Earth Surface
Dynamics
Discussions

Interactive comment on "High natural erosion rates are the backdrop for enhanced anthropogenic soil erosion in the Middle Hills of Nepal" *by* A. J. West et al.

Anonymous Referee #2

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This manuscript by West et al. discusses soil degradation promoted by agricultural management with respect to the natural background erosion in the Middle Hills of the Nepal Himalayas. This manuscript provides 7 new cosmogenic 10Be measurements from 3 sampling sites, one from the Likhu Khola catchment and two from nested head water catchments. The results are discussed in comparison to previously published suspended sediment records and erosion plot observations from the same catchment as well as recently published 10Be analysis from the wider surroundings.

The manuscript tackles a very interesting subject with high societal significance. The anthropogenic impact on the erosional signal in an active mountain belt, is especially interesting since erosion studies in the Himalayas have often aimed to invoke tectonic

and/or precipitation feedback control on erosion and vice versa. On a short term human activity might have not only the potential to bring these feedbacks in disequilibrium but also might offset/adulterate the previous analysis that did not consider agriculture to be an important player. To solve this question West et al. have chosen a watershed with a priori information of historical suspended sediment fluxes and land use specific erosion plot observation as well as land use distribution. In particular interesting I find the authors effort to evaluate grain size specific 10Be concentrations of their samples.

Overall, I find the basic concept of the manuscript appealing but considering the little new data and the large amount of literature data I am hesitating to call this contribution a review article rather than a research article. Maybe the authors can revise their manuscript to better highlight the new findings of this manuscript.E.g. figure 5 to my knowledge seems to not provide any new information, it is very well known that degraded land (forest and scrubs) in the Middle Hills of Nepal and also in other mountain regions have a much more negative erosion balance than native or proper cultivated lands.

To enrich the findings and to make them more significant, it would be interesting to report on the total area affected by farming, not only in the Likhu Khola catchment but also in the wider area of the Middle Hills. In particular it would be very interesting to document how land use and its subdivisions have changed between 1992 (time of the suspended sediment and erosion plot analysis) and 2002 (time of the 10Be sampling respectively) and how much these changes could modify overall erosion budgets. Land use classifications from multispectral imagery are straightforward and standard tools are included in all common GIS engines. The authors should also check the work on land use and erosion in the Middle Hills carried out within the PARDYP project (http://pardyp.icimod.org/) managed by ICIMOD around the year 2000. The data is summarised in the PhD thesis of Juerg Merz http://lib.icimod.org/record/7484 and publications referenced therein. With respect to land use changes, it is necessary to discuss when farming actually has started in this area of Nepal, especially with respect

of the integration period of the 10Be analysis. A natural question would be, could this time span already impact the long term erosion evaluation by cosmogenic nuclides and how? E.g. I could imagine one scenario that terracing and urbanisation activities have lead to a quick loss of the upper soil horizon and thus, of the long exposed and 10Be enriched soils, leaving behind less well exposed and lower concentrated soils. These remaining low concentrated sediments could falsify the "natural" background erosion signal towards unnatural higher erosion rates, although it is clearly an anthropogenic signal.

On the methodological side, I am somewhat confused by the soil production determination. The values in the discussion section (p 952, l 1-8) arrive totally out of the blue. The method and assumptions to estimate soil production should have at least been presented in the method section. The authors discuss (p 951, l 27 onwards "... the difference between the long-term denudation rate (...) and the denudation rate driven by mass wasting should be the average rate of soil production ...") a vague assumption for soil production. First, I am not sure there is only two mechanisms of erosion in the middle Hills. What about weathering, sheet erosion, road construction, etc.? Secondly, it is not really clear to me where the mass wasting evaluation is coming from. I think to make such assumptions the authors need to make a much more thoughtful evaluation, including more observations, in order to base parts of their conclusions on these results. In particular I am bothered by assuming "... background natural fluxes from mass wasting ..." (p. 952, l. 2-3), since mass wasting, also in the Middle Hills, is a stochastic process making year to year comparison very difficult and thus need to be averaged over much longer time spans to derive a real background value. Lupker et al. (2012, EPSL), report for the whole Narayani Catchment (of which Likhu is part of) changes in 10Be erosion rates from one year to the other by a factor two and attribute this to localised and catastrophic sourcing by mass wasting. Although in previous publications (Hewawasam et al., 2003 and Vanacker et al., 2007), suspended sediment fluxes have been taken as true integral values of the total erosion flux. I think a discussion of comparability of sediment fluxes vs 10Be denudation rates is necessary in the

Himalayan context. Especially because the timing of sampling of 10Be (2002) and the suspended sediment measurements ($\sim<$ 1992) used in this manuscript do not overlap. The publications above use a much more complete dataset, including soil pit samples and sedimentation rates in retention basins (bedload, depth integrated concentration profile, etc.). Last, I found the back and forth between mm yr-1 and t km-2 yr-1 through the manuscript a little bit confusing. It would be better to single this down to one unity.

Further comments on the manuscript:

* For a non native speaker the title seems to contradict the conclusions on p. 954 l. 4-6 (and through the whole text).

