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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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Response to review by J. Turowski

Author's note: reviewer comments are reproduced here, and our responses are *italicized*.

General comments

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Discussion paper

In the paper, the authors develop a new formulation for river sediment transport and erosion, with a formulation that both honors conservation of mass along the stream, as in transport-limited formulations, and calculates the erosion of bedrock, as in detachment-limited formulations. While I do not have a problem with the model development and the description of the numerical implementation, the literature overview is incomplete and the introduction, review, discussion and conclusions will need to be adjusted. In particular, the author have overlooked a number of contributions (more than half of those that exist by my count, and there are not that many!) that attempted to solve the same problem. These include the Exner-equation-based approach by Inoue et al. (JGR, 2014), the adaption of erosion-deposition models for partially alluviated beds by Turowski (WRR, 2009) and Turowski and Hodge (ESurf, 2017), the 2-D models by Nelson and Seminara (GRL, 2011 and GRL, 2012) and Inoue et al. (JHE, 2016 and ESPL, 2017), and the formulations based on St.-Venant equations (the most relevant paper here is by Fowler et al., SIAM J. Appl. Math., 2007). There might be other papers and the authors should look out for them. Really, the number of publications is not that large, and a review should encompass the entirety of the literature. I think the formulation proposed here is sufficiently different to previous models to warrant publications, but it is definitely necessary to put it into proper context. The discussion could contrast the different model formulations and highlight the differences, advantages and disadvantages of the new formulation. Finally, it would be useful to develop testable hypotheses that can be used to discriminate the various models.

We thank the reviewer for taking the time to review our manuscript, and for their helpful comments. In response to the reviewer's point that several items are missing from our literature review, we have re-written and expanded the introduction (section 1) and literature review section (section 3). The review now includes almost all of the contributions noted by the reviewer. We have also added a section (section 9.3) dedicated to elucidating the similarities and differences between our model and previous models. In response to the reviewer's comment noting that testable hypotheses would be useful *(which was echoed by the other reviewer), we have added section 9.1, which discusses some of the testable hypotheses generated by the examples we present in the paper. Below, we address the reviewer's specific line-by-line comments.*

Specific comments

2.20 I am not too happy with the term 'hybrid' here. This implies that two rather different approaches are put together. I rather see the two model families that are commonly termed detachment- and transport-limited as rather extreme approximations of a single approach. See also the comment to 2.30.

We had been using the term 'hybrid' following Hobley et al (2011), but we have removed that term from the paper. At the end of the introduction, we introduce our review by saying that we will "review existing models that fall between these two end-members."

2.20 There are a number of important contributions missing in this overview. Inoue et al. (JGR, 2014) described a 1-D model based on an adapted Exner equation. There is also the surface-roughness model by Johnson (JGR, 2014; cited elsewhere). Turowski and Hodge (ESurf, 2017) and Turowski (WRR, 2009) adapted the erosion-deposition framework to partially alluviated beds, the latter in a stochastic context (although these papers are more concerned with cover dynamics on the reach scale, rather than sediment routing on the catchment scale). Nelson and Seminara (GRL, 2011 and 2012) and Inoue et al. (JHE, 2016, and ESPL 2017) described fully coupled 2-D models. I'd also like to point out the family of landscape evolution models that sprang from Smith and Bretherton's (WRR, 1972) seminal work. These have since been continuously developed and expanded. Versions of these models including bedrock erosion terms have been discussed by Fowler et al. (SIAM J. Appl. Math., 2007), Smith (JGR, 2010), and Cattan et al. (Math. Geosci., 2017). The Fowler et al. paper is the most relevant here.

We have restructured the review to incorporate these contributions (the only ones we do not include are the Smith (2010) and Cattan et al (2017) papers. We felt that the focus of Smith (2010) on subcritical vs supercritical flow, and the focus of Cattan et al (2017) on numerical techniques made them less applicable than Smith and Bretherton (1972) and Fowler (2007), which we now discuss.

In response to this comment and one from the other reviewer asking for a discussion of reach-scale vs. landscape-scale models, we have added a paragraph to the review discussing the differences in approach between the reach-scale cover models and landscape evolution models incorporating bedrock-alluvial dynamics.

2.30 The work of Hodge et al. (JGR, 2012), Chatanantavet and Parker (WRR, 2008) and Turowski and Hodge (ESurf, 2017) should probably be cited here.

We now include discussions of these three contributions in the review, as well as Chatanantavet and Parker (2009).

