REPLIES TO COMMENTS OF REFEREE 2 ON: MACRO-ROUGHNESS MODEL OF BEDROCK-ALLUVIAL RIVER MORPHODYNAMICS

Replies to comments of referee 2 on: Macro-roughness model of bedrock-alluvial river morphodynamics

We have found your comments quite helpful, and we believe that they have helped sharpen the text.

COMMENT 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,

REPLY We hope that we have corrected this in the modified version. We have revised the paper to point out (in two places) that the original Sklar-Dietrich model of 2006 is an 0D model, and that our new formulation is more directly applicable to long reaches because it routes sediment downstream. Sklar and Dietrich (2006) "upscale" Sklar and Dietrich (2004), and Lague (2010) "upscales" a modified version. Our "upscaling" is defined in terms of our "Highly Simplified Reach" and our hydraulic assumptions. We prefer not to call this "upscaling"; it just means, to us, an application of the model to settings which require other constraints. But perhaps the diffusion equation represents an upscaling of Fick's law.

COMMENT There are a lot of inappropriate and unnecessary statements (e.g., page 326, line 20 'CSA cannot even be implemented for this case').

REPLY We have edited the text to reword or eliminate 15 places where the wording might be inappropriate in this regard. For example, instead of "but are not captured by CSA", we now say "but are not captured by models which assume a relation for cover based on the ratio of sediment supply to capacity transport rate, i.e. Eq. (2)." We have also clarified in 3 places that CSA was never designed to handle mixed alluvial-incisional processes.

COMMENT There are a large number of examples of the capabilities of the model, without in-depth discussion or comparison to real-world examples.

REPLY The reviewer is correct, but we hope that he/she will bear with us on this point, for the reasons described below. The main purpose of this paper is to introduce a new formulation, MRSSA, which has capabilities that go beyond existing models which assume a relation for cover based on the ratio of sediment supply to capacity transport. We show four cases which have clear physical analogs. We originally developed the model to describe the effect of waves of alluviation and evacuation on bedrock incision. We found, however, that such a paper could not be written within a reasonable word length without first deriving and demonstrating the MRSSA model itself. The reviewer was kind enough to point us in the direction of a paper by Yanites et al. (2010), which is now referenced in the text. Assuming the present paper is accepted, Yanites (2010) will be invaluable toward our placing in field context (in a paper for which the MS already exists), the problem the effect of waves of alluviation and evacuation on incision.

COMMENT 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.
REPLY We feel that the mathematical analysis of advection-diffusion is a useful contribution. It is of considerable value to know the general characteristics of governing equations before solving them. It is also necessary to know these characteristics in order to set boundary conditions. Having said, this, we have taken to comments of the reviewer to heart. We now inform the reader and the end of Section 3.1 that he/she may skip Sections 3.2 and 3.3 if they prefer to go directly to applications.

COMMENT The discussion does not place the work into the current body of knowledge and direct links to field observations are missing.
REPLY At the advice of the referee, we have added five new references to the discussion, both to better survey the current body of knowledge and to provide a better link to field observations. The papers are Johnson and Whipple (2009), Yanites et al (2010), Hobley et al. (2011), Turowski et al (2013) and Lague (2014)

COMMENT 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.
REPLY We have implemented this advice in the following way. 1. We now provide a road map, and the end of Section 3.1, which allows a less mathematically inclined reader to bypass the densest o the mathematics. 2. As described above, we have now taken great care in comparing MRSSA CSA. In our conclusion, we have specifically described 9 novel results, so we have not changed this text.

COMMENT In the discussion, put the work better into the context of what is already known.
REPLY As noted above, we believe that we have implemented this suggestion.

COMMENT 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.
REPLY In the original version, we had only 1 reference to Lague (2010). Now we have 6, and we think that this has strengthened the paper. We also now specifically refer to Yanites et al. (2010).

COMMENT 298.9 : : :rise to unphysical consequences
REPLY Done

COMMENT 300.13 opening parenthesis missing
REPLY Done

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

COMMENT 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.
REPLY Now noted in text

COