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Interactive comment on "Numerical modelling landscape and sediment flux response to precipitation rate change" *by* John J. Armitage

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We would like to thank the reviewer for taking the time to review our manuscript. Below we respond to the major comments raised:

(1) Conclusion that rapid response in sedimentary basins more easily explained by using transport model for two reasons: 1) this model has a faster response time (Is this a new finding?) and 2) instantaneous transport of bedload (assumed by stream power model) is not justified for specific case cited in the Pyrenees. But I don't think anyone would argue that stream power model is justified, so this weakens potential impact of paper.

First, we believe that the faster response time for the transport model is a new find-

ing. Second, the stream power model is used to invert river profiles across whole continents (e.g. Rudge et al., G-cubed, 2015), and has been used, for example, in explaining drainage reorganization in catchments draining off the Appalachians into the Eastern North American Margin (Willett et al., Science, 2014). Moreover, it has been recently used to model erosion and the evolution of Miocene Pyreneean megafans (e.g. Mouchene et al, 2017, published in Earth Surface Dynamics this year). We contend that the stream power model is used to model erosion across continents with out much thought for the processes it represents. We also suggest that the treatment of erosional end of the catchment – e.g. do we use a stream power model to generate sediment fluxes is not the same as treating the depositional end – Qs from a stream power model could easily be fed into any type of depositional or stratigraphic model or reconstruction.

To address this point we have substantially rewritten the discussion section about comparison to field sites to make this point as clear as possible.

(2) "a series of experiments": give very brief description of experiments. Also examples of real catchments responding to changes in precipitation would be useful to give readers an broader framing for your work.

We have reworded the paragraph to state:

"In laboratory studies, a series of experiments where granular piles of a length scale of order of centimetres are eroded due to surface water, have demonstrated that a change in precipitation rate leads a period of adjustment of the landscape topography until a new steady-state is achieved (e.g. Bonnet & Crave, 2003; Rohais et al, 2011). These experiments use a mixture of granular silica of a mean diameter in between 10 and 20 μm, that is eroded by water released from a fine sprinkler system above. Given the complexity of these experiments, unfortunately there have been insufficient different precipitation rates studied to fully understand how the recovery time-scale varies as a function of precipitation or other parameters. In this contribution we will focus on this transient period of adjustment to a perturbation in precipitation rates, and using numerical models will attempt to evaluate how the response time varies as a function of the model forcing.

(3) Transport/diffusive models do not produce knickpoints in transient state. Pointing out that knickpoints occur for reasons other than a transient state in advective-dominated systems doesn't change that, nor does it support the motivation for the study in the following lines (L31-33). That said, I think exploring model end member behavior is a worthy goal.

We take the reviewers point and have therefore deleted the text that discusses how knickpoints may form in the absence of a stream power model.

(4) Be more specific here about what experiments show about response time to a perturbation. It's self-evident that there will be a response time for systems to return to steady state.

As mentioned in the response to point 2, we now include a more specific description of the experiments.

(5) Make it clear that this is the new piece this study adds to the existing body of knowledge.

We have added "Sediment flux response times for the advective stream power law have been previously characterised by Whipple (2001) and Baldwin et al. (2003), and for the transport model they have been studied by Armitage et al. (2011) and Armitage et al. (2013), but not systematically or using 2-D models. Furthermore, to our knowledge no comparison between the transport model has been previously made."

(6) Be clear and consistent throughout the paper with terminology when referring to advective/stream power law/detachment limited and diffusive/sediment transport model/transport limited. These are used interchangeably throughout the manuscript. I recommend explaining the meaning of all three descriptions for each endmember

model early in the paper, then using one of the terms for the rest of the paper

We have made sure the terminology is consistent, where we refer to either the transport model or the stream power model.

(7) Why ask the question of if mass transport is appropriate at continent scale when this paper doesn't answer that question. It seems to me the paper addresses the question of if advective transport is appropriate for all models with changing boundary conditions.

Fair point. We have deleted the question. However in this we are not changing boundary conditions, rather the precipitation rate which impacts the transport coefficients.

(8) This discussion of mass transport in suspension seems unnecessary here and unrelated to the point of the paper.

We disagree. If mass transport is not in suspension then it is along the bed, and not rapid. Therefore, if mass is not transported as a suspended load the idea of instantaneous mass transport, which is implicit in the derivation of the stream power law, is wrong and hence the model is nonsense.

(9) General comment about derivations: there is lots of discussion of various exponents, but I would like to see explanations of the link exponent values with natural systems where possible.

