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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climatic and geologic controls on suspended sediment flux in the Sutlej River Valley, western Himalaya" *by* H. Wulf et al.

Anonymous Referee #2

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Review of Wulf, Bookhagen, Scherler: Climatic and geologic controls on suspended sediment flux in the Sutlej River Valley, western Himalaya.

This manuscript presents some highly interesting data on suspended sediment concentrations across the Himalayan mountain range. The main findings are of high value to a wide community of scientists in Earth's surface processes and related fields. The authors present a dataset of suspended sediment concentration and discharge measurements from 8 stations crossing the orographic gradient from the humid southern front to the arid northern Plateau. The authors analyze the data in parallel with time series of remotely sensed precipitation and fractional snow cover measurements as well as with rock compressive strength measurements. The main findings are unique, especially the tracing of sediment pulses trough the mountain range is very intriguing.

Also the documentation of the 26 June 2005, Parechu Flood is highly valuable. Most figures and tables are of very good quality and support the argumentation of the authors, please see my comments below. However, overall, the manuscript's structure is a little loose and several arguments are vague and not supported by data and/or observations. The authors can do a much better job by concentrating only on the hard data they have and discussing processes which they observe. Finally, the manuscript lacks fundamental methodological explanations on the data processing. To recommend this manuscript for publication, a much more rigorous and transparent data handling description is necessary.

I have two strong concerns and some further questions and minor editorial comments.

Concerns/objections: (1) I am confused by the Schmidt hammer analysis. The authors present rock compressive strength analysis from Schmidt hammer rebound measurements without explaining what they are good for. It is mentioned that these measurements are taken as a proxy for erodibility only in the discussion section. How does rock compressive strength scales with erodibility? In particular, the bulk of the suspended sediments is not coming from abrasion of bedrock itself. Finally after a superficial discussion of the results which do not show the expected tendency the authors decide not to speak about it anymore. Schmidt hammer analysis are originally developed to evaluated the concrete quality of building structures (Schmidt, 1951), later some authors have used the method to evaluate rock strength under laboratory conditions (e.g. Katz et al. 2000, Kahraman, 2001). A detailed study by Steer et al. 2011, on a natural outcrop, reports that Schmidt hammer rebounds scales with rock stiffness and fracture density. This raises the question what you actually did measure? It is also not clear what the authors wants to tell the reader. If it is to say Schmidt hammer does not work, it should be covered with a proper introduction and argumentation in a separate publication. If it is to say lithologies does not control the sediment fluxes, the geological map does a better job. The compressive rock strength analysis presented here does not lead to any conclusion. I strongly recommend the authors to reconsider the presentation of this data.

(2) My second concern is about the rating model presented in the light of a hysteresis effect and the calculation of annual SSY and the SSC statistics. Figure 3 presents rating curves for three selected rivers, but what is the meaning of a rating curve if a hysteresis effect can be observed? You can not deviate one simple relation because it is certainly not the same on the raising and on the falling limb. I recommend to exclude figure 3, since it might be wrongly used by workers interested to predict sediment fluxes. The point that concentrations are overall higher for high discharge stages (page 550, line 25-26) can easily be demonstrated on the hysteresis plot (figure numbering unclear). This raises furthermore the question how were the annual SSY values and SSC statistics calculated? All your sediment records have missing data gaps (Figure 4), especially for the rivers Ganvi, Spiti, Namgia, Jangi, Karchham. Did you apply the sediment rating curve to fill the missing data gaps in order to make an annual budget analysis working? This is crucial to all your discussions and conclusions. It is absolutely not clear to me how the SSY values, e.g. in table 1, have been calculated. The rather intuitive equations on page 548 can can not shed any light on this. Furthermore, if the records are not complete, e.g. the Karchham data comprises only summer data, how did you calculate the SSC distribution to extract the 99% quantiles in order to define extreme events? The authors have blatantly avoided to explain their methodology. Furthermore, I recommend the authors to read the PhD thesis of Simon Dadson which is comprehensively reviewing rating models for seasonal concentration regimes. Fuller et al. 2003 is also of interest.

