustained rock uplift.

2. by proposing a set of mountain ranges around the globe presenting a high relief above the ELA and that could potentially behave like the Annapurna summits, with the prevalence of very large rockslides as a major mode of erosion for the glaciated ridges.

5) Some minor reorganization of materials in the final sections of the manuscript and the removal of a few redundant sentences may help further improve what already is an eminently legible text.

In addition to these points, I have left a string of comments in the left margin of the pdf document. I have also left suggestions for minor edits littered across the text. I hope that the authors will find them useful.

We are very grateful to the reviewer to have proposed many suggestions to improve the style and the syntax. We followed most of his propositions.

I ask that you please disclose my name to the authors. If we don't do double blind, then we do double seeing.

Thank you for asking me to review this manuscript. I do hope it will make it to print in Nature.

Niels Hovius

[please also see one file attached]

We answer below to the most important comments written directly in the margin of the pdf by the reviewer, all other raised points and suggestions have been fully implemented.

* (line 72 _ Line 180 in the revised version including the revision tracking) Suggest: a comprehensively operating glacial buzzsaw

This wording is somewhat important. I don't think that you want to completely reject the buzzsaw idea, but it does not explain all topography. By the way, I don't think that the buzzsaw paradigm was ever meant to exclude all topography high above the ELA. Some milder wording would be good here.

We modified our sentence as suggested, and expand this issue in the final discussion.

* (line 75 _ Line 185) There is a conflation here of glacier cirque headwall erosion and peak erosion. The link between the two should be demonstrated or surmised.

The sentence was modified as suggested by the reviewer 1. Furthermore, in former lines 68 and 69 (lines 177-178 in the the revised version including the revision tracking), the link between the headwall and the peaks is implicitly made. This link is also done now in the abstract (line 103), so that we felt that there is no need for further clarification or demonstration.

(line 77 _ Line 188) To avoid mental images of beanstalk mountains, it is important to mention the geophysical limits on mountain height here. A sentence about lithospheric strength is called for here.

This question is more complex than it seems, because it is particularly a problem of wavelength of the topographic anomaly. In theory one could create very high mountains supported by the rigidity of the upper crust, as long as the mountain is small. Indeed, at long wavelengths, especially if we introduce a temperature dependence of the viscosity, the models show a mechanical limit, but this will concern more the average altitude of the mountain range or the orogenic plateau, than an isolated summit. At this stage, it seems a bit complex to go into these considerations without diluting the focus of the paper: we have preferred to reformulate using a less dramatic or definitive formulation.

* (line 86 _ Line 199) The coseismic landslide inventories are from the same remote sensed sources as the landslide inventories mentioned earlier in the paragraph. Some coseismic inventories cover higher peaks and glaciated terrain. If the mapping is complete, then the lower landslide rate at high elevations points at the operation of a different mechanism, or at a different size-frequency scaling at high elevations.

The reasoning is incomplete here.

Size-frequency scaling may act as a segway into the next paragraph and section.

The term “the specificity “ in the initial sentence “ both the effectiveness nor the specificity of this erosive process” was meant to suggest the possibility of a different mechanism. We have rephrased the sentences and followed the reviewer’s suggestion about the transition to the next paragraph.

* (line 118_ Line 314) The descriptive sedimentology is clear, but purists would ask for any sedimentary logs, photo logs, or more detailed descriptions in supplementary information.

The first author unfortunately did not have the opportunity to spend enough time in the Sabche cirque to draw a full sedimentary log (this place is remote and can only be reached by helicopter, even for mountaineering expeditions). In addition, even if you spend some time in the cirque, we don't think it would be easy to do so as the breccias are mostly outcropping along the very steep ($>60^\circ$) walls of the penitents.

We made a selection of pictures taken in front of outcrops or from ultra light plane, and added them in the Supplementary Information SI-1.

* (line 128_ Line 328) Is it possible to exclude a genesis of this deposit separate from the cirque fill? The writing assumes the two are linked.
The evidence comes from dating: point ahead.

The reviewer is right: the evidence come mostly from the dating, but also from carbonate content and facies similarities. In the absence of continuity of the deposit because of intense erosion in the upper Seti valley, we can only speculate that the different remnants are associated to the same deposit. On the other hand, these spurs of breccia in the upper Seti valley are unique in all the surrounding valleys: the uniqueness of these spurs as well as of the rock-avalanche cirque deposit is a rather supportive argument for a common origin if one considers principle of parsimony.

It would have been more rigorous to present the facies and the dating first and then to conclude that we consider that the two breccias are of similar origin. However, this is less compatible with linear reading for the reader and a compact form: for that reason we had chosen this kind of presentation that anticipates the dating presentation, and we had added on former line 153 (line 391 in revised version) the sentence “confirming the monogenic nature of the rock avalanche between Sabche and upper Seti”. We preferred to keep it as it.

* (Line 136 _ Line 371): Suggest: compare somewhere with other giant paleolandslides, for example Bogd, Mongolia, 50 km³

Given the paper focus on the deep rockslides in Himalaya, we prefer to restrain our comparison to the Himalaya setting (The case of the giant landslide in Mongolia is very different as it is located along a shallow slope and above an evaporitic level.).

* (Line 145 _ Line 382): It is not clear from the writing here whether all samples were fully reset, or some were only partially reset.

Clarified as suggested.

* (line 215 _ **Line 660**) The phrasing suggests that the buzzsaw concept does not apply. Rather, the buzzsaw may not catch all topography in the window in which it is most active. Once through this window, mountains protrude into a very cold domain in which cold-based ice shields rather than attacks rock mass, so that ambient erosion rates are very low. Such summits inevitably become mechanically unstable, and may collapse in giant events, especially where the rock mass has a high competence.

We have largely rewritten this section and added a paragraph to precise our views as explained in the general comments above.

* (Line 235 _ **Line 722**): It may be better to talk about rock mass: Bedrock is read by many as intact material. What is meant here is a binding effect of ice in interstitial spaces, which lends strength to the fissured rock mass. The phrasing 'strengthening and/or absence of weakening' is confusing.

The concept makes sense. It might be termed 'cryostrengthening'. The contrast with the weakening effect of freeze-thaw in the frost cracking window, lower down makes for a striking message.