* p. 937, l. 27-28: What about local references on soil plot erosion?

* p. 938, l. 5-18: If erosion intensity has changed over time, how does this fit the strong hypothesis to calculate erosion rates from cosmogenic nuclides in river sand samples, that "Erosion in the catchment is constant over the period over which the cosmogenic nuclides average denudation." (Dunai et al., 2010, page 121)?

* p. 940, section 3.1.: This section should be entitled "Short-term, and anthropogenic erosion rates". I generally struggle with the synonymous use of "short term" and "anthropogenic" in this manuscript. This paragraph needs references!

* p. 940, section 3.2.: Why was the Likhu Khola sample not grain size specific analyzed? The fact that there was only little coarse grained material in the Likhu Khola sample might introduce a sampling bias. If the headwaters source a rather wide distribution of granulometries this should be also found in the main trunk.

* p. 941, l. 10: Portenga and Bierman (2011) is not a good technical reference here. Granger et al. (2013, GSA Bull.) would be better.

* p.943, section 4.2: I find it a little pretentious to present the data from previous publications in the results section. I propose to have this in a separate section, e.g. entitled: Literature review.

* p. 943, l. 13: It would be important to include the area size contribution of each land use type for each catchment (maybe in table 3).

* p. 944, l. 6-10: What is the link between the MCT and anthropogenic erosion?

* p. 944, l. 19-21: The fact that erosion rates vary in the order of two must not necessarily be the reason of tectonic/relief distribution, but can also derive from stochastic sediment input, e.g. Puchol et al. (2014, Geomorph.) or Lupker et al. (2012, EPSL).

* p.944, line 23 onwards: Indeed there is no study so far comparing SSC to CN erosion in the Nepal but a comparison between different publications can be made. Just to give a few possible publication combinations:

* For the Khudi Khola: Gabet et al. (2008, EPSL), Gallo & Lave (2014, Geomorph.), Puchol et al. (2014, Geomorph.), Godard et al. (2012, JGR)

* For larger catchments: Lupker et al. (2012, EPSL), Andermann et al. (2012, EPSL)

* p.945, l. 13: What represents the +-11mm yr-1? It would be more representative to calculate the average erosion rate as area weighted mean. This would also prevent problems of a skewed distribution.

* p. 945, l. 18-19: "...and application of the same production scheme may be expected to bring results even closer together." this assumption is highly speculative, please proof.

* p. 945, l. 26 onwards: The explanation that the high variability of the erosion plot analysis might derive from the background erosion rate is highly speculative and contradictory, especially for terrace farming types.

* p. 946, l. 7: The Siwaliks should have much higher erosion rates than the Middle Hills since a significant portion of the tectonic shortening is accommodated here and rocks are very weak.

* p. 946, l. 10: Although it is difficult to work out the contribution of the Middle Hills to

the total Himalayan erosion budget, 10Be (Lupker et al. 2012) and SSC (Andermann et al. 2012) erosion rates do compare very well for large areas.

* p. 946, l. 13-15: Considering the very high spread of nearly two magnitudes of erosion rates, I doubt n=8 is a robust set of observations. Consider here also the use of an area weighted average.

* p. 946, l. 15-18: Chalise & Khanal (1997) do not report on how many years and how many measurements were included in their calculations. The erosion rates in Andermann et al. (2012) are calculated with a rating curve from long river discharge records.

* p. 947, l. 24: I find Kirchner et al. not a very good citation here. The integration times of both methods is comparatively very short (not even one interglacial) and can not integrate the "high magnitude, low-frequency" events. In particular the settings of Kirchner et al. are very different to the Himalayas. Furthermore, Andermann et al. (2012) demonstrates that most of the annual sediment flux is related to moderate events and not to peak floods.

* p. 5.3, section 5.3: The stochastic sourcing of sediments and its impact on the 10Be signal needs to be discussed here, e.g. Lupker et al. (2012) and Puchol et al. (2014). Godard et al. (2012, JGR) show very nicely how concentrations of 10Be change from the high Himalayas to the Middle Hills, this spatial aspect should be discussed too.

* p. 951, l. 8-10: This statement has been made several times already through the manuscript.

* Figure 1: It would improve the figure to include the channel network and the catchment outlines. The zoom in the left panel is too high. A larger subset would help the unfamiliar reader to orient himself better. E.g. a map stretching from the Terai to the Tibetan Plateau.

* Figure 2: What happened with the DEM in the left panel? Please use a adequate

DEM without artifacts. Stretch color range to the min-max elevation range of this sub-set. How much do the different land use types (left panel) contribute to the total catchment area? What about co-referencing the map to the digital elevation model?

* Figure 3: It would be interesting here to give a better overview on how sediment fluxes from different settings compare to agricultural dominate catchments. Therefore, more data could be plotted into this graph, e.g. Carretier et al. (2012, Geology), Andermann et al. and Lupker et al, Meyer et al. (2010, Int. J. Earth Sciences)...

* Table 2: Can you explain how the bi-directional uncertainties of denudation rates have been derived?

* Table 3: Please include the contribution of the different land use types to the total area.

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