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Earth Surface Dynamics Discussions

ESurfD

Interactive comment

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

Anonymous Referee #2

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General Comments: The objective of this paper was explore end-member models of landscape evolution with the goals of finding how changes in model forcing and model assumptions control model response time and whether changes in sediment flux during a perturbation is diagnostic of the end member models. The motivation for the study was whether the end member models can constrain sediment fluxes to depositional basins and allow the interpretation of past climate signals from the sedimentary record. Their main findings were that transport-limited models have a faster response time to a change in precipitation rate and sediment flux is higher in transport limited models. In my opinion, one of the most interesting parts is the asymmetry in the response times of transport limited models, with a longer response time to a drying event than to a wetting event, but this result was not discussed in detail. Overall, the quality of the paper including the motivating questions, the derivation of model equations, and the explanation of model set up (in appendix B) was good. I strongly suggest that the material in appendix B be moved to the Methods section. This will really improve the reading experience. The results were adequately explained, but at times the text was dense with much discussion about the results without references to appropriate figures. The weakest section of the paper was the discussion and conclusions. The authors conclude that a sedimentary record of the PETM is best explained by using a transport-limited model because this model has a faster response time. But a faster response time for one model doesn't rule out another model. I would like to see a more robust support of the sediment transport model in making this conclusion. Second, the authors favor the transport-limited model to explain a particular sedimentary record of the PETM because the 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. In the discussion, I think the paper could benefit from a more generalized framework of the circumstances under which the findings of this study are relevant. For example, 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? I think that exploring end member models to identify diagnostic characteristics of each in transient state is a worthy goal, but the authors could do a better job articulating how the diagnostic characteristics they have identified are useful in a larger context outside of interpretation of sedimentary records.

Specific Comments:

P1L19-21: 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.

P2L6: “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.

P2L15: Can you also point readers to your own modeled examples of landscapes that were created by transport model and stream power model, but are indistinguishable from each other at steady state.

P2L23: Mentioning that river profiles can be inverted to extract climate history at this point seems to bog down the explanation of differences between advective and diffusive dominated systems in transient state.

P2L27-31: 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.

P3L7: typo “dirven”

P3L7-8: 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.

P3L10: Make it clear that this is the new piece this study adds to the existing body of knowledge.

P3L20-21: 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.

Figure 1: include erosion, E, in the figure.

P4L10: 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.

P4L13-14: reference here, e.g. Davy and Lague 2009.

P5L1-8: This discussion of mass transport in suspension seems unnecessary here and unrelated to the point of the paper.

P5L12: define physical meaning of coefficient kp, bedrock erodibility, for readers who are not familiar with erosion models.

P5L20: To this point, the derivation is good, easy to follow for the most part, except authors need to define kp, as noted above.

P5L24-26: alpha (precipitation rate, I found later) is not defined here and makes this section very confusing.

P5L27: more specific definition of kw, width coefficient?

P6L1-5: Before you launch into derivation of transport-limited model, give a few more lines to discussing what this means physically, in a natural system. Also here explicitly say this is the transport-limited erosion equation we use.

P6L23: a fourth name/way of describing stream power as a kinematic wave equation. This is obvious to people who are immersed in the world of erosion models, but many readers are not. So again, be careful and consistent in the terminology used to describe the two end member models.

P6L25: can you reference a figure that shows a migrating knickpoint?

P6L25-27: mentioning shock wave seems unnecessary and distracting to me. 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.

P7L16-19: why do you use different grid sizes for the two models? Does this affect the outcome of the models?

P7L22-24: This is confusing on the first read through. Explain more or reference figure that shows this relationship (maybe Figures 12 or 14)

P7L25: 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.

Figures 2 and 3: These figures are not high impact by themselves. Suggest combining these figures and then give them clear titles indicating which end member model results are shown.

P8L16-17: this was the first time it was clear to me that the heart of this paper is model response to changing precipitation rates. Emphasize this more clearly in the methods section.

P9L2: Sentence that begins, "The response to a reduction in precipitation. . .." is confusing because it's not specific and is a bit of a run on. Needs punctuation or splitting into two sentences to make it clearer.

P9L3-6: Very long run on sentence. This sentence says "importantly, however, it changes the model elevation. . .". Reference a figure where readers can see this change in elevation with changing c.

P9L4: Be specific about where in appendix A readers should look

P9L10-11: Reference one of your figures that shows where these three different sets of parameters result in similar model topography, e.g. figure 14.

Figure 4 and 5: These need titles to make it clear that one is the transport model and one is the stream power model.

Figure 5: indicate knickpoint on figure 5a

Figure 6: this figure must have titles indicating that one is the transport model and one is the stream power model.

P12L3-5: 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.

Figure 7: include in caption these results apply to catchments where L=100km only. Should also make clear where the data to make this figure comes from. Is it just a line through your model data? If made from model data, these should be indicated with points and justification given for how the line was drawn through the model data.

P13L1-2: include reference for knickpoint celerity models, e.g. Berlin and Anderson 2009, JGR

P14L2-3: This seems important that response time takes twice as long when L=500 km compared to L=100km. I suggest a figure showing this difference in response time.

P14L12-13: explain what this empirical evidence of $0.5<h<0.7$ means in a physical system.

Figure 9: show points where you have data from model runs.

P16L2-3: reference figure 7 that shows how models respond to precipitation rate.

P16L6: reference figure that shows similarities between landscapes created by transport models and advective models.

P16L7-9: I appreciate the goal, but what constraints are needed to make this useful? See comment below on conclusions.

P16L13: reference figure that shows difference in response times.

P17L1-2: this asymmetry is interesting. Reference figure that shows it.

P17L7-8: 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.

P18L2: At Claret: mention this is the site in the Pyrenees.

P18L6-10: confusing. 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.

P19L12-14: I don't see why one would attempt explain evolution of a megafan with a stream power model in the first place. This is a bit of a strawman argument.

P18L20-24: Run on sentence.

P19L1-3: Again, it seems obvious that bed load transport is more easily described by a diffusive/transport limited model rather than the stream power model. It would be useful to reference studies where authors have used stream power to model bedload dominated systems. 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.

P19L10-12: I don't think that noting that knickpoints are not a unique indicator of erosional dynamics is helpful for understanding the motivation of this study.

P19L23-24: This is one of the most interesting outcomes of the paper. I would suggest discussing this and the implications of this effect on interpretation of the sedimentary record.

P20L1-4: nicely summarized objective of the paper. I would like to see this in the abstract also Figure 10: Figure needs a legend for the lines Figure 11: It's not immediately clear why both Figure 10 and Figure 11 are necessary. It's explained further down, P22L3-7, but figure 11 is initially confusing.

P22L12-13: show example of how slope-area relationship is sensitive to river network in T-Lim case. Or remove this line as it's not relevant.

P22L14-20: The point of this paragraph is unclear even after reading several times. Rewrite and reword.

Appendix B: As noted above, I think appendix B should be included before results are discussed. It would make the paper flow much more smoothly.

Figure 12: Labels on the figures: transport limited models.

P24L5: typo, should read steady state.

Figure 14: Figure needs label: stream power models. Also, there is an error in the caption. The caption currently says sediment transport model, should read stream power model.

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