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Interactive comment on "Millennial-scale denudation rates in the Himalaya of Far Western Nepal" *by* Lujendra Ojha et al.

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REFEREE: This article by Ojha and co-authors presents a new dataset of basin-wide denudation rates derived from 10Be measurements in rivers sands. The dataset consists in 7 new 10Be concentration measurements for the Far Western Nepal region in the Himalayas. While numerous studies have reported CRN derived denudation rates in various parts of the Himalayan arc, this is one of the first dataset from this particular area. The data and the methods are well presented and the authors discuss thoroughly the various hypothesis and caveats when calculating the denudation rates from the 10Be concentration measurements. They compare their results with available data in other areas along the Himalayan arc, and then discuss the relative contributions of various types of forcings to denudation rates. Seven samples is quite a small dataset

when compared with other similar studies in this area. However, this is a very important and understudied part of the arc (as noted by the authors this area is difficult to access). Indeed, the region illustrates the existence of important along-strike variations, in particular with respect to the intensively documented central Nepal sections. Any new data is thus a very welcome addition to the body of knowledge of the dynamics of the Himalayas, and has the potential to provide critical constraints on future discussion of the lateral variability along the arc.

My main concern is that, in its present form, the article lacks the formulation of a clear problem statement. The abstract and in particular the introduction read like the primary focus of the article is just to present a new dataset (see for example line 8), which is not a very attractive prospect for potential readers. For example, recent studies highlight a number of peculiar features of Far West Nepal (Harvey et al., 2015, van der Beek et al., 2016) and it might provide a starting point to present the results in terms of the analysis of lateral variations along the arc, in particular with respect to the much better constrained central Nepal area. Concerning the discussion of the implications on the effective controls on denudation, I feel that figure 4 only provides a very blurred and generalized perspective on the problem, and is not a very robust support for this discussion at the scale considered here. Comparing cross sections for denudation data, topography, geology and precipitation (etc . . .) in Far West and Central Nepal, could be very helpful for that purpose. See for example figure 2 of van der Beek et al. (2016).

Specific comments keyed to line numbers

p1-l38 : at this stage of the introduction you should highlight clearly why this region is important and interesting.

p2-l1-8 : the few studies that have looked in details at this area (van der Beek et al., 2016 and Harvey et al., 2015) have articulated a clear problem statement, and you could build on that to reformulate your introduction.

RESPONSE: Thank you for these constructive suggestions. To address this, we added the following text at the end of the Introduction, and we added a new figure (now Figure 2) that shows profiles of topography and specific stream power perpendicular to the range across Far Western Nepal.

"Previous studies suggest that the relative strengths of the controls on denudation rate in Far Western Nepal may differ from those in central Nepal. In central Nepal, the presence of a single, major mid-crustal ramp in the Main Himalayan Thrust (MHT) (e.g., Schulte-Pelkum et al., 2005; Bollinger et al., 2006; Nábělek et al., 2009; Elliott et al., 2016) has given rise to a steep topographic gradient with spatially focused exhumation and orographic precipitation (van der Beek et al., 2016). In Far Western Nepal, by contrast, the topography rises more gradually and induces a less intense focusing of orographic precipitation, and has been hypothesized to be a reflection of two distinct mid-crustal ramps, each smaller than the one in central Nepal (Harvey et al., 2015; van der Beek et al., 2016). This is consistent with apatite fission-track thermochronometric measurements that show that Myr-scale exhumation rates and specific stream power are significantly higher and more spatially focused in central Nepal than in Far Western Nepal (van der Beek et al., 2016). To the extent that along-strike variations in uplift and orographic precipitation influence the spatial patterns and magnitudes of denudation rates, they may also induce along-strike variations in the feedbacks between climate, tectonics, and topography. In this study, we report new basin-averaged denudation rate measurements inferred from cosmogenic 10Be in stream sediment in Far Western Nepal to better understand denudation rate patterns in this segment of the Himalaya. Our measurements show that denudation rates in these basins are consistent with those both east and west of Far Western Nepal, suggesting similar controls on denudation across this portion of the Himalayan arc over millennial timescales, and they highlight the regions that may be most useful to target for future denudation rate measurements."

REFEREE: p2- l30-33 : the presentation of the ksn belongs to the Methods section.

RESPONSE: We respectfully disagree. Section 2's description of the study area includes reference to the steepness index, which warrants definition of ksn here.

REFEREE: section 2 Provide some information about the implication of these lithological variations between the different units in terms of relative quartz abundance.

RESPONSE: As the manuscript discusses this issue in detail in Section 5.4, we do not add further discussion of it here.

REFEREE: p3 - l24 what is the upper limit for the grain size analyzed?

RESPONSE: (Here we repeat our response to a similar comment by Reviewer 1.) As noted in Table S2, most of the samples were dominated by sand-sized sediment. We did not measure the size of the largest grains, but we estimate that the largest grains at Raduwa (which has the largest median grain size among our samples) were no larger than 40-50 mm in diameter. We analyzed all grain sizes in the proportion they were present in our samples for precisely the reason the reviewer notes, i.e., that 10Be concentrations are often inversely related to grain size (e.g., Brown et al., 1995, EPSL, p. 193-202). Analyzing only a single grain size would yield a biased estimate of the basin-averaged 10Be concentration and hence a biased estimate of denudation rate (e.g., Riebe et al., 2015, PNAS, p. 15,574-15,579). To avoid introducing this bias, we analyzed all grain sizes in our samples to obtain a representative estimate of the mean 10Be concentration. To clarify this, we added the following text at Line 44 in Section 3.1.

