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Earth Surface Dynamics Discussions

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

Anonymous Referee #2

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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 understud-

ied 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.

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

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

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

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.

P5 l20-23 : sentence not clear, “why most applicable?”

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.

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.

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.

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.

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.

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.

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.

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