2.30 Here, the different concepts of sediment transport and bedrock incision models seem to be muddled. An incision law attempts to predict the bedrock erosion rate, given sediment flux, hydrodynamics, etc. A sediment transport model predicts the sediment transport rates, given the hydrodynamics. Many of the cited erosion models (such as the saltation-abrasion model or the stream power model) were not constructed to include the prediction of sediment transport rates. The assumption that the river is always under capacity, allowing to neglect mass conservation, is separate from this. In essence, there is a description of mass conservation (such as the Exner equation or the erosion-deposition framework) and a description of erosion mechanics (such as the saltation-abrasion model or the stream power model). As the authors are aware, one of these is often neglected in landscape evolution modelling – the mass conservation in the so-called detachment-limited models and the erosion mechanics in the so-called transport-limited models. The authors do seem to be aware of this distinction, as they advocate their formulation as one that might work with different erosion models.

In our revised review, we have been careful to state whether each model discussed a) conserves sediment and b) contains an incision rule. As we note in comment 3.4, we have deleted the term 'under-capacity' because of the confusion around that term existing in the literature.

3.2 Earth capitalized.

Fixed.

3.4 There have been several other potential solutions. See comment to 2.20.

As discussed above, we have restructured the review to include the contributions noted by the reviewer. We feel that the revised review both better acknowledges the general body of work on this topic and better categorizes existing models than the original manuscript did.

3.5 Erosion-deposition models are NOT equivalent to 'under-capacity' models.

We have deleted the term 'under-capacity' due to its confusing use in the literature.

We included this comment originally because Davy and Lague (2009) note that their erosion-deposition model, when the sediment transport length scale is constant in space, becomes equivalent to the model of Beaumont et al (1992). However, we now understand that the term 'under-capacity' in that context may be confused with the detachment-limited assumption (e.g., mass conservation may be neglected). As such, we do not use the term in the revised paper.

3.13 If I remember correctly, this validation is for alluvial rivers, right?

That is correct. To make this clear, we have changed this sentence to read: "The erosion-deposition framework, validated for alluvial rivers by the laboratory experiments of. . . "

3.18 There are two papers that have done these modifications, at least partially: Turowski (WRR, 2009) extended a stochastic Markov-chain model of bedload transport to partially alluviated beds and Turowski and Hodge (ESurf, 2017) described a 1-D model. Both these papers focus on cover dynamics rather than sediment routing.

As discussed above, we have re-written the review to incorporate these two (and other) contributions. Additionally, we have discussed in the review the differences between approaches that focus on cover dynamics vs. those that focus on catchment-scale sediment routing and landscape evolution.

6.21 The exponential model is functionally equivalent to that derived by Turowski et al. (2007, cited elsewhere). If H is the average height of the sediment, then this H scales with the total mass of sediment residing on the bed.

We have referenced Turowski et al. (2007) in this sentence.

8.33 To me, ‘shown’ seems to be an overstatement here. Also, the meaning of state function may be unclear to readers in the current context.

We have substantially revised section 4.3.1 to make our meaning more clear. In so doing, we have replaced “shown” with “. . . the sediment transport literature suggests. . . ”. We have also removed the term “state function.”

9.5 It is unclear why this approach is deemed necessary and why this particular function is chosen. The motivation for a different approach seems sufficiently clear, but the authors could better describe their train of thoughts for arriving at eq. (9).

In our re-writing of section 4.3.1, we have clarified our thought process with regards to the development of the smoothed-threshold functions. The added text is copied below.

“If a distribution of thresholds exists, erosion should decline to zero not exactly when available stream power drops below the user-defined threshold as would be the case for a standard threshold model, but when available stream power is significantly below the defined threshold. As available stream power becomes larger than the defined threshold, entrainment and erosion should increase smoothly as a greater portion of the distribution of thresholds is exceeded. In the limit where available stream power is many times greater than the user-defined threshold, available stream power should simply be reduced by the user-defined threshold. An exponential function describing the increase in entrainment/erosion as available stream power increases relative to threshold stream power satisfies these requirements without adding any model parameters. We include an optional exponential expression for threshold stream power. . . ”

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Discussion paper

10.6 The formulation seems a bit cynical here – either the model is a good representation of reality, and then one should just have to deal with sharp discontinuities, or it is not. To choose a particular model set up to ease the analysis of the results (or to recommend it) is rather unscientific.