We indeed believe that two scenarios are possible: cryostrengthening but also absence of rock weakening. The intact bedrock is weakened only by fracture due to tectonic stress, but as soon as it arrives close enough to the Earth surface, the circulation of meteoric water at depth (>100m) can produce weathering of the rock mass, accompanied by an increase in the length of fractures or a decrease in their friction coefficient, so that the strength of the bedrock is decreased/weakened. If the ground is permanently frozen, these processes are interrupted and the initial strength of the bedrock is better preserved, or even strengthen in the permafrost domain.

In any case, we replaced bedrock by rock mass in the revised manuscript.

Line 239 _ **Line 728**: A metric would help here to understand the qualifications small and mid-size.

It is a qualitative term, and we would be hard to provide a precise cut-off between mid-size and large rockslides.

Line 240 _ **Line 729**: Not so much in favour of, but giving rise to. Using the argument developed above, the absence of regular smaller-scale mass wasting forces eventual large-scale collapse.

The sentence was modified as suggested.

(line 241 _ **Line 731**) The reasoning is focused on the Himalayas, from where the evidence is presented. The mechanism proposed here should apply globally. A short discussion and examples of mountains elsewhere that are prone to this mechanism would enrich the manuscript. Suggestion: Denali has the highest relief above the local ELA, maybe as a result of cryostrengthening.

This broadened scope has been added. Thanks for urging us to try to zoom out.

Line 245 _ **Line 453**: A comparison to earthquake energy release may help raise attention. This amount is in the M8 range!

We had thought initially about making such comparison, but in fact, according to the literature, just a small fraction of the energy is converted into acoustic energy (most of the energy is converted into heat or is absorbed by the fragmentation process), so that giving an earthquake magnitude would provide a wrong idea to people who could believe that the collapse generated a M8 whereas it was probably more like a M6.5-7.

Line 250: Some information is repeated here from earlier sections. Potential for redaction

We moved that section at the end of the one “Sediment export and downstream consequences” in order to avoid text repetitions.

Line 268 _ Line 591: It might be worth describing possible effects on channel stability and irrigation, affecting larger population numbers in the Indian foreland.

Done

Line 271 _ Lines 582 to 595: This first paragraph is about consequences, not causes. It may be better place in the section debris export.

Done with the reorganization of the discussion sections.

Line 276 _ Line 598: This paragraph is about predisposition rather than causes. It belongs in this section, but should be signposted correctly.

I suggest to add a sentence at the end, stating that the area has a clear disposition to large-scale slope failure. This also clarifies the contrast mentioned in the following sentence.

We have preferred to change the first sentence of this paragraph.

Line 291 _ Line 647: This statement may be correct, but it is not underpinned by reference or supplementary calculation.

We rewrote this part and also added a brief calculation in the Supp. Info SI-8.

Line 292 : Information about the base of the projected failure plane is only given in Fig.3. Given that this is a part of the argument here, essential aspects should be described in the main text. It might help to add this to the section 'Reconstruction of the collapsed paleo-summit'. This could also include a sentence on the geological formations mobilized by the collapse according to the reconstruction, which would help clarify the presence of some indicator lithologies even better.

We have added a sentence (Line 449 of the revised version with revision tracking) on this subject at the end of the section “Reconstruction of the collapsed paleo-summit”. However, as all the lithologies in the cirque are carbonate-bearing and potentially have quite strong variations in carbonate content, it is difficult to rely on this argument to validate our reconstruction (unfortunately).

Line 462 _ Line 1287: P: explanation missing - I assume Pokhara _ Why P in CAPS and k in lower case?

As indicated P=Pokhara and k=Kathmandu. We specified now “P=Pokhara” in the caption of 1a and 1b to avoid any ambiguity.

Line 472 (fig. 2b): The paleosol dates don't make sense if they are taken from within the Pokhara formation. Are they from sites buried by the Pokhara formation? Some explanation somewhere would be good.

Interpretation of the reviewer is correct. We clarified it in the figure 2b caption.

Line 493 (fig. 3a): The fill reconstruction relies rather crucially on the little top surface remnant at the southern edge of the Sabche basin. Can you link to a description in supplementary information?

The first author did not go up to this location and recognised the breccias on satellite images due to their typical colour and morphology. The additional figure SI-4-1c now shows that the breccias identified on the southern flank of the cirque in satellite images and during the microlight flight go almost as far as this summit surface and make the constraint brought by this small remnant less crucial.

Line 498 (fig. 3b): The transparent grey in 3b does not show the hypothetical reconstructed topography sufficiently clear.

The paleosummit has been displayed darker, and the failure surface delineated by a pink line, we modified several elements to make the figure, we hope, clearer.

Line 628 _ Line 1758: Is this zone shown on one of the main figures?

We added a sentence to clarify.

Line 641 _ Line 1805: The third criterium must not be met if the cryostrengthening mechanism operates.

“Smaller than the giant rockslide” does not mean that we consider only rockfalls or small/medium size landslides with shallow depths of failure. In the procedure (Extended Data Fig. 7), we consider slides with failure surfaces up to several km², i.e. sensitive to the average strength of the rock mass.

Referee #3 - Sean Gallen (Remarks to the Author):

Summary of study

Lave et al. present a comprehensive forensic analysis of a large rockslide and associated deposits in the Annapurna range in Nepal. Using field observation, a suite of geochronological techniques, and slope stability analysis, they argue for the catastrophic failure of a paleo-Annapurna IV peak that likely exceeded 8000 m elevation. Recognizing that the height of this paleo-peak and other >8 km giants in the Himalaya lie well above the equilibrium line attitude, they argue that the denudation of these high peaks is caused by infrequent, high-magnitude mountain collapse. This research also shows that in the aftermath of this event, the downstream fluvial system for >150 km (maybe as far as 300 km downstream) became saturated with the easily eroded mass-wasting detritus for more than a century. Further, the authors speculate that permafrost and muted chemical weathering at these high altitudes effectively strengthen, or at least not weaken, the material strength needed to maintain such high, steep mountains that intermittently fail.

