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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 manuscript adds new cosmogenic 10Be data to the growing number of studies mapping out modern denudation rates across the Himalayan arc and fills a data-gap in Western-Nepal where no such data was published so-far. Calculated denudation rates are at par with other central-Himalayan catchments and the authors discuss it in the context of tectonic and climatic driving processes.

This data-set is relatively modest in size (7 samples) and the study is not very ambitious in the way it is set up. However, it provides new and useful data that is worth publishing in my opinion. The authors provide a very detailed and careful discussion of the limitation of the cosmogenic nuclide-derived denudation approach, carefully evalu-

ating the different steps which is something I have appreciated. The writing is clear and the figures are informative. I, however, think that the rationale for the study could be improved so that it reads less like a simple data-report (which is not a bad thing per se, but in that case, an e-surf research article might not be the most appropriate format).

As it currently stands, the authors are focusing on the climate vs. tectonics debate to motivate their study. But I feel that with a limited number of additional samples and a relatively short analysis they do not contribute much to this discussion. This would require a more thorough re-analysis of all 10Be along the Himalayan arc than what is presented here. On the other hand, the authors could have focused in more details on the specificities of the Karnali catchment with older AFT ages and lower stream power values compared to other parts of the range (e.g. van der Beek et al., 2016 – Geology). How these characteristics may or may not be expressed in surface catchment denudation rates seems a worthy discussion angle for this manuscript that is not necessarily very well addressed in this submission.

RESPONSE: We appreciate the reviewer's suggestions on how to strengthen the motivation for this study. To address this, we added the following text to the end of the Introduction, which provides more information for why we focus on Far Western Nepal in this study. To provide further context, we also added a new figure (now Figure 2) that shows where our samples are located within profiles of topography and stream power across this section of the Himalaya.

"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: Some more detailed considerations:

- It seems that the authors try to provide a global overview of 10Be data available across the Himalayan range (Figure 1). This is useful but is incomplete. At least 4 papers (maybe more) were omitted and should be mentioned: Puchol et al., 2014 – Geomorphology; West et al., 2015 – Esurf; Lupker et al., 2017 – Esurf; Dingle et al., 2018 – Esurf.

RESPONSE: Thank you for pointing these out these omissions. To address this, we added the locations of these studies to the map in Figure 1. (The sites in West et al. (2015) had in fact already been plotted in Figure 1, but the figure caption had incorrectly cited them as West et al. (2014), so this citation has been changed to West et al. (2015) in the caption.) We also added the relevant citations to the figure caption and the corresponding list of citations in the first sentence of Section 6.

REFEREE: - The comparison between this dataset and published data (e.g. Figure 4) should be made carefully. If I understand it correctly the denudation rate, as well as steepness or stream power, have not been recalculated in a homogeneous way for different datasets. Given that the authors report some large variations between different approaches (e.g. snow cover effect) it is uncertain how these differences may bias this type of comparison. I would suggest to recalculate the data using a homogeneous procedure (even though I am aware that this represents a significant amount of work) or convincingly show that the differences are minor.

RESPONSE: We agree that care needs to be taken in comparisons between denudation rate estimates computed in different studies, since there can be $\sim$10-40% differences in denudation rate estimates computed with different production rate parameters (e.g., Mudd et al., 2016). This is relevant in Figure 5, which aims to compare denudation rate estimates at our study sites in Table 2 (which were computed with CRONUS v2.3) to the denudation rate estimates we compare them to from the literature (Scherler et al. (2017), Adams et al. (2016), and Olen et al. (2016)), all of which were computed with CRONUS v2.2, which used different production rate parameters than those in CRONUS v2.3. As the reviewer notes, recalculating denudation rate estimates for these other studies would be a significant task, particularly because not all of these studies reported each basin's effective elevation or the degree of shielding by ice and seasonal snow, which would be needed to recalculate denudation rates under the same procedure we used. We therefore do not attempt to recalculate denudation rates from those studies here. Instead, for the purpose of making the visual comparison between the datasets in Figure 5 without this source of bias, we recalculated the denudation rate estimates at our study sites using CRONUS v2.2 (the same version that was used in the other studies), and we modified Figure 5 to show these denudation rate estimates alongside the denudation rate estimates in the other studies. The recalculated denudation rate estimates from CRONUS v2.2 are an average of 19% (range: 16-26%) higher than the values computed with CRONUS v2.3 in Table 2. Because these estimates follow the same broad patterns that were visible in these data before this recalculation, our central interpretations of Figure 5 are unchanged from those in the previous version of this manuscript. To make it clear that all denudation rate estimates in Figure 5 were calculated using the same version of the CRONUS calculator, we added the following text to the end of the figure caption.

