

Interactive comment on “The SPACE 1.0 model: A Landlab component for 2-D calculation of sediment transport, bedrock erosion, and landscape evolution” by Charles M. Shobe et al.

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General comments

This paper derives a new formulation for modelling of channel evolution incorporating simultaneous erosion of bedrock and sediment transport, set within the Landlab modelling framework. This modelling framework has the potential to be useful to many within the earth science community by tackling a gap between the two end member scenarios of detachment- and transport- limited models: although previous models between these have been proposed, the inclusion of the authors' model within the

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Landlab framework allows it to be used easily by the community and for the results of the authors' study to be reproduced, as well as being applicable over large spatial and temporal scales. The manuscript is well written, clear, and the derivation of the model is well laid out. I therefore believe that the paper is suited to publication within GMD following to some corrections which I have specified below.

Specific comments

Abstract: previous models have been presented in the literature that include combine erosion and deposition, which the authors review in Section 2, but there is no mention in the abstract of the novelty of the authors' approach compared to these previous methods. The abstract should describe precisely why a new modelling approach is needed for this problem.

Page 1, Lines 18 - 20: sentence needs reworded.

Page 2, Line 5: ‘...the superiority of one model over the other in tests against real landscapes...’: I think it would be useful to include in the introduction some examples of how these different models have been tested against real landscapes. A brief review of this would be useful to the reader, and set the context for providing some demonstration of the potential of using equivalent tests for the SPACE model.

Page 4, Lines 1 - 7: The authors could expand here upon what the limitations are of the erosion-deposition models of Lague (2010) and Zhang et al. (2015).

Page 4, Lines 15-17: Other models have been developed that model mixed-bedrock alluvial channels that are not mentioned here, as noted by Reviewer 1. I think it is worth mentioning here the difference between the reach-scale approach

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generally taken by the development of these mixed bedrock-alluvial models compared to the whole landscape scale that is used as a framework in this paper, to demonstrate the need for and motivation behind development of the SPACE model. It would also be good to discuss the challenges behind scaling reach-scale models up to whole landscapes.

Page 5, Eq 1: Do you think that including a variable channel width would be possible in the model, or would this be too computationally expensive to do for large spatial/temporal scales? I know this is discussed in the previous section as processes which are not treated in the model, but it would be interesting to have an idea of which processes would potentially be possible to include in future developments, and what isn't due to scale issues.

Figure 2: It seems like it would be possible to include the tool effect as well as the cover effect, where E_r would decrease where $H/H^* = 0$ as there are no tools for effective incision into bedrock? Is there potential for this to be included as an option in the model formulation?

Page 8, Line 33: It would be useful to explain 'state function' in some more detail here.

Page 9, Line 6: What is the justification for choosing an exponential decline in erosion at the threshold value here?

Page 12, Line 27: Is there a specific value of q/V above which the model predicts detachment-limited behaviour? It would be good to clarify this here.

Page 13, Line 9: add in reference to equation 11 here.

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Page 13, Equation 29: I like the within-cell calculation of sediment flux!

Page 14, Line 1: If $Q_s = Q_s^{out}$, does this imply that all of the deposition happens at the downstream node of each model cell?

Figure 4: It would be useful to have a plot of a run where you vary the ratio of E_s to D_s to demonstrate how this could affect the sediment thickness as well as the plots where you vary the parameters independently.

Page 18, Line 9: Have you assessed the stability of the model to the timestep? How stable is the model at greater timesteps? 1 year seems like a very short timestep if you wanted to run the model over geological timescales.

Section 6: Although testing of the model against the analytical solutions is useful in showing that the numerical implementation is working (and it's really nice to see that it can replicate both detachment-limited and transport-limited behaviour), this does not evaluate the applicability of the method to real landscapes, especially as the analytical solutions are from the same framework as that of SPACE model (e.g. detachment-limited stream power and transport-limited eqns). What is really interesting is to know how we could validate the model predictions against real landscapes. I think the paper would be improved if the authors could provide some recommendations of how their model can be tested on real landscapes (either natural or experimental), maybe as a section in the discussion (I'm aware it's a model description paper, so actually performing these validations is probably beyond the scope here).

Figure 8: This figure is really interesting. I wonder if it would be possible to compare the different concavities of the channel profiles predicted from the bedrock surface and that of the alluvial layer. Does the concavity of the profile through time give some indication of how detachment-limited or transport-limited the model is at

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that point? I wonder if in real landscapes this could give an indication of transience, or for the transition between detachment- and transport-limited conditions downstream along channels.

Code availability: I like the iPython notebook, it was really easy to use and gives a good idea of the model structure.

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