Major comments

This is an interesting, well-documented, and thorough investigation of a high-magnitude rockslide and its downstream impacts. While rapid, pulsed deposition of the downstream Pokhara Formation during the medieval period was documented (Schwanghart et al., 2016), the source of this material and precise mechanisms remained speculative. This study provides the first evidence that the catastrophic failure of a mountain peak altered the dynamics of the catchment, resulting in the deposition of the Pokhara Formation affecting the river much farther downstream, possibly to the Gandak fan ~300 km downstream of where the slope failure took place.

This study is of wide interest for several reasons. First, it documents and describes an underappreciated mechanism of denudation in tectonically active mountains, challenging aspects of the so-called glacial buzzsaw hypothesis. Second, it highlights the impact of such events on the downstream system. There is a growing body of literature and recognition of the impact of these rare, high-magnitude events on downstream river systems (e.g., Scherler et al., 2016; Bruni et al., 2021), but nothing of this scale. Third, the results emphasize a rare but important hazard for communities in the Himalaya. This is not only in terms of the impact of the slope failure itself, but the potentially disruptive impact of the ensuing valley aggradation. Finally, this study is exciting in highlighting the demise of a likely 8 km peak in the Himalaya.

While I do think that this study advances our understanding of mountain denudation in tectonically active settings, I do have some comments and suggestions I would like the authors to address prior to publication. Further, I provide line-by-line comments to streamline the revision process that I hope the authors find helpful. I think that what I am requesting amounts to minor to moderate revisions.

Upon reading this study, I went back and reread Schwanghart et al. (2016). It is very clear that this study is distinct from Schwanghart et al., but I was hoping for some more discussion of the implications of this study on Schwanghart et al.'s results. Specifically, I found evidence for pulse aggradation of the Pokhara formation presented in Schwanghart et al. to be

convincing. From this study, pulsed aggradation documented by Schwanghart et al. is genetically linked to the rockslide deposit such that sediment evacuation in the aftermath of the slope failure is unsteady in time. I do not think that the authors need to dive into this in detail, but I think it is worth acknowledging this presumably unsteady catchment response to the high-magnitude perturbation because it is consistent with the results of Scherler et al. (2016) and Bruni et al. (2021) and has implications for the ensuing hazard cascade that can affect downstream communities for years to perhaps centuries following the initial event.

As recognised by the reviewer, it is not the purpose of the present manuscript to dive into the detail of the sediment export, nor on the exact sedimentologic nature of the Pokhara conglomerate. For the record, previous authors have had very fluctuating readings of these deposits which might suggest that they fall within the range of intermediate processes (sheet flows, hyperconcentrated flows, debris floods) between pure fluvial and pure debris flow, and make their interpretation hesitant. W. Schwanghart and co-authors interpreted these deposits as essentially fluvial ("We find that the major part of the Pokhara formation consists of fluvial facies ... Our preliminary results suggest rather repeated deposition largely accomplished by fluvial aggradation." in a 2013 congress abstract) before focusing instead on debris flows in Schwanghart et al., 2016.

The variety of facies shows in all cases that there was an alternation of several transport processes, with probably a fluvial redistribution of the deposits. If there were sedimentary pulses (at least for all facies of the debris-flow type), with roughly metric units and a mean aggradation rate of 1m/yr, it can be argued that these pulses were quasi-annual. It is therefore difficult to demonstrate that this deposition can really be characterised as unsteady, and not as the repeated arrival of sediment each monsoon. In any case the unsteady character seems less obvious than in the San Gabriel Mnts (Scherler et al., 2016) or in Crete (Bruni et al. (2021), where climatic conditions are Mediterranean with more episodic storms, and where sediment delivery by debris flows has been documented.

Furthermore, one of the arguments of Schwanghart et al. (2016) on the unsteady character stems from the so-called evidence of 3 pulse periods, based on 14C dating. Nevertheless, we consider that their interpretation is erroneous for various reasons, in particular the following one: the first peak around 1100 AD is related to the "charcoal" type samples which clearly show an inheritance/reworking problem as highlighted in an age vs stratigraphic position graph (see Fig. 2b). The third peak around 1350 AD is mainly related to the use of old dating (80s) made without AMS by radioactive counting (and which we decided to ignore because made with an old methodology, giving stratigraphically inconsistent results and with a large error bar) contrary to the measurements made more recently on AMS by Schwanghart et al. (2016), Stolle et al. (2017, 2019) or by ourselves. For us, only the age range between 1180 and 1300 AD is informative, and the notion of 3 large pulse periods does not really have any strong basis.

But again it is not the purpose of our manuscript to focus on the Pokhara conglomerates, to develop about the exact sediment transport dynamics or to discuss our interpretation differences with Schwanghart et al. (2016), and we prefer not to engage in discussion of this point even if the sediment export issue remains of interest for future discussions or studies (the re-incision of the filled valley and the landscape recovery are also quite interesting issues that could warrant some extensive discussion) .

I felt that there was a missed opportunity in this study to highlight the importance of this event (and possibly others) to civilization in the Himalaya. I realize that if such an event were to

happen today, that Pokhara would be “completely wiped off the map” as stated in the manuscript; however, I kept thinking that Pokhara, the second largest city in Nepal, wouldn’t be there if it were not for this event! The deposit clearly provides flat surfaces for infrastructure, impounded lakes in backfilled tributaries, and, owing to the rapid development of fertile soils, allowed for expansive agriculture. Absent this event and the associated deposition of the Pokhara formation, I doubt a population of nearly half-a-million people could thrive in this valley. This seems a critical and important implication of this work that is perhaps as relevant and headline-grabbing (maybe more so) as the hazard implications. I find the idea that this event set the stage for the second-largest population center in Nepal exciting and thought provoking.

We did not initially think about this perspective, and we thank the reviewer for bringing this to our attention. Although we are not sure that the stony marl-limestone soil of the Pokhara formation offers better agricultural yields than the soils of the surrounding hills, it is undeniable that this large flat area has allowed the development of the country's second largest city and its infrastructure in the 20th century.

We have now added a sentence in the discussion to emphasise this “beneficial” consequence of the giant rockslide event.

