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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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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.

Comments by line

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.

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.

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.

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.

3.2 Earth capitalized.

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

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

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

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.

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.

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.

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).

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.

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).

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.

14.1 This sentence is rather awkward. Consider reformulating.

17.4 Earth capitalized.

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.

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.

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