Earth Surf. Dynam. Discuss., 2, C216–C220, 2014
www.earth-surf-dynam-discuss.net/2/C216/2014/

Interactive comment on "Macro-roughness model of bedrock-alluvial river morphodynamics" *by* L. Zhang et al.

Anonymous Referee #2

Received and published: 28 July 2014

In the article, the authors provide a unified description of alluvial processes and fluvial bedrock erosion by saltating bedload. The mathematical development is interesting and seems sound. Nevertheless, I am critical about the style of presentation, to structure and the choice of content, and I have a number of comments that need to be addressed.

There seems to be a confusion as to what the saltation-abrasion model was intended for in its original form (as of Sklar and Dietrich, WRR 2004). This was a process-based description of the physics of fluvial bedrock incision by saltating bedload particles. This gives a point-description of this process under steady conditions. Partial spatial upscaling of the model has been given by Sklar and Dietrich, Geomorphology 2006, who used reach-scale scaling laws and assumed a non-depositional environment, and by Lague, JGR 2010, who provided a numerical solution in the SSTRIM model framework. In addition, I have recently reviewed two other papers proposing spatial upscaling models, and the authors should look out for in press papers on this topic.

In essence, what the authors do is provide a spatial upscaling using a form of the Exner equation that is designed to for treating sediment dynamics over a bedrock bed. This is a novel approach and can meaningfully add to our understanding of bedrock channel dynamics. However, most of the paper reads like a showcase of what the model can do in comparison to what the saltation-abrasion model cannot do, without going into depth in discussion or highlighting as what was learned by the exercise. There are a lot of inappropriate and unnecessary statements (e.g., page 326, line 20 'CSA cannot even be implemented for this case').

There a large number of examples of the capabilities of the model, without in-depth discussion or comparison to real-world examples. With close to 12000 (excluding abstract, appendices and references!), the manuscript is very long. In addition, the writing is technical and mathematical, and difficult to follow due to many symbols and abbreviations. The discussion does not place the work into the current body of knowledge and direct links to field observations are missing.

I suggest the following: Rewrite the manuscript both in style and in structure, with a focus on legibility for an earth science readership, who may not have a strong mathematical background. Be more careful as to how both the saltation-abrasion model and this new development are protrayed. Provide a clear message on what novel things we learn from the paper. Cut out several of the examples, and focus on just a few where the progress made by this new formulation can be clearly illustrated, best in comparison with field examples. In the discussion, put the work better into the context of what is already known. In particular, and these suggestions are not exhaustive, the authors could compare with the model of Lague, JGR 2010, or how bedrock channels respond to large earthquakes as described by Yanites et al., Geology 2010, and other authors.

ESurfD

2, C216–C220, 2014

Interactive Comment

Page.line

298.9 . . .rise to unphysical consequences

300.13 opening parenthesis missing

300.16 field evaluations were also done by Hobley et al., JGR 2011, and Turowski et al., JGR 2013.

300.29 A third restriction (also close to the issues the authors try to address in the paper) is that the saltation-abrasion model is formulated for steady conditions.

301.1 and following: The transition from eq. 1a to 1b is not obvious and needs to be elaborated. In my mind this is only true for steady conditions. Turowski et al., JGR 2007, provided a discussion of this point to some detail.

307.9 Although it has been used in reach-scale applications later, originally, the saltation-abrasion model was developed as a process model, designed to describe incision physics in a small reference area with constant conditions. Lague, JGR 2010, used a compartment approach to address the spatial upscaling.

307.24 Again, the model was not originally developed to describe erosion in an entire reach.

308.3 reflect

308.4 Again, this is overstretching the model to applications that it wasn't developed for originally.

309.21 Which bedrock surface?

309.22 Which rough layer?

309.20-24 The wording here implies that the hollows are evenly filled from the lowest parts of the surface. This assumptions needs to be stated explicitly? Does it make sense? The experiments of Johnson and Whipple, ESPL 2007 and JGR 2010, and of

ESurfD

2, C216–C220, 2014

Interactive Comment

Finnegan et al., JGR 2007, could be informative here.

309.25 The term bedrock “base” is not self-explanatory in this context. It needs to be stated explicitly that z = 0 at the lowest-lying bedrock surface.

310.4 Why should all the hollows be filled with sediment to the same level?

310.17 This sentence does not make any sense to me.

315.17 ‘Figure 4 cannot be precisely correct’ – this does not make any sense. Maybe some elements depicted in the figure cannot be precisely correct.

315.19 Why does the elevation variation have a random element? This can be measured to high precision in the field. The statement needs to be reformulated.

315.15-26 I cannot follow this line of argument. From a physical perspective, clearly, there can be a clean bed and the cover fraction would equal zero precisely at z=0. The boundary condition as z goes to minus infinity does not make any sense to me. In a physical interpretation, this would imply that there are infinitely deep pockets in the bedrock! It seems to me to be an unsatisfying ad-hoc fix of the before-mentioned problem. At least, the reasoning applied here needs to be much better explained.

316.2 No, they shouldn’t. That seems unphysical to me and makes little sense. I cannot see a way as to how this definition can be meaningfully applied in the field or in experiments.

317.21 Maybe, but the model does not describe incision near waterfalls, and thus the theoretical description breaks down at this point.

318.1-4 Why this change with regard to the literature? Note that it is currently unclear whether these equations are valid at all for bedrock-floored channels.

321.5 Lague, ESPL 2014, has provided a recent review of natural slope-area scaling

326.13 This is not very helpful. The authors could summarise the model values used in the different examples in a table instead. Similar at other points in the manuscript.

326.11, 20 What is the point of such statements? CSA was not developed for these applications, and the statements are completely useless; moreover, they take space in an already long article.

Fig. 12 & 13: The current versions of the figure is so small that it is unreadable.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 297, 2014.

ESurfD

2, C216–C220, 2014