I found the evidence presented to largely support the authors' interpretations. However, there is one additional analysis that I think is needed to fully support the authors' proposed mechanism for what might be termed “climate-related strengthening (or non-weakening) of high-alititude peaks”. The authors conduct a nice back-analysis for the apparent cohesion of the Anapurna range and find values that exceed 1 MPa. I found this convincing. The authors then use results from previous studies that show in fluvially-dominated regions, studies with similar experimental design (i.e., back analysis with slope stability equations) find values generally <0.1 MPa. While this is correct, the slope stability equations used here and in those previous studies are different (infinite slope stability equations for Gallen et al., 2015, finite slope stability equations here). Furthermore, the studies were conducted in different settings (Gallen et al. in Longman Shan, here in the Himalaya). This is to say that there are important differences between these studies that might raise questions about some of the conclusions regarding higher-strength hillslopes at higher elevations in the Himalaya. Because of these lingering questions, I think it would strengthen the authors’ arguments if they conducted a similar back-analysis study of a subset of unglaciated hillslopes at lower elevations in the Himalaya similar to the analysis presented in Extended Data Figure 8. I emphasize that I think the results will show hillslopes are generally weaker in the fluvially-dominated, lower elevation landscape (this is because the topography is so much steeper at high elevation), and will, therefore, better support the authors’ interpretations. I am only requesting this additional analysis because I think it will make the arguments related to permafrost and lack of chemical weathering stronger as it is more of an “apples to apples” comparison of results.

We had started to think about this aspect in advance and to carry out an analysis of the cohesion in the unglaciated parts of the range, but for not expanding too much the initially submitted paper it seemed to us quicker to mention the values in the literature than to add an additional appendix.

However, the reviewer is right on the fact that the method he has used in the Longmen Shan is distinct, and might introduce some distinct estimate of the cohesion value. So to actually

compare apples with apples we agree that it is more rigorous to make this comparison with Himalayan landforms, preferably undergoing similar rock uplift rates.

We have therefore included this analysis, carried out south of the Annapurnas, in the fluvially-dominated zone while remaining in the region subject to strong rock uplift rate (but with different lithologies - the geology of the central Himalayas does not allow this to be done otherwise). As anticipated by the reviewer, the cohesion values found are indeed much lower than in the upper glaciated range (about 4 times lower) even if they remain higher than in the Longmen (~0.3 MPa vs 0.1 MPa). Another dedicated study, beyond the scope of the present manuscript, will be necessary to understand if the difference in cohesion between these two mountain ranges is due to a different tectonic context or if it is due to the difference of approach to estimate the cohesion values.

While somewhat less important, I think the description/presentation of some of the data can be improved. Specifically, the presentation and description of the geochronology in Figure 2 could be better. In looking at the figure and reading the figure caption, it took me a while to decipher each panel (e.g., the shaded gray pdfs are calibrated pdfs for individual samples). I also had trouble with the IRSL panel. I produce and work with luminescence data, but it wasn't obvious to me how this figure support some of what was stated in the manuscript. I'm sure that it does, I just struggled with how to interpret this panel and found the related Extended Data Figure 4 easier to understand. I think with a little more information in the figure caption, the reader would have an easier time with this important figure.

There was a small error in Fig. 2, because the y-axis values of the distribution had not been renormalized. Also the distribution curves in Fig. 2 and Extended Data 4f were not realized with the same software so that there was more smoothing on the curve of the Extended Data Fig. 4f. We have homogenized the two graphs (note that the distribution is given in yr^{-1} and kyr^{-1} in Fig. 2 and ED4f respectively). We don't know if it was part of the difficulties met by the reviewer, but we have also updated the caption of Fig. 2 in the main text for clarity. Note that it is not easy to present the IRSL data of Fig. ED 4f in Fig. 2, as the range of values of the x-axis (time) would have to be increased and the resolution of all other figures would be lost. We have also tried to add single-aliquot ages (the cumulative plot on Extended Data fig. 4f) but the graph is too compressed along the y-axis, and the result was not really readable. Instead, we have only tried to clarify the legend of the figure.

Relative to ^{14}C dating, we have replaced the stacked shaded gray curves by a black or gray profile. We hope it makes the graph clearer.

If it isn't clear from what is above, I think that this is an excellent and thorough investigation. It provides new insight into landscape evolution, denudation, sediment transport, and natural hazards in one of the most rapidly deforming mountain ranges on earth. I also think it is eye-catching work given the "death" of an 8 km peak, the resulting hazard implications, and the fact that the second-largest city in Nepal probably wouldn't exist if this event didn't happen.

Many thanks for this frank and enthusiastic appreciation of our work and manuscript.

All the best,- Sean F. Gallen

Beyond these comments, I provide some line-by-line comments to help during the revision process.

We have followed most of the reviewer's suggestions, and respond below only to those not related to syntax.

Line-by-line comments:

L43 (Line 112 in the revised version including the revision tracking): I think this should be “~23 km³” instead of “~23km³”. If I am correct, there are a bunch of spaces needed between numerical values and their associated units throughout the manuscript.

Done as suggested.

L47 (Line 115): I know this is quickly handled in the introductory part of the main text, but I found the phrase “growing indefinitely” here to be a little dramatic. The concept of threshold hillslopes, as articulated in the introduction, as been around longer than the glacial buzzsaw hypothesis (e.g. Schmidt and Montgomery, 1995), so I found this conceptual argument to be overstated. Can this please be rephrased so that the general reader isn’t led to believe that earth scientists think mountains might grow indefinitely?

We modified this sentence. However, the idea of infinite growth was just a view of mind, and the following section on

L52-53: and the rise of civilizations? Well, that is perhaps overstated, but I don’t think Pokhara, at least as a large population center, would exist if this event had not occurred. This seems an important implication of this work that seems worth highlighting given the readership of this journal.

See response to main comments.

L76-77 (Line 188): This only considers a component of what we know about landscape evolution, particularly in rapid uplifting and denuding settings. As acknowledged in the next paragraph, mechanical limits to relief are important and have a longer history in our understanding and conceptualization of landscape evolution than “the glacial buzzsaw”. I am a little uncomfortable with this framing. I don’t think glacial buzzsaw people (perhaps better phrased as proponents of the glacial buzzsaw) think that the glacial buzzsaw is the only denudation mechanism operating at high altitudes. To put this another way, I don’t think anyone would argue that mountains grow indefinitely. Please consider rephrasing this to be a little more faithful to how earth scientists think about landscape evolution from a more holistic perspective.

See response to main comments.

