

Interactive comment on “Pertinent spatio-temporal scale of observation to understand sediment yield control factors in the Andean Region: the case of the Santa River (Peru)” by S. B. Morera et al.

Anonymous Referee #1

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This paper by Morera et al. analyses the factors that control spatial and temporal variability in sediment yield in an Andean Region. The database that is used in this study is quite exceptional for an Andean basin, and it provides an excellent starting point basis for an interesting study. However, the analyses of the hydrometeorological and sedimentological data are not well developed; and the current descriptive analyses do not allow to make solid conclusions on the SSY spatial or temporal variability.

The structure of the manuscript can be improved. In the current version of the manuscript, the authors jump from one idea to another, without having a clear line of thought. This is certainly the case for the introduction, but also for section 2 on

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‘Study area and settings configuration’. The part on ‘slope degree’ has not much to do with a description of the study area, but is a (fuzzy and very general) overview of the link between erosion and slope gradients. Parts of the conclusions are not supported by the data and analyses. Finally, the authors conclude that the difference in SSY between two large basins is mainly linked to dispersed mining developed on a specific lithological formation. There are no data or analyses that support this idea.

Overall comments In the introduction, the authors state that ‘numerous studies address this fundamental question by looking generally at a specific scale and at some potential factors such as...’ They then give some examples of studies that have analyzed the spatial variability in sediment yield or specific sediment yield or even long-term denudation rates (cosmogenic nuclides). Most of the studies that are cited are very complete, and have analyzed various environmental factors (and not just one factor, as stated by the authors). Besides, some of these studies are based on ^{10}Be -derived denudation rates, and integrate over very long timescales. Hence, there is no interest to analyse human impact or land use in these studies. When you want to compare the results of these studies, this has to be done correctly by discussing their complexity and their impact.

The authors state that ‘the analyses of worldwide data present a large statistical dispersion, and cannot be used to design an easy-use universal or physical model’. This is a bit surprising, as relatively simple empirical models have been proposed by Syvitski et al. (2003), and Vanmaercke et al. (2011). These models have a good fit, and it has clearly been showed that it is possible to model specific sediment yields based on a limited number of environmental factors. See also Syvitski and Milliman (2007), Restrepo and Syvitski (2006), Kettner et al. (2010)

Recent studies have analyzed the relation between ENSO and sediment yield. So, there are data on the relation between ENSO, precipitation, runoff and sediment transport (in contrast to what is mentioned on p. 631, Line 1-10). They clearly show that negative ENSO events have strong effects on streamflow discharges and sediment

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fluxes. This new research (from Northern Peru) is not referred to in the introduction. Have a look at Tote et al. (2011), Romero et al (2007), Tarras-Wahlberg and Lane (2003).

The authors make use of stratigraphic maps to discuss the link between lithology and SSY. It would be far more useful to have the lithological maps (Figure 3, Table1). The description of the lithology (page 633, line 6-23) is not all clear. From an interpretation of the stratigraphic map, it seems that sedimentary rocks are important, but this does not appear in this paragraph. I do not see how you can conclude that the major geology includes 'pyrite, schists, phyllite, pyrite-bearing, and quartzite material intruded by a central granodiorite-tonalite batholith that is all overlain by clastic sediments deposited during the glacial retreat'.

As one of the aims of the study is to analyse the influence of ENSO on P, Q and SSY, I would have expected more information on interannual variability in precipitation in the section on the hydro-climatological context (Section 2.5). In this section, there is a lot of information on weather patterns, but nothing on ENSO. The same is true for the section 3.5 (page 639) on rainfall information. Two rainfall stations are used to derive rainfall patterns (?). Given the size of the Santa watershed, this is largely insufficient. Besides, both stations are located at relatively high altitude, and will probably underestimate the rainfall amounts during the ENSO periods (as most rainfall is concentrated in the coastal regions during ENSO events). . . The authors state that there is no clear effect of ENSO on discharge (Discussion 5.1, page 644-645). It remains uncertain why this is the case. A proper analysis is necessary to have final results, and this would include an analysis of correlation between ENSO and rainfall, rainfall and discharge, and finally ENSO and discharge. The authors currently analyse the effect of ENSO on discharge by comparing classified MEI data with discharge data. Why not using directly the SOI absolute values instead of classified values? This would allow you to do a quantitative analysis between P, Q and SOI values.

The study lacks a quantitative analyses of the factors that control Q, SY and SSY. In

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the section on Sediment Yield Control Factors (page 646-645), the authors criticize previous analyses of SY and SSY that would be biased by large uncertainty. This statement is not entirely correct, and should anyway be part of the introduction. There might, indeed, be a large uncertainty on the individual SSY estimates, but by compiling many individual data, the individual (random) errors are minimized. In this study, there is no quantitative (statistical) analysis of the controlling factors. There is no plot of correlation between Q, SY or SSY and the controlling factors. Hence it is difficult to follow the interpretation and the conclusions made by the authors as they are not supported by numerical analyses.

Detailed comments

p. 627, line2: What do you understand with 'hydro-sedimentology development'? Rephrase.

p. 630, Line 15: In the study of Molina (2007, 2008), the size of the catchments ranges from 1 to 20 km². This is far beyond the 'hillslope scale' what is stated here by the authors.

p. 631, Line 17: What do you understand with 'seasonal vegetation cover'? Rephrase

Page 631, Line 21: What you understand with 'mining of aggregate'?

Page 632: Line 1 : Rephrase title

Page 632, Line 3-5: Really sure about this? The Catamayo-Chira is often mentioned as one of the largest, with a drainage area of 17000km².

Page 633, Line 1-4: Not clear, Rephrase this section. It is not clear what you mean with 'plate areas', and I do not see any part 2 in this manuscript.

Page 634, Line 20-23: Why are oversteepened slopes a result of agriculture or construction? I do not see the causal relation.

Page 635, Line 12: You mention that the range was 0-1150mm before. . .

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Page 635, Line 24: Rephrase 'semi-arid mountain chains'

Page 638: The authors state that the theoretical number of samples is not reached, but they do not give any indication on the number of samples that do exist. Which % of samples is missing? Are there important gaps during peak events?

Terminology: The terms 'sediment yield' and 'specific sediment yield' are not always corrected used. Sediment yield normally refers to the total sediment yield (Mass/Year), while specific sediment yield is the sediment yield normalized by catchment area. See e.g. Figure 1

Table 1: This table is not really necessary. The stratigraphic maps can be replaced by lithological maps, which is far more interesting for this analysis.

Figure 1: The caption mentions that the numbers in the figure refer to the sediment delivered to the oceans. I would rather interpret these numbers as specific sediment yield values, that can be very different given deposition processes. Certainly for the catchments draining to the Amazon, there is a big difference between the two!

Figure 3: A lithological map (and not stratigraphic map as shown here) would be more useful.

Figure 8: Not clear at all if this is about the relation between SSC and Specific Discharge (as suggested by Q_{Specific} , $\text{m}^3/\text{km}^2/\text{s}$) as it is shown in the graph; or about the relation between SSC and SSY, as it is mentioned in the caption. . .

Figure 11: Nice figure, but would be far more interesting to have the lithologies here instead of the stratigraphic units.

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