We have re-focused section 4.3.1 and deleted the sentences about the relative ease of model analysis. Our main purpose with the smoothed-threshold approach is to provide a mathematically simple way to honor the reality that entrainment and erosion thresholds are not generally simple constants, and to do so without adding any additional model parameters. We believe that exponential smoothing of the erosion and entrainment thresholds is a reasonable representation of reality. To avoid confusing the reader and/or distracting from this main point, we deleted the sentences relating to how discontinuities in a model response surface can hinder analysis.

10.12 Eq. (11) holds only if the sediment and the water move at the same speed. The (mass) concentration is defined as $M_{sediment}/M_{water}$ for a control volume. The mass is related to the mass transport capacity (Q_s with units kg/s) including sediment velocity $V_{sediment}$ as $M_{sediment}$ * $V_{sediment} = Q_{sediment}$ * transport_length (and a similar equation for water).

We have added the following sentence to the end of section 4.4: "Eq. (11) assumes that sediment and water move at the same speed such that all changes in $\frac{Q_s}{Q}$ are driven by erosion and deposition."

11.8 Eq. (18) may be clearer if the common factors in the two terms are taken out of the parenthesis. In effect, this is a standard stream power model long profile, with an erodibility coefficient that is an inverse sum of the coefficients for bedrock and alluvium.

We have added equations making this change. The form of equation 18 was intended to facilitate comparison with Davy and Lague (2009), who showed their slope-area relationships in a similar way. However, we agree with the reviewer's point that putting the S-A relationship in a form more familiar to the basic stream power prediction will likely be helpful for readers. We have added one more step to our derivation (now equation 19) in which we separate U and A^m from the two additive terms. In addition, we take this opportunity to add text to address the reviewer's point (made in comment on 20.5) that constraining $\frac{V}{r}$ in natural channels would be useful. Our additional text and equations are shown below.

"*Eq. (18) may be rearranged to show that SPACE predicts a standard stream power slope-area relationship modulated by $\frac{V}{r}$ as well as sediment and bedrock erodibility:*"

$$S = \left[\frac{V}{K_s r} + \frac{1}{K_r}\right]^{1/n} U^{1/n} A^{-m/n}. \tag{1}$$

"The ratio between the effective settling velocity V and the runoff rate r controls the relative importance of the bedrock and alluvial components of the steady-state channel slope. In the simplified case of $K_s = K_r$, a ratio of $\frac{V}{r} = 1$ would indicate equal contributions from the two regimes. Quantifying $\frac{V}{r}$ for natural systems could therefore give a valuable indication of process dynamics in natural channels."

14.1 This sentence is rather awkward. Consider reformulating.

We have changed this sentence to read: "Because sediment deposition in a cell depends on both Q_s^{in} from upstream and sediment entrained from the cell itself, we can substitute Q_s^{out} for Q_s in the deposition term. Eq. (29) may then be solved to yield the local analytical solution for Q_s within a model cell:"

17.4 Earth capitalized.

Fixed.

20.5 Eq. (43) may be clearer if the common factors in the two terms are taken out of the parenthesis. In effect, this is a standard stream power model long profile, with a modulating factor depending on runoff and settling velocity. It would also be interesting to quantify this factor to see how far typical values are different from one.

We have added one last step in our derivation (now equation 45) to address this point. The new equation, shown below, should be more familiar to people used to working with the standard stream power model.

"Eq. (45) may be re-written to show that it predicts a standard stream power slope-area relationship that is modified by the ratio of settling velocity to effective runoff:"

$$S = \left[\frac{V}{r} + 1\right]^{1/n} \left[\frac{U}{K_s}\right]^{1/n} A^{-m/n}. \tag{2}$$

In response to the comment about quantifying V/r, we agree that because this ratio is an important control on the transport-limited vs. detachment-limited model behavior, it is worth quantifying. At this point however, we feel that the constraints on V are too poor to facilitate adequate comparison. We intend to focus future work on unpacking V so it no longer lumps together so many processes and variables. We have added text in section 4.5 (after equation 19) to emphasize the importance of this ratio to the reader, and to suggest that quantifying it in natural channels could be a valuable exercise.

"The ratio between the effective settling velocity V and the runoff rate r controls the relative importance of the bedrock and alluvial components of the steady-state channel slope. In the simplified case of $K_s = K_r$, a ratio of $\frac{V}{r} = 1$ would indicate equal

contributions from the two regimes. Quantifying $\frac{V}{r}$ for natural systems could therefore give a valuable indication of process dynamics in natural channels."

24.20 The test results do not indicate that the model is useful for natural settings as claimed here. They just demonstrate that the numerical implementation is working.

We have removed this sentence, as the preceding sentence already makes the point that the numerical implementation is working.

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