L167 (Line 404): Why 82%? I realize this can be added to the methods or simply rephrased here. My confusion is that I didn’t understand if this was a result (i.e., 82-100% of slopes are stable when using 1-2 MPa) or if 82% was a lower threshold used to determine that 1-2 MPa was a reasonable value for apparent cohesion.

It has been rephrased as suggested.

L172 (Line 447): What are the results for 1 MPa? This analysis assumes the stronger value of 2 MPa, correct?

We did not do this for 1 MPa for the following reason. As for 2 MPa, all regional slopes have a $SF > 1$, this represents an easy criterion to meet. On the other hand, for $C=1$ MPa, we could consider the criterion that the paleo-slopes have a distribution such that a maximum of 16% of them have $SF < 1$ (see new Fig. SI-6-1c), but this threshold depends on the exact extent of the areas used for back-analysis both at the regional and paleo-slope level, so this criterion did not seem so absolute.

L201 (Line 526): What defines “oversaturated”? I recommend using a different word to avoid ambiguity.

Done as suggested.

L231-232 (Line 714): See my major comments above. I think this argument is fine, but it could be much stronger with an independent analysis of lower elevation, fluvially-dominated parts of the Nepal using the same methodology. Doing such an analysis would make the arguments here more persuasive.

See response to main comments.

L262-264 (Line 582): This is probably correct, but would Pokhara even exist without this event? It seems relevant to highlight that the depositional products from this event are critical to the thriving population and economy there. That seems like an attention-grabbing implication of this study that is worth emphasizing.

See response to main comments.

References cited in the review that are not in the manuscript:

Bruni, E.T., Ott, R.F., Picotti, V., Haghipour, N., Wegmann, K.W. and Gallen, S.F., 2021. Stochastic alluvial fan and terrace formation triggered by a high-magnitude Holocene landslide in the Klados Gorge, Crete. *Earth Surface Dynamics*, 9(4), pp.771-793.

Scherler, D., Lamb, M.P., Rhodes, E.J. and Avouac, J.P., 2016. Climate-change versus landslide origin of fill terraces in a rapidly eroding bedrock landscape: San Gabriel River, California. *GSA Bulletin*, 128(7-8), pp.1228-1248.

Schmidt, K.M. and Montgomery, D.R., 1995. Limits to relief. *Science*, 270(5236), pp.617-620.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

Review of Lave et al., "Demise of a Himalayan giant and the dramatic fate of the highest peaks on Earth"

This submission provides one of the first (or perhaps the first) rather thorough, quantitative, and convincing analyses of what processes set limits on the heights of Earth's highest mountains. For decades, alpine geologists and geophysicists have recognized that a competition between rock uplift and erosion must modulate the height of mountains in active orogens, such as the Himalaya. But, a largely unresolved conundrum has existed, because, whereas tectonically driven rock uplift persists irrespective of ambient climate, the character of erosion changes as a function of that same climate. At lower altitudes on these high mountains, glacial scour, rivers, landslides, and rainfall erode the bedrock and can, in theory and likely in reality (in my opinion), balance the long-term rate of rock uplift. However, in very high mountains, such as for the 8000-m-high peaks of the Himalaya, the temperatures above 7500 m altitude almost never rise above freezing, such that freeze-thaw erosive cycles are suppressed. Because frigid air at high altitude holds little moisture, snowfall and ice accumulation are minimal. Erosion by glacial sliding or wind scour is negligible, because glacial ice is frozen to the bedrock, such that little to no glacial sliding or ensuing erosion occurs, whereas the thin air makes wind scour less effective. So, why don't peaks keep growing higher and higher? In this paper, Lavé et al. propose an appealing possibility (and, in my opinion, the most likely candidate) for sustaining this "rock uplift vs erosion" balance by a mechanism that has been rarely been proposed or documented: episodic, large-scale collapse of elevated, frozen summit pyramids. Documenting the episodic and abrupt collapse of unstable summit "towers" in Nepal's Annapurna Range is the focus of Lavé et al's submission. The authors argue that wholesale failure of unstable, perennially frozen summit "towers" is a key, but largely ignored, component of sustaining a long-term topographic balance between rock uplift and erosion. extending into the perennially frozen high elevation occurring due to the finite strength of the rocks that form the "vertically extruded" mountain edifice rising above ~8000 m.

In this revised submission, Lavé et al provide the first robust documentation (at least, that I know) of a massive collapse of a Himalayan summit that previously rose above 8000 m and, thereby, into this zone of perennially frozen conditions where effective physical and chemical erosion at local scales is largely absent. Instead Lavé et al deduce that a mega-landslide with a volume exceeding 20 km³ removed the former >8000-m-high summit pinnacle of Annapurna 4 about 1000 years ago. The combination of (1) the distinctive chaotic composition of the resulting rockfall deposits, (2) the atypical erosional landscape (numerous pentitents) flanking the high valley walls, (3) the downstream stratigraphy resulting from subsequent erosion of the landslide mass and resultant pulsed, rapid downstream aggradation, and (4) the quite high-resolution digital topography of the region together enable a remarkably well constrained reconstruction of the initial landslide emplacement, its aerial extent and lateral margins, its initial volume, the subsequently eroded volume, and the likely landslide source area. Each of these characteristics are remarkably well estimated. The authors are quite upfront about where the uncertainties lie, and as a consequence, they are (in my opinion) conservative in their estimates of volume, reconstructed source areas, and landslide dynamics. On the other hand, the (1) local stratigraphy, such as the chaotic and highly eroded landslide deposits, (2) the presence of some obvious candidates for large-scale sliding surfaces in the preserved bedrock, (3) localized downstream regions of very rapid sediment accumulation with debris eroded from the initial landslide deposits, (4) a suite of self-consistent radiometric dates, and (4) a convincing reconstruction both the sequence and effects of this mammoth, high-altitude rockfall and its subsequent erosion and re-distribution underpin a convincing reconstruction of the main characteristics and character of this unusual (or at least, rarely observed) landslide/summit-collapse event.

