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Interactive comment on "Climate, tectonics or morphology: what signals can we see in drainage basin sediment yields?" *by* T. J. Coulthard and M. J. Van de Wiel

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Some comments on the paper by Coulthard and Van de Wiel

1) The paper lacks referencing and discussion-comparison with some fundamental prior results and relevant papers. Among others, in disorder:

Allen and Densmore, 2000 study the response of small catchment fan systems to tectonic pulses (the minimum response times to tectonics are on the order of 100 ka in small (10 km) in these systems).

Goodbred and Kuehl, 2000; Clift et al., 2008: Evidence for abrupt millennialscale

changes in sediment fl ux, such as recorded in the shallow marine sediments near the mouth of major river systems.

van den Berg van Saparoea and Postma, 2008 => experimental study of river response to water discharge and sediment supply perturbations.

Simpson and Castelltort (Geology, 2012, attached): "We conclude that marine sedimentary basins may record sediment flux cycles resulting from discharge (and ultimately climate) variability, whereas they may be relatively insensitive to pure sediment flux perturbations (such as for example those induced by tectonics)."

Important papers have addressed before the response of fluvial systems to climate and tectonics: have a look at the fundamental works of Howard, 1982 and Paola et al., 1992 (defined the characteristic time required for a river to achieve equilibrium following a perturbation). Paola 1992 already clearly identifies a different response to tectonic and climatic forcings

Lisle et al., 1997: fate of sediment waves supplied kinematically to channels

Buffer effect of rivers: Métivier (1999), Métivier and Gaudemer (1999) among other Métivier's publications (pay attention to the orthography: Métivier and not Metevier), Castelltort and Van den Driessche (2003).

Non-linear response of a coupled catchment fan system and autocyclicity: Humphrey and Heller 1995 (tintinnabulations).

See also the series of Allen and Armitage et al. papers on the same topic.

2) Confusion with the term "shredding" and reference to the Jerolmack and Paola paper. To my understanding of their paper, Jerolmack and Paola proposed that upstream signals could be shredded through an alluvial system because of internal mechanisms themselves able to generate sed-supply signals of the same frequency and amplitude as the upstream signals. They attribute this behavior to a kind of morphodynamic turbulence: "Here we propose that sediment transport can act as a nonlinear filter

that completely destroys ("shreds") environmental signals. This results from ubiquitous thresholds in sediment transport systems; e.g., landsliding, bed load transport, and river avulsion. This "morphodynamic turbulence" is analogous to turbulence in fluid flows, where energy injected at one frequency is smeared across a range of scales. We show with a numerical model that external signals are shredded when their time and amplitude scales fall within the ranges of morphodynamic turbulence." Floodplain storage is a very different mechanism which tends to "buffer" upstream signals rather than "shred" them.

3) Lack a description of what is inherent to the model (numerics) versus what is a result of the studied natural configuration. How do we know whether these results come from the physics or from the numerics?

4) The studied timescales are very short (ka), such that a generalization to climate, tectonics and the sedimentary archives is not appropriate.

5) Lastly, a large amount of parameters are included in CAESAR and the authors should explain the importance of each, or simplify to those that are most relevant to the studied problem. In this line of thought, the comment in the discussion-conclusion "Compared to alternative landscape evolution models, CAESAR has a high level of process representation. Therefore, some of the phenomena we have simulated here (e.g. different grainsize responses to varying uplift) require such a high level of parameterization and could be missed by less complex, especially one dimensional network based models. This raises a question as to the level of process detail, spatial and temporal resolution that are required to simulate landscape dynamics" – is disputable. Indeed, CAESAR does not seem to implement the physics of water flow like in models such as DELFT for instance who treat the full dynamics of water flow. If climate matters, through rainfall and stream flow erosion, perhaps water flow should be one of the things to include in a discussion of how it actually does so?

Please also note the supplement to this comment:

http://www.earth-surf-dynam-discuss.net/1/C1/2013/esurfd-1-C1-2013-supplement.pdf

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