We thank the reviewer for the thoughtful and constructive comments. In the following we address the comments and suggestions.

General comments

Overall, I found the manuscript scientifically interesting, well written and structured. The topic is of interested for the geomorphological community, however its acceptance could be strengthened after minor corrections (see details below).

We are pleased that the reviewer appreciates our work and sees it as a contribution to the community. In the following, we address his specific comments.

1. *I would suggest that the authors use a different misfit function for calculating the fit of the model to the data (see details in technical corrections).*

We think it is an interesting suggestion to calculate a model performance metric which considers the analytical uncertainty on the observed data (E_{CRN}). However, errors on CRN data are heteroscedastic: they systematically increase with increasing erosion rates. Although the *ME* thus provides a good metric to evaluate overall model performance, the metric is not well suited to optimize model parameters in an optimization procedure: during the optimization of the model, too much weight will be given on the lower regime of the erosion spectrum where the analytical errors on E_{CRN} are low whereas the higher E_{CRN} data will not be approximated well because of their large associated errors. To compensate for the effect of heteroscedasticity we rescale values of O_i, M_i and E_i using a logarithm with base 10 when calculating *ME*. In the revised version of the paper, the *ME* will be reported as a metric to evaluate model performance, but not to optimize model parameters. Model optimization is done using the Nash Sutcliff model efficiency, and we will explain this in the revised version of the manuscript.

2. *It is not clear if the gained conclusions are applicable or transferable to other settings and therefore how much impact the manuscript will have in the community. The scientific relevance could be significantly strengthened if other available datasets are compared to the presented study (e.g. from DiBiase or Carritier in the the San Gabriel Mountains and the Andes). I hope you find my comments and suggestions helpful.*

We propose a methodology for studying the spatial variability of river incision rates which can be used as a framework to study the coupling between river incision, lithological heterogeneity and climate at larger continental to global scales. However, developing a regional erodibility index and compiling hydrological datasets for regions others than the one studied here would be a project on its own and is therefore beyond the scope of this paper. In the revised version of the paper, we do stress that our findings are based on a study case and that the significance of our results should be tested by applying a similar methodology to continental or global scales.

Technical corrections

Line 16-27: Since there is not word limit on the Abstract youshould give some more details here. For instance, what are the erosion rates and howthey differ in different lithologies/rainfall? Would be nice to have some absolute or rel-ative values on erosion/incision depending on lithology/rainfall.

We will follow the suggestion of the reviewer to extend the abstract. However, since reviewer 2 requested more clarification on the main objectives and conclusions of our paper, we will elaborate the abstract along those lines rather than giving specific values.

Line 38: I would notgive a fixed minimum catchment area since this is site-to-site depending, e.g. Koberet al. (2012) or West et al. (2014) found that nuclide concentrations of larger catch-ments are perturbed by single mass-wasting events.

We will remove the minimum catchment area as suggested

Line 42: Change to '. . .have been found to correlate with a. . .'.

Noted, we will revise.

Line 55: Delete 'external'.

Noted, we will revise.

Line 58-62: Pleaserewrite/reorder this sentence.

Noted, we will revise.

*Line 144: I would suggest to use a different misfit func-tion, since the result is depending on the distribution of measured erosion rates and does not take into account the analytical uncertainties. Use a simple misfit function such as: Misfit=∑_(I= 1...nb)√(((O_i-M_i)/E_i)ˆ2) A misfit of nb or smaller would indicate that you predict the observations within the e.g. 1 standard deviations of all observations (if E is the standard deviation) and a value of 2*nb would mean you are within 2 standard deviations. . .*

$$\sum_{I=1}^{nb} \sqrt{\frac{(O\_i - M\_i)^2}{E\_i}}$$

See reply general comment 1.

Equation (10): Not sure, but have you explained whatKst is?

Thanks for pointing this out, should be *K*. We will revise.

Equation (11): I guess it should be ksn and not ks.
Thanks for pointing this out, should be k_{sn}. We will revise.

Line 182: Please refer to the corresponding equations (4).
Noted, we will revise.

Line 184: Please make sure that all local names of locations, mountain ranges, basins. . .. are shown in a figure for those reader that are not familiar with the geological/geographic setting.
Noted, we will adjust Figure 1.

Line 216: A recent paper (DiBiaseet al. 2018) showed that TCN do not need to be corrected for topographic shielding because of deep non-vertical attenuation paths.
Thanks for pointing us to this paper. Since our paper uses the data as processed in Vanacker et al. 2015 (where a correction was applied), we will keep this section as it is.

Line 378: Would be nice to show that the fits to your data are statistically different for your different complex models. Visually they are look very similar and if I take the confidence intervals shown that overlap.
We agree: the fits for the different scenarios are similar. We feel that our sample size does not warrant a thorough statistical analysis. However, we will add the following sentences to the revised version of the paper:
"Note that differences in model performance between R-SPM scenario 2 and ST-SPM scenarios 5-8 are existent but not very pronounced. To evaluate the significance of these differences, our analysis should be repeated on larger datasets capturing a wider variability in erosion rates and hydrology"

Line 384: I would not use a chapter heading without text.
Given the different topics covered in the discussion section, we feel the use of subsections is warranted here to structure the flow of the paper and to keep the overview.

Line 391: In addition to the supplementary figure please add the position of knickpoints in one of your maps.
Good suggestions, we will adjust the figure.

Line393: Is the baselevel lowering or the uplift increasing, please clarify!

Here we refer to the effect of propagating pulses of river incision. We will clarify: “Facing a sudden lowering of their base level after river rejuvenation, …”

Line 430: Why do you assume that hydrological/climate changes occurred more likely on Myr-time scale compared to timescales erosion rates are averaging over? Please explain this.
We do not know for sure, but given that k_{sn} values integrate over several thousands to millions of years, and CRN data only over 100-100k years, it is *more* likely that the climate has changed over the integration time captured in river steepness than over the time represented by CRN data. We will clarify as such in the text.

Line432: Add ‘. . .timespan of ECRN and ksn measurements.’
Noted, we will revise.

Table 1: Change to ‘Flow resistance. . .’

Noted, we will revise.

Figure 1: The faults and labelling of faults isdifficult to see. Larger line width and fonts, maybe even colour would help. Please show the main streams as lines.

Good suggestions, we will adjust the figure.

Figure 5: Add coordinates.

Figure will be moved to the SI in the new version of the paper.