The rather ubiquitous presence of a perennially frozen zone in the Himalaya above 7500-8000 meters, the persistent convergence of the Indian subcontinent with the Himalaya with the sustained rock uplift that this convergence generates, and the fact that these very high peaks exist in a frozen, alpine desert where typical alpine erosion processes (glacial scour, freeze-thaw cycles) are largely ineffective seems to DICTATE that large-scale (catastrophic) collapse of high summits is a likely (and, it seems to me, the most viable) explanation for the long-term topographic evolution of the High Himalaya. I predict that this well-documented, but provocative paper will be well cited and will set the stage for innovative reconstructions and re-interpretations of collisional mountain belts through earth history. A key take-away from this paper is that on a global scale, where active tectonics are causing rapid uplift of summits into a perpetually frozen zone (be it in Antarctic, Alaska, Patagonia, or the Himalaya), the expectation should become that giant landslides are a likely outcome of the ensuing imbalance between gravity and rock strength. I have always likened it to vertically extruding a structurally weakened obelisk that will eventually collapse of its own weight. This submission is the first paper to document this phenomenon with a wealth of supporting data, as well as a catalogue of consequences.

Reviewer's note: Because this paper is written by a non-native English speaker, I have spent some time editing the text (using "track changes") to conform with more standard (and slightly more parsimonious) English usage. Hopefully, these suggested changes will be of use to J Lavé et al in subsequent revision prior to publication in Nature. Because your online system doesn't seem to accept such documents, I will send those separately to Dr Lave.

Referee #2 (Remarks to the Author):

Thank you for the careful replies and revisions in response to my comments and suggestions. I have no further concerns that should hold up the publication of this study, but offer two further comments at this stage.

The manuscript has bloated somewhat, and with little editorial effort some reduction of the word count could be achieved.

I am not entirely convinced by the arguments of the authors for a higher centre than edges of the deposits in the Sabche cirque. Many examples of landslide run up exist - the Val Pola landslide (Crosta et al., 2001, 2003) had 300 m run up with deposits well above the centre of fill. The acentric location of later glaciation of the cirque is the best argument for a raised centre of fill.

In addition, I very much like the attention to the likely benefits of the downstream sedimentation, suggested by the third referee.

I look forward to seeing this study in print in Nature!

Niels Hovius

Referee #3 (Remarks to the Author):

Lavé et al. present a revised and improved manuscript of their investigation of a large rockslide and associated deposits in the Annapurna range in Nepal. The general aspects of the study have not changed since the first submission, so I refer the editor to my summary from the previous

round of review.

As noted above, this revision is an improvement on the first submission, which I thought was quite good. I think that the writing is clearer, the technical details are better explained, and the presentation is improved. I enjoyed the restructuring of the manuscript, which makes for a better and more exciting read, the additional analyses (i.e., slope stability analysis in fluvially-dominated regions to the south of the study area), and the addition of the conceptual model figure.

The authors have done an excellent job addressing each of the reviewers' comments. I think that this has strengthened the work and makes an even more compelling case for the demise of high mountain peaks in the Himalaya that survive the "glacial buzzsaw". This exciting and well-supported forensic investigation is tightly bound in a compelling narrative and thus well suited for this high-profile journal. The evolutionary model presented in the text and the new figure 5 is exciting and has important implications for the growth of mountains in the Himalaya and possibly elsewhere and provides insight into catastrophic natural hazards not observed in modern times. Furthermore, the hypothesis that mountain peaks that survive the glacial buzzsaw might experience strengthening via permafrost development is clever. From a more speculative standpoint, this is an exciting idea beyond what is discussed in the manuscript because it might suggest some link between mountain height and climate that I am sure will stimulate new research. Collectively, I think this study will be of broad interest to geologists and the general public, given the scientific advances and the general intrigue regarding the life cycle of the highest mountains on the planet.

The authors have addressed all of my primary concerns with this revision. Below I have a few line-by-line comments to help clarify a few things, but beyond these minor points, I think that this study is basically ready for publication. I am really looking forward to seeing this come out!

All the best,

- Sean F. Gallen

Line-by-line comments:

L37-38: Suggested revision "This suggests that erosion is muted and much less than regional rock uplift rates for the highest Himalayan peaks raising questions about their long-term evolution". Another shorter suggestion is to add "largely" between "are" and "free" in this sentence.

L40: "in altitude" rather than "of altitude"?

L57: "surrection" is a fairly fancy word and I don't think is used much. I actually had to look it up. Can more plain language be used here? "Development" or "growth" might work?

L70: There seems to be a word missing between elevation and in. maybe "occurs"?

L165-166: Can the internal angle of friction assumed be mentioned here? i.e., "...rock mass cohesion of 1-2 MPa, assuming a friction angle of 35 degrees."

L166: I think "Afterwards" can be deleted.

L169: Hillslope rather than hillslopes

L183: "was" rather than "have been"

L189: "Farther" rather than "Further"

L226: "a double face" is a little awkward in English. I think it can be more simply stated that there are positive and negative aspects to collapse.

L229: I think this might work better with this minor revision: "...second largest city with all of its infrastructure."

L288-292: For some reason, I found this sentence difficult, but I can't think of a good revision. Can the authors please try to revise this sentence for clarity?

L 294: is "continental" needed here?

L314: "farther" rather than "further"

Figure 2: I still don't understand how to read the second panel from the top in a showing the IRSL pdfs. I don't understand how they relate to figure ED6. It seems like the probabilities should go down to the left in this plot as they do in ED6, but they go up. This is why I was confused the first time I looked at this plot, and I apologize for not explaining this before. I also think "age" rather than "ages" works better for the y-axis label.

Figure 5: can a rock uplift arrow be added to panel A?

L586-589: I think these sentences can be added to the paragraph above without the paragraph breaks per sentence.

L594-595: No need for a paragraph break here.

L610: "amounts" rather than "amount"

L610-611: "Luminescence signals" or "The luminescence signal"

L614-615: There seems to be a word or something missing for the sentence starting with "Collected samples"

L631-632: no need for a paragraph break here.

L641: paragraph break can be added between these sentences.

L659-660: no need for a paragraph break.

L705: "the restoration" rather than "restoration"

L710-711: no need for a paragraph break.

L721-722: no need for a paragraph break.

L754: farther rather than further.

