in black reviewer comments

in blue our response

in green changes in the manuscript

REVIEWER 1 (Stefan Hergarten)

Dear Authors,

overall, I am quite satisfied with your revisions. Nevertheless, there are a few lingering issues that should be seriously taken into account.

(1) The description of the chi-method is much better now. However, three things still need to be repaired:
(a) Equation (5) is still not correct: The denominator in the parentheses must be K A_0^m instead of K A(x)^m (so the same as in Eq. 6a) and the integral must be exactly the same as in Eq. 6b).

We corrected it.

$$\int \frac{dz}{dx} dx = z(x_b) + \left(\frac{U}{K {A_0}^m}\right)^{\frac{1}{n}} \int_{x_b}^{x} \left(\frac{A_0}{A(x)}\right)^{\frac{m}{n}} dx$$

(b) The statement that all channels "plot in the same location in transformed coordinates chi and z" even if a river is not in a steady state is a bit misleading. In a transient state or under spatially heterogenous conditions, knickpoints are only located at the same chi values, while the z values may differ.

We corrected it.

Even if a river is not in a topographic steady state, the advantage of using the χ plot of its longitudinal profile is that transient signals with a common origin (e.g., fault-related knickpoints), propagating upstream through different channels, along either the main stem or tributaries, plot in the same location in transformed coordinate χ (Perron and Royden, 2013; Schwanghart and Scherler, 2020).

(c) The term "proportional" typically refers to linear relations. So the proportionality of the slope to the uplift rate and the inverse proportionality to the erodibility only holds for n = 1. In order to justify the usage of the chi-slope as a proxy for the uplift rate, however, you do not need the proportionality in the strict sense. So you can fix it by rewording and keep it valid for n not equal to 1. Another little point is that the dependence of the chi-slope on the uplift rate relies on (local) equilibrium conditions. In general, the chi-slope is a proxy for the actual erosion rate, which can be transferred to uplift rates only for equilibrium conditions.

We corrected it.

Finally, according to Eq. (6a), the slope of χ-profiles is dependent to the uplift rate and the erodibility: it increases as the uplift rate (U) increases and decreases as the erodibility (K) increases. In general, the chi-slope is a proxy for the actual erosion rate, which can be transferred to uplift rates only for equilibrium conditions.

(2) Sect. 6: In my opinion, it would still be better to use the same m/n ratio (= concavity index theta) for all considered catchments (perhaps 0.45 after discussing the variations in theta obtained for the catchments). There is no reason why the erosional environment should differ

much among the catchments, which would justify different concavities. Using different m/n ratios, we cannot compare chi-values and thus the locations of knickpoints across catchments. (e.g., lines 485-491). In each case, it should be taken into account that the apparent concavity of S3 and S4 (lines 477-488) is probably owing to the low concavity index.

We corrected it. Accordingly, figs. 9, 10 and 11 have been modified.

To perform χ transformations of longitudinal river profiles, we first have calculated for each basin the m/n ratio in Eq. (6) that minimises the variability of elevation values for similar values of χ (Fig. 8). The obtained values indicate that the larger catchments (S1, S2, S5, S6) are consistent with the widely used reference value m/n = 0.45, while the smaller ones (S3, S4) show lower values. Since all considered catchments are similar concerning their fluvial erosion characteristics and in order to compare χ -values and thus the locations of knickpoints across catchments, a reference value m/n = 0.45 was used to all the catchments.

(3) Fig. 8: Use m/n or theta instead of "mn" and remove "with" in caption.

We corrected it.

Best regards,

Stefan Hergarten

REVIEWER 2 (Colin P. Stark)

The authors have made a solid effort to respond to the reviewers' comments and have revised accordingly. I am happy to recommend publication.

Some technical corrections:

p13, eqn 3 and text on line 46: please don't use δ (one kind of lower-case delta), which means variational or substantial derivative in math/physics; instead use the partial differential symbol ∂ (another kind of lower-case delta)

We corrected it.

$$\frac{\partial z(x,t)}{\partial t} = U(x,t) - K(x,t)\, A\,(x,t)^{\,m} \left|\frac{\partial z}{\partial x}\right|^{n}, \qquad (3)$$

where U is the uplift, A is the drainage area, m and n are positive an empirical constants and K is the erodibility. If both processes are perfectly balanced, a state of a dynamic equilibrium or steady state ($\partial z/\partial t=0$) is assumed.

p16, fig 2(b): typo "insiced"

We are afraid that the term “insiced” does not appear on Fig.2(b), but only on Fig. 3(d)

p17, fig 3(d): typo "insiced"

We corrected it.

p29, fig 8: "mn" should be "m/n" on all subfigs

We corrected it.

p41, l.699: should be "localised"

We corrected it.