Further concerns and Questions:

Page 543, Line 24-26: Are the erosion rates presented by Lal et al. (2003) for the internally drained Tibetan Plateau really representative for the upper Sutlej valley? E.g. Vance et al. present much higher values (> 1 mm/yr) for the tibetan part of the upper Ganges valley.

Page 544, Line 21-23: "..., and rock-strength measurements to investigate the climatic and geologic controls on low frequency, high-magnitude sediment discharges." How can geology control the magnitude frequency distribution of SSC events?

Page 544, Line 25: Conclusions by Henck et al. (2005): "Our analysis demonstrates that mean monsoon discharge is Qeff in Yunnan and Tibet and suggests that monsoon discharge is more important than individual storms in governing suspended sediment transport for rivers in monsoon regions." What are you citing here?

Page 548, Line 18-20: I understand this argument. You are interested in a frequency signal and not the absolute rainfall values. Yet, TRMM-3B42 does a very poor job in picking up the orographic rainfall gradient. Did you somehow test if your trends are right? How does TRMM-3B42 rainfall distribution perform with respect to the Bookhagen and Burbank (2006) dataset? You say you have a dense network of weather station in your study area, so why not testing with this data, or using it?

Page 549, Line 3: ">=2 mm/day" are you sure about that?

Page 549, Line 12-24: Can you provide any more information on your interpolation techniques? In particular did you test your data compilation against the 8 day interval MOD-10 dataset?

Page 556, Chapt. 5.2: In your analysis before your are talking about SSY. It would be much easier to understand if the examples would be given in SSY and not in SSL.

Page 556, Line 14: If 14.2 to 8.8 % is a moderate decrease in fractional snow cover and does not show any effect, why should the decrease (page 557, Line 22-23) before the Parechu Flood (15.7-7.5%) make such a big difference? In other words the decrease in fractional snow cover is not so much different between the two events. It might help if you add the absolute area to the first example.

Page 556, Line 22 - Page 557, Line 5: This paragraph should be in the introduction section. You do not discuss any of your results in the light of this literature review.

Page558, Line 12-13: Yes, the flood is the most dominant event during your study period. But what does it imply to the overall erosion dynamics? It would be much more interesting to compare the erosional budget of the flood with the overall SSL values. How much does a flood event like this one contributes to the total sediment transport, does it really dominate?

Page 558, Line 23-25: Just to be clear, does higher discharge automatically leads to a shift in the proportionality between bedload and SSL? Please see also publications by Burtin et al. 2008 and 2011, and Attal et al. 2006.

Page 559, Line 23-28: This is one more example where the authors completely failed to introduce and explain the aims and methodology of their work. Neither, say the authors on page 544 that seismic data will be analyzed, nor do they explain in the methodology what data they are looking at. And also in the results section these analyses do not show up. There is only a 5 pages seismic record in the appendix section the authors refer to from time to time (page 551, Line 17 and page 556, Line 7). Then, in the discussion section, the authors make such an important observation “Therefore, we assume that low- to intermediate-magnitude earthquakes mobilize material, which is rarely evacuated by the rivers.”. Please see also Dadson et al. 2003, and Meunier et al. 2008.

Paragraph 5.5 needs to be introduced before, problematic .....

Figures: There is a mess in the figure captions. e.g. caption 8 should go along with figure 9 and so on.

Figure 1: Does No.3 joins the Sutlej River upstream or downstream of No. 7? Please include source of the glacial information.

Figure 7 A and B: This is not a probability density plot.

Fig 13 and associated Tables (4 and A2): It would be much easier if Table 4 and A2 are combined. You need to add an identification on the map to make it possible to relate

the table with the map. Most readers will not know where Dudh Khola for examples is. Also for a reader with onsite experience this is not an easy task.

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