Referees' comments:

Referee #1 (Remarks to the Author):

Review of Lave et al., "Demise of a Himalayan giant and the dramatic fate of the highest peaks on Earth"

This submission provides one of the first (or perhaps the first) rather thorough, quantitative, and convincing analyses of what processes set limits on the heights of Earth's highest mountains. For decades, alpine geologists and geophysicists have recognized that a competition between rock uplift and erosion must modulate the height of mountains in active orogens, such as the Himalaya. But, a largely unresolved conundrum has existed, because, whereas tectonically driven rock uplift persists irrespective of ambient climate, the character of erosion changes as a function of that same climate. At lower altitudes on these high mountains, glacial scour, rivers, landslides, and rainfall erode the bedrock and can, in theory and likely in reality (in my opinion), balance the long-term rate of rock uplift. However, in very high mountains, such as for the 8000-m-high peaks of the Himalaya, the temperatures above 7500 m altitude almost never rise above freezing, such that freeze-thaw erosive cycles are suppressed. Because frigid air at high altitude holds little moisture, snowfall and ice accumulation are minimal. Erosion by glacial sliding or wind scour is negligible, because glacial ice is frozen to the bedrock, such that little to no glacial sliding or ensuing erosion occurs, whereas the thin air makes wind scour less effective. So, why don't peaks keep growing higher and higher? In this paper, Lavé et al. propose an appealing possibility (and, in my opinion, the most likely candidate) for sustaining this "rock uplift vs erosion" balance by a mechanism that has been rarely been proposed or documented: episodic, large-scale collapse of elevated, frozen summit pyramids. Documenting the episodic and abrupt collapse of unstable summit "towers" in Nepal's Annapurna Range is the focus of Lavé et al's submission. The authors argue that wholesale failure of unstable, perennially frozen summit "towers" is a key, but largely ignored, component of sustaining a long-term topographic balance between rock uplift and erosion. extending into the perennially frozen high elevation occurring due to the finite strength of the rocks that form the "vertically extruded" mountain edifice rising above ~8000 m.

In this revised submission, Lavé et al provide the first robust documentation (at least, that I know) of a massive collapse of a Himalayan summit that previously rose above 8000 m and, thereby, into this zone of perennially frozen conditions where effective physical and chemical erosion at local scales is largely absent. Instead Lavé et al deduce that a mega-landslide with a volume exceeding 20 km³ removed the former >8000-m-high summit pinnacle of Annapurna 4 about 1000 years ago. The combination of (1) the distinctive chaotic composition of the resulting rockfall deposits, (2) the atypical erosional landscape (numerous pentitents) flanking the high valley walls, (3) the downstream stratigraphy resulting from subsequent erosion of the landslide mass and resultant pulsed, rapid downstream aggradation, and (4) the quite high-resolution digital topography of the region together enable a remarkably well constrained reconstruction of the initial landslide emplacement, its aerial extent and lateral margins, its initial volume, the subsequently eroded volume, and the likely landslide source area. Each of these characteristics are remarkably well estimated. The authors are quite upfront about where the uncertainties lie, and as a consequence, they are (in my opinion) conservative in their estimates of volume, reconstructed source areas, and landslide dynamics. On the other hand, the (1) local stratigraphy, such as the chaotic and highly eroded landslide deposits, (2) the presence of some obvious candidates for large-scale sliding surfaces in the preserved bedrock, (3) localized downstream regions of very rapid sediment accumulation with debris eroded from the initial landslide deposits, (4) a suite of self-consistent radiometric dates, and (4) a convincing reconstruction both the sequence and effects of this mammoth, high-altitude rockfall and its subsequent erosion and re-distribution underpin a convincing reconstruction of the main characteristics and character of this unusual (or at least, rarely observed) landslide/summit-collapse

event.

The rather ubiquitous presence of a perennially frozen zone in the Himalaya above 7500-8000 meters, the persistent convergence of the Indian subcontinent with the Himalaya with the sustained rock uplift that this convergence generates, and the fact that these very high peaks exist in a frozen, alpine desert where typical alpine erosion processes (glacial scour, freeze-thaw cycles) are largely ineffective seems to DICTATE that large-scale (catastrophic) collapse of high summits is a likely (and, it seems to me, the most viable) explanation for the long-term topographic evolution of the High Himalaya. I predict that this well-documented, but provocative paper will be well cited and will set the stage for innovative reconstructions and re-interpretations of collisional mountain belts through earth history. A key take-away from this paper is that on a global scale, where active tectonics are causing rapid uplift of summits into a perpetually frozen zone (be it in Antarctic, Alaska, Patagonia, or the Himalaya), the expectation should become that giant landslides are a likely outcome of the ensuing imbalance between gravity and rock strength. I have always likened it to vertically extruding a structurally weakened obelisk that will eventually collapse of its own weight. This submission is the first paper to document this phenomenon with a wealth of supporting data, as well as a catalogue of consequences.

Reviewer's note: Because this paper is written by a non-native English speaker, I have spent some time editing the text (using "track changes") to conform with more standard (and slightly more parsimonious) English usage. Hopefully, these suggested changes will be of use to J Lavé et al in subsequent revision prior to publication in Nature.

Because your online system doesn't seem to accept such documents, I will send those separately to Dr Lave.

We would like to thank the reviewer for his enthusiastic assessment and for his suggestions

Referee #2 (Remarks to the Author):

Thank you for the careful replies and revisions in response to my comments and suggestions. I have no further concerns that should hold up the publication of this study, but offer two further comments at this stage.

The manuscript has bloated somewhat, and with little editorial effort some reduction of the word count could be achieved.

I am not entirely convinced by the arguments of the authors for a higher centre than edges of the deposits in the Sabche cirque. Many examples of landslide run up exist - the Val Pola landslide (Crosta et al., 2001, 2003) had 300 m run up with deposits well above the centre of fill. The acentric location of later glaciation of the cirque is the best argument for a raised centre of fill.