"To avoid introducing biases to comparisons of denudation rate estimates determined from different versions of the CRONUS calculator, the black dots in Figure 5 show denudation rate estimates at our study sites that have been recalculated using the same version of the CRONUS calculator (v2.2) as that used in Adams et al. (2016), Olen et al. (2016), and Scherler et al. (2017). These rates are an average of 19% (range: 16-26%) higher than those calculated with CRONUS v2.3 in Table 2."

REFEREE: - The use of a topographic shielding correction in catchment-wide denudation rates has been recently questioned DiBiase, 2018 – Esurf

RESPONSE: (Here we repeat our response to a similar comment by Referee 1.) We agree that an accurate assessment of topographic shielding effect can be important, especially in exceptionally steep topography, as DiBiase (2018) showed. To the extent that the model geometry adopted by DiBiase (2018) applies to our study basins, where our estimates of topographic shielding are relatively small (0.6 to 2.5% among basins), this would increase our estimates of denudation rate by < 2.5%. To address this, we added the following text at Line 50 in section 3.2.2.

"Recently, DiBiase (2018) showed that this approach can overestimate the extent of topographic shielding, particularly in steeply dipping catchments, and argued that topographic shielding factors should be 1 in basins with horizontal surrounding ridges. If this horizontal ridge geometry is applicable to our study basins, where our estimates of topographic shielding range from 0.9759 to 0.9939 (Table 2), then the denudation rates in Table 2 would be underestimated by 0.6% to 2.5%."

REFEREE: - p.6, l.52: it is not clear to me how grain-size data on fluvial sediments will tell you much about the importance of landslide inputs given transport segregation

processes.

RESPONSE: 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 (e.g., West et al., 2014, EPSL, p. 143-153) and therefore can provide a useful clue about recent landsliding, we agree that the grain size distributions in our samples are not a strong test of the prevalence of upstream landsliding. We have therefore removed mention of the grain size distributions from this sentence.

REFEREE: - On the effect of chemical erosion on 10Be denudation estimate (p.7, section 5.2): the fact that chemical denudation is only a very small fraction of the overall mass export (as mentioned later in the manuscript) should provide a rough estimate on the magnitude of this bias.

RESPONSE: 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 chemical erosion may have only a small effect on our denudation rate estimates."

REFEREE: - p.8, l.26: Puchol et al., 2015 – Geomorphology provides a direct example of 10Be concentrations correlated with grain-size induced by landslide processes in a Himalayan catchment.

RESPONSE: We believe this is referring to Puchol et al. (2014), which refers to the grain-size dependence of 10Be in a Himalayan watershed, rather than Puchol et al. (2015), which we were not able to find a reference to. We added a citation to Puchol et al. (2014) to the list of citations at Line 22 in Section 5.5.

REFEREE: - p.9, l. 1-2 the difference between short-term denudation estimates and long-term rates in the Himalaya has been very recently discussed in the context of large landslide occurrences: Marc et al., 2019 – Esurf

RESPONSE: Thank you for drawing our attention to this recent study from central Nepal. We modified this sentence to include a citation to this study at Line 23 in Section 5.5, which now reads as follows.

"This difference of a factor of 1.5-2 is relatively small compared to the order-of-magnitude differences between short-term and long-term rates often observed in small catchments, particularly those subject to large, rare landslides (e.g., Kirchner et al., 2001; Hewawasam et al., 2003; Covault et al., 2013; Marc et al., 2019)."

REFEREE: I am looking forward to seeing this manuscript published in a revised form. Maarten Lupker

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