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Interactive comment on "Erosional response of an actively uplifting mountain belt to cyclic rainfall variations" *by* J. Braun et al.

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General comments

This manuscript by Braun and co-authors reports on the lag and gain of the erosional response to cyclic climate forcing in a stream power law landscape. This is found through a thorough analytical derivation of the stream power law's response to sinusoidal variations of rainfall that are supported by numerical simulations using Fastscape. It also is confronted to natural data collected in the Bengal fan where systematic lags are observed between the d18O and Nd at Milankovitch periods (Gourlan et al., 2014), and presented as a possible explanation for it.

The manuscript is important because it convincingly questions the fidelity of the strati-

graphic (sediment flux to basins) and geomorphic (their "topographic waves") archives in constituting a record of past climate changes. In addition, it proposes that this is inherent to bedrock erosion (if dominated by the stream power law, as carefully underlined along the text), making it potentially a general prediction for sediment flux out of active mountain belts under changing climates. Finally, it is useful in providing a tool to the community to address such problems, by searching through the parameter space to explore whether the observation can be explained by the model under consideration. As such, the paper is clearly relevant for ESurf.

Below I list several specific comments and questions:

p.973, L.2: than instead of that

p.974, L.6: Wilgoose (1994, 2005) => See also Paola, C., Heller, P. L., & Angevine, C. L. (1992). The large-scale dynamics of grain-size variation in alluvial basins, 1: Theory. Basin Research, 4, 73–90.

p.975, L.7: "daily" perturbations were used only for experiments of "laboratory scale", but the behavior is non-dimensional.

Their last paragraph, first page:

"When the forcing period is far smaller (four orders of magnitude) than the response time, only a small aggrada- tional wedge is accumulated in the time during which the water flux decreases, releasing a relatively small pulse when the wedge is eventually flushed downstream. Conversely, when the forcing period is larger than the response time, the entire river nearly attains its equilibrium slope in the time between cycles, enabling sediment to pass downstream in a more continuous, less pulsed manner. Importantly, even when the driving period is two orders of magnitude less than the river response time, the magnitude of sediment outflux pulses exceeds the input amplitude by a factor of almost ten."

p.975, L.14: "strongly damped": Is this dependent on the choice of diffusivity?

p.977, L.9: "small": here and in the appendix, the term "small" is used several times. It appears in the appendix that the "smallness" of the perturbation is used to simplify the equations.

What is "small"? If you rescale the equations back to natural values, how small are the variation of precipitation in percents of the baseline precipitation rate that you consider?

It would be interesting to know how this assumption of "small" perturbations influence your results. And what are typical precipitation variations in the Cenozoic?

p.977, L.22: Is this really the "erosional response time of the system"? I guess yes if erosion = uplift, but that is assuming that the system is "always" at steady state, even during perturbations. In Whipple and Tucker (1999) and Whipple (2001 - Fluvial landscapes response time...), the response time is defined differently to take into account the transient adjustment to new conditions. Here it would perhaps be more adequate to state that H/U is a "characteristic" time. This is a detail.

p.979, L.14: “small”: Is it the case for Milankovitch type precipitation variations?

p.979, L.15: “200km”: Still, it would be good to see some experiments with "Taiwan" type catchments (25 km long) such as in Whipple (2001).

p.979, L.16-17: “the response of an incising river to a perturbation in precipitation rate does not depend on its size or the size of its catchment.”: Does that essentially comes from assuming that the ratio mp/n is close to unity?

If yes, you should perhaps state it here because as you state above: "the slope and area exponents ... are not well constrained" and in the appendix: "Here we need to make a further assumption, which is unlikely to be valid in all situations"

p.980, L.12: m and n choice: would it change much if you remained in the range of the assumption of the paper, i.e. mp/n is close to unity? Another way to frame the question is: is 1.25 close to 1?

p.981, L.26: here you omitted “small”.

p.983, L.6: can you explain how the numerical experiments constitute a test of the validity of Hack’s law as a good descriptor of catchment topology?

p.997, Figure 1: This figure is reminiscent of figure 1 in Howard, A. D. (1982). Equilibrium and time scales in geomorphology: Application to sandbed alluvial streams. Earth Surface Processes and Landforms, 7(4), 303–325. However, Howard’s figure and text is not specific to erosion but rather presented as an illustration of the general response of linear systems with finite memory to cyclical variations (first 2 paragraphs of page 306). Could it be, thus, that the case of the stream power law be a sub-case of a more universal behavior?

p.982: “Although m is likely to be smaller than unity, it is possible that, if m = 1, the sedimentary signal be enhanced, which may explain the strong imprint that Milankovitch cycles have on the sedimentary record (De Boer and Smith, 1994) despite the relatively small changes in both solar insulation and temperature that are associated with the corresponding variations in the Earth’s orbital parameters. At long forcing periods (compared to τ), the gain tends towards zero, inhibiting detection of the time lag”. This is an important statement that should be taken as a take home message of the paper, in broad agreement with works by Howard, 1982, Godard et al. (2013) and Simpson and Castelltort (2012) among others.

p.984, L.15: About Nd and O18 lags: you imply that O18 is a record of temperature. Nd is a proxy for continental provenance modulated by fluvial erosion and transport. Could it be that rainfall itself lagged behind temperature changes? For instance, I read that “eustatic minima lag a quarter phase behind the July 65°N insolation curve, which is dominated by eccentricity and obliquity (Imbrie & Imbrie, 1980)” in Bijkerk et al., (2014 – Basin Research). What are the relations between orbital forcing, temperature, rainfall and eustasy?

References

Bijkerk, J. F., Veen, ten, J., postma, G., mikes, D., van Strien, W., & de Vries, J. (2013). The role of climate variation in delta architecture: lessons from analogue modelling, 1–35. doi:10.1111/bre.12034

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