We recognise that the problem of the initial surface remains partly open as we lack examples in the literature of large landslides collapsing in an almost closed cirque. Nevertheless, we still believe that the case of Val Pola mentioned by the reviewer does not contradict our argument given in Supp. Info 4. According to the descriptions given in the literature on this landslide, the landslide mass did indeed run several hundred metres up the opposite side of the valley, before some of it moved backwards before the mass finally stabilised. The final geometry of the deposit roughly fits into typology "C" of figure SI-4-a. For this type of deposit, we observe an inverted gradient with respect to the initial slope of the slide, and an altitude of the distal part higher than that of the proximal zone of

the deposit. If Sabche deposit corresponds to this type of rock avalanche, the distal point must be higher than the 4600m altitude of the proximal deposits (to the NE of the cirque) as seen in the cross-section in Fig. SI-4-c. Since the distal area, i.e. the ridge to the south of the cirque, is at about this altitude, then one would have to imagine that a large mass of debris jumped over this ridge. This is one possibility, which could imply that our estimates of the volume of deposits in the upper Seti valley are underestimated. But the important point here is that in this scenario, the centre of the cirque, which lies between the proximal zone to the NE and the ridge to the south of the cirque, will be above 4600m or close to it. In fact, whatever the scenario, A, B or C, the central zone must be at a higher altitude than either the distal zone (scenario A) or the proximal zone (scenario C). Because in our case, the distal and proximal zones are at the same altitude of 4600m, then the central zone must be at an altitude of at least 4600m.

In addition, I very much like the attention to the likely benefits of the downstream sedimentation, suggested by the third referee.

I look forward to seeing this study in print in Nature!

Niels Hovius

Referee #3 (Remarks to the Author):

Lavé et al. present a revised and improved manuscript of their investigation of a large rockslide and associated deposits in the Annapurna range in Nepal. The general aspects of the study have not changed since the first submission, so I refer the editor to my summary from the previous round of review.

As noted above, this revision is an improvement on the first submission, which I thought was quite good. I think that the writing is clearer, the technical details are better explained, and the presentation is improved. I enjoyed the restructuring of the manuscript, which makes for a better and more exciting read, the additional analyses (i.e., slope stability analysis in fluvially-dominated regions to the south of the study area), and the addition of the conceptual model figure.

The authors have done an excellent job addressing each of the reviewers' comments. I think that this has strengthened the work and makes an even more compelling case for the demise of high mountain peaks in the Himalaya that survive the "glacial buzzsaw". This exciting and well-supported forensic investigation is tightly bound in a compelling narrative and thus well suited for this high-profile journal. The evolutionary model presented in the text and the new figure 5 is exciting and has important implications for the growth of mountains in the Himalaya and possibly elsewhere and provides insight into catastrophic natural hazards not observed in modern times. Furthermore, the hypothesis that mountain peaks that survive the glacial buzzsaw might experience strengthening via permafrost development is clever. From a more speculative standpoint, this is an exciting idea beyond what is discussed in the manuscript because it might suggest some link between mountain height and climate that I am sure will stimulate new research. Collectively, I think this study will be of broad interest to geologists and the general public, given the scientific advances and the general intrigue regarding the life cycle of the highest mountains on the planet.

The authors have addressed all of my primary concerns with this revision. Below I have a few line-by-line comments to help clarify a few things, but beyond these minor points, I think that this study is

basically ready for publication. I am really looking forward to seeing this come out!

All the best,

- Sean F. Gallen

We have followed all of the reviewers' suggestions, with the exception of the one relating to Figure 5. Therefore, we respond below only to this specific point, and to the issue of the presentation of the IRSL in Figure 2.

Line-by-line comments:

L37-38: Suggested revision "This suggests that erosion is muted and much less than regional rock uplift rates for the highest Himalayan peaks raising questions about their long-term evolution". Another shorter suggestion is to add "largely" between "are" and "free" in this sentence.

L40: "in altitude" rather than "of altitude"?

L57: "surrection" is a fairly fancy word and I don't think is used much. I actually had to look it up. Can more plain language be used here? "Development" or "growth" might work?

L70: There seems to be a word missing between elevation and in. maybe "occurs"?

L165-166: Can the internal angle of friction assumed be mentioned here? i.e., "...rock mass cohesion of 1-2 MPa, assuming a friction angle of 35 degrees."

L166: I think "Afterwards" can be deleted.

L169: Hillslope rather than hillslopes

L183: "was" rather than "have been"

L189: "Farther" rather than "Further"

L226: "a double face" is a little awkward in English. I think it can be more simply stated that there are positive and negative aspects to collapse.

L229: I think this might work better with this minor revision: "...second largest city with all of its infrastructure."

L288-292: For some reason, I found this sentence difficult, but I can't think of a good revision. Can the authors please try to revise this sentence for clarity?

L 294: is "continental" needed here?

L314: "farther" rather than "further"

Figure 2: I still don't understand how to read the second panel from the top in a showing the IRSL pdfs. I don't understand how they relate to figure ED6. It seems like the probabilities should go down to the left in this plot as they do in ED6, but they go up. This is why I was confused the first time I

looked at this plot, and I apologize for not explaining this before. I also think “age” rather than “ages” works better for the y-axis label.

We believe that the reviewer's difficulties stem from inadequate vocabulary on our part. We had named the x-axis in Figure 2 "age AD (yr)", when in fact it is a date (the present is counted to the right with increasing dates). We have therefore renamed the axis with "Date (year AD)". This choice to represent a date and not an age from the present and counted backwards was guided by the fact that a date from the Gregorian calendar seemed easier to understand by a large audience than a BP age (i.e. counted since 1950).

In order to improve the reading of the correspondence with the Extended Data 4f figure, we have also included a second label for the x-axis indicated in date (AD and BC), as well as a rectangle zooming in on the period represented in fig.2. For reasons of convention for graphs representing IRSL ages, the increasing IRSL ages point to the right, so that the increasing date point to the left in this graph (i.e. opposite to fig.2).

Figure 5: can a rock uplift arrow be added to panel A?

We prefer not to include an uplift arrow because the initial situation may correspond to, for example, a landscape characterised by low relief before it is subjected to a tectonic uplift phase. In fact the initial situation is not of major importance, but it seems simpler to start from a landscape with little or no uplift rate.

L586-589: I think these sentences can be added to the paragraph above without the paragraph breaks per sentence.

L594-595: No need for a paragraph break here.

L610: “amounts” rather than “amount”

L610-611: “Luminescence signals” or “The luminescence signal”

L614-615: There seems to be a word or something missing for the sentence starting with “Collected samples”

L631-632: no need for a paragraph break here.

L641: paragraph break can be added between these sentences.

L659-660: no need for a paragraph break.

L705: “the restoration” rather than “restoration”

L710-711: no need for a paragraph break.

L721-722: no need for a paragraph break.

L754: farther rather than further.