We could relate the stream power parameters to nature, but not as a unique parameter value set, rather as a broad parameter value space (see eg. Croissant and Braun, 2014). However, there is an inherent trade off between the constants and exponents in both models that makes relating the m to natural landscapes challenging. Unfortunately we are of the opinion that desire to see a link between the exponents and natural systems is beyond us.

(10) Why do you use different grid sizes for the two models? Does this affect the outcome of the models?

We use a traingluar grid to solve the transport model as this is appropriate for the finite element implementation of the numerical solution. This makes having the same grid size as Fastscape difficult. We have tried to make them as similar as possible.

(11) You've switched from deriving stream power model first, then transport model to discussing transport model first, then stream power. It would help readers follow if you kept the same order throughout the paper, but I recognize that's difficult and perhaps not always possible. Methods: I strongly recommend moving Appendix B to the methods section so readers have a better idea what you're doing and what these models look like before discussing results. This is very important and will help the readability of the paper.

We have reorganized the methods to introduce the transport model first. We have also moved Appendix B into the main body of the text.

(12) It seems to me that it's at least intuitively known that aggradational (drying) events happen more slowly than an erosional (wetting) event, but I couldn't point to a reference for this. If there aren't many studies that show this, I think this makes a very interesting point. If there are studies, they should be cited.

We are not aware of any numerical modelling studies which make the explicit point that aggradational drying events are slower than erosional wetting events. If the reviewer has access to studies that show this, we would be very happy to include these in the references.

(13) Can you say more about catchment response time for the sediment transport model? For example, when is this information useful for evaluating sediment records? I think this point needs to be discussed more thoroughly.

We now specify that the transport limited model produces response times of 10ˆ5 to 10ˆ6 years. Additionally we state that "To evaluate this question with reference to real examples, we need to consider systems in which the timescales of erosion (or as

a proxy, deposition) are known, stratigraphic sections are complete, and the driving mechanisms well-documented (c.f. Allen et al., 2013; D'Arcy et al., 2017)."

(14) What is the justification for comparing paleosols in the Bighorn Basin with paleosols in Claret? Perhaps too much detail in this section to get to the point of rapid deposition.

We have simplified this section and removed the comparison with the Bighorn Basin. We make use of the Schmitz and Pujalte sedimention constraints as suggested by reviewer 2.

(15) Relevance to sediment record and climate change: This section should include a more generalized framework of the circumstances under which the findings of this study are relevant. What is needed in the field/sedimentary stratigraphy to distinguish between a landscape that was depositing under a detachment-limited vs. a transport-limited system? For example, magnitude and duration of precipitation change, magnitude of sediment flux. It's also important to include an expanded discussion of why the sedimentary record at Claret is difficult to explain with an advective model. How much does precipitation have to change in the time period of deposition for an advective model to work? Is this anywhere close to reality? Generally, I want more to back up the conclusion that the transport model explains deposition in the sedimentary basins simply because it has a faster response time.

We now write "Erosional source catchment areas were likely 100 km in length at the time, given the palaeo-geography of the Pyrenees at the time (Manners et al., 2013). The very short duration of the erosional response, which is required for the sediments to be transported and deposited in a timescale of ca. 10ˆ4 years is therefore difficult to model within an advective end-member model for catchments of this scale, although a version of such a model has been recently used to explore the controls on the evolution of later Miocene megafans in the northern Pyrenees (e.g. Mouchene et al. 2017). (e.g. Table 2). To do so would require us to increase the bedrock erodibility parameter, k, significantly within the model (by greater than one order of magnitude), implying slopes and topography in the palaeo-Pyrenees that were highly subdued indeed. In contrast, the sediment transport model more easily reproduces the documented response timescales given an increase in precipitation, is consistent with the volumetrically significant export of bedload transported gravel clasts, and therefore honors the independent field data more effectively. We also note that the transport model displays a response time that has a stronger dependence on precipitation rate change (e.g. Figure 9). We therefore suggest the erosional pulse that led to the deposition of the Claret conglomerate is most appropriately modelled as a diffusive system response to a sharp increase in precipitation over the source catchments of the developing Pyrenean mountain chain at that time."

However it is important to stress that our model actually only deals with the erosional part of the catchment, and it does not address whether any erosional signal is "sampled" into stratigraphy. This is beyond the scope of this contribution (it is an important question in its own right!) and we acknowledge this on p24 of the manuscript.

Finally, we have addressed all the other minor points raised in this review, and would like to thank the reviewer again for going through our work in such detail.

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