"We analyzed quartz in all sediment grain sizes to avoid introducing biases that would be associated with analyzing only a single grain size (e.g., Brown et al., 1995; Riebe et al., 2015)."

REFEREE: p3- l35 : this is discussed later, but you should clearly state the fact that you do not take into account the variations in quartz content as a potential limitation.

RESPONSE: To clarify this, we added the following text at Line 18 in Section 3.2.

"In the Discussion section, we describe how our denudation rate estimates may be affected by uncertainties in a variety of factors, including lithologic variations in quartz abundance, which are not well quantified across our study basins."

REFEREE: P5 l20-23 : sentence not clear, "why most applicable?"

RESPONSE: To clarify this, we revised the parenthetical statement in this sentence so that it reads ". . . (and hence most appropriate for the ïĄč methodology, which was developed for bedrock-dominated channels subject to the stream power law)".

REFEREE: P5 l40-45 : this is interesting and not frequent in this kind of studies, so it might be worth giving a bit more visibility to the corresponding results.

RESPONSE: Thank you for the positive feedback. As the text states, this is part of a separate study in prep that will discuss these results in more detail, so we have left this section as is.

REFEREE: section 5.1 : in the absence of indications on the grain size used here it is difficult to discuss the impact of eventual landsliding contribution on the measured concentration.

RESPONSE: (Here we repeat our response to a similar comment by Referee 3.) We agree that grain size distributions in fluvial sediment are only a coarse reflection of landslide-derived inputs, given the partial filtering of grain size accomplished by fluvial transport. Although we maintain that grain size distributions can partly reflect landslide inputs to fluvial sediment and therefore can provide a useful clue about recent landsliding (e.g., West et al., 2014, EPSL, p. 143-153), we agree that the grain size distributions in our samples are not a strong test of upstream landsliding. We have therefore removed mention of the grain size distributions from this sentence.

REFEREE: P7 l5-20 I would be surprised if the contribution of chemical weathering were significant in this tectonic context and at this scale. I think there are estimates of solutes fluxes in one of the Lupker et al. paper.

RESPONSE: (Here we repeat our response to a similar comment by Referee 3.) We agree that the effects of chemical erosion are likely to be small at these sites. We added the following text at Line 6 in Section 5.2 to address this.

"Similarly, modern fluvial sediment and solute fluxes elsewhere in the Himalaya suggest that the chemical weathering flux in the Ganges and Brahmaputra Rivers is $\sim$9 $\pm$ 2% of the suspended sediment flux (Galy and France-Lanord, 2001) and that chemical weathering fluxes in Himalayan basins may be small relative to those generated in the lowland floodplains (West et al., 2002; Lupker et al., 2012). To the extent that these measurements are applicable to our study basins, this suggests that the chemical erosion may have only a small effect on our denudation rate estimates."

REFEREE: P8 l25 "larger tan 250 microns" is not a very precise definition of the grain size and does not allow to make a robust claim on this point.

RESPONSE: We agree that the influence of grain size on 10Be concentrations can't be strongly constrained from a comparison between our measurements and those of Lupker et al. (2012). This is consistent with one of the intended points of this paragraph, which is that our data do not require that the observed difference in 10Be concentration be attributable to grain size differences, but rather that they are consistent with that possibility. To emphasize that it's possible that grain size differences may be responsible for some of the difference in 10Be concentrations, we revised the sentence at Line 18 in Section 5.5 as follows.

"In addition, it is possible that grain size differences between samples may have contributed to the difference in 10Be concentrations, as Lupker et al. (2012) analyzed quartz grains in the 125-250 micron size fraction, whereas we analyzed only grains larger than 250 microns."

REFEREE: section 6 - even if you have less data it might be interesting, in terms of comparison, to plot you denudation rates along a cross section perpendicular to the range (as well as topography), with similar sections for central and/or eastern Nepal.

section 7 - same as previous points, having cross sections with denudation, topography, precipitation (+ thermochron data, etc. . .), might help to put everything into context, and make the argument easier.

RESPONSE: To address these two related suggestions, we added a new figure (now Figure 2) that shows the locations of our samples within profiles of topography and specific stream power perpendicular to the range across a swath of the Himalayas in Far Western Nepal. Our goal in adding this figure is to provide further context for the study area.

REFEREE: Figure 3B&C : at this scale there might be some overlaps with the data points representing ksn or Spw on individual stream segments, it would probably be better to use a continuous representation (topotoolbox has some function for that purpose). Same comment for S2 and S4.

RESPONSE: We are not sure what the reviewer means by "continuous representation" or what the reviewer is suggesting as an alternative. Regardless, we feel that the figures display the relevant information clearly enough, so we have left them as they are.

REFEREE: Figure 4 : The trends and relationships discussed in the text should be plotted on the figure with their confidence intervals. Figure 2b of Scherler et al (2014) display less scatter than what you plot here, did you subset the data according to some criteria (glaciated, lithology, ...)? You could display the trends identified by Olen et al. in adjacent regions. A and B are actually not indicated on the figure.

RESPONSE: We intentionally refrained from including regression lines from this figure, since we are not trying to suggest that the data follow a particular functional form. Instead, our aim is merely to show that denudation rates are broadly positively correlated with both ksn and specific stream power, as described in the text. The gray dots in panel B include data from the compilation of Scherler et al. (2017) and the dataset of Adams et al. (2016), the latter of which had been missing from the figure caption. We

updated the last sentence of the figure caption so that it now reads as follows.

". . . (Scherler et al. (2017) and Adams et al. (2016) in panel A, and Olen et al. (2016) in panel B)."

We added the labels "A" and "B" to the panels, as suggested.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2019-7/esurf-2019-7-AC5-supplement.zip

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2019-7, 2019.