

Interactive comment on “Erosion rates deduced from Seasonal mass balance along an active braided river in Tianshan” by Y. Liu et al.

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Before comments, I should point out that I take into account the original text of the paper and also replies done by authors to Marc Jolivet and Hendrik Wulf comments on this website. Note those comments and replies have proofread several of my initial question-marks on the original text. In the following I will discuss only points which are still not clear for me.

General comments : This paper investigates present day solid and dissolved sediment flux from Urumqi river in the high range of Tianshan. As authors write in the text, one of the key issues of this work is to assess rates of denudation to deep in the geodynamic of Tianshan range. Reliability of present day denudation rate for long term geodynamic modeling is an important question for this kind of study and understanding discrepancy

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with sedimentary basin records or measurement of in-situ cosmogenic nuclides is always an interesting discussion on methodology uncertainties (for both : present day and long term denudation rates measurement) and/or intrinsic time variability of the erosion rate. Such a discussion request dataset with well documented methodology and uncertainty calculation. This paper is a nice contribution to this debate for the specific high mountain range context.

It is an original complete analysis of mass flux (suspension, bedload and dissolve mass) at the outlet of a basin with a glacial dominated geomorphology. Because of accessibility and climate conditions, such in-situ mass balance with bed load measurement in asian mountain range are seldom in bibliography. At least, for these two reasons, this paper must be published. The observation that non negligible part of total dissolved matter are atmospheric input is original. Is it possible to specify this amount in the text?

I really appreciate how authors care to present honestly all results and uncertainties. Comparing complete mass balance at two sites hundred meters apart is rarely done to test the spatial relevance of the data. As for all datasets on mass balance flux in mountain river, uncertainties in results are large but show sufficient statistical consistency for the discussion on this work.

Results show mechanical denudation rate lower than other studies on mountain ranges contexts. Undoubtly the relatively lower mechanical denudation rate is induced by the low precipitation rate on Tianshan. But I wonder if Tianshan range, and specifically the upper part of the range with U-shape morphology, where this study has been done, present different statistical slope distribution and seismic activity comparing other places sited in the text (Haut Glacier d'Arolla, Siachen Glacier, Anapurna watershed, Kuitun river and Taiwan). In section 6.1, present day mechanical erosion rate comparison between the former areas would be more relevant with such additionnal informations about slopes and seismicity.

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Authors do not propose definitive interpretation to explain the relatively low mechanical denudation rate found with this dataset. This is a reasonable position and Tianshan geodynamic modeling is still an open question.

Specific comments : - Title should be changed. There is no reference to the "active braided" river properties in the text. - Page 547, line 3 : I prefer "runoff" with its general sense instead of "precipitation". Glaciers are built with precipitations. - Page 548, line 13 : A short explanation (one sentence) about reasons discussed by Liu et al. 2008 would be appreciated; - Page 550, line 20 : you can insert " (Fig 4)" after "within 20%". - Page 552, line 16 : precise which "levels" - Page 552, line 20 : usually summer has 3 months, precise that % indicated line 19 are for two months. - Page 558, lines 13- 15 : to demonstrate a non dilution effect, a variable dissolved mass production is the best argument. Could you verify this with your data ? - Page 560, lines 14-15 : I don't understand the significance of 2.9 for $V_{s,full}$ calculation. Could you develop this ? $V_{s,av}$ is definitely not a volume but a mass. I have the feeling that $V_{s,full}$ and $V_{s,av}$ have not the same dimension and cannot be compared. I am wrong ? - Fig 5, caption : "perfect agreement line" is a dark line not a dashed line. - Fig 8 : symbols for suspended and bed load should be changed. Currently they are too similar.

Interactive comment on Solid Earth Discuss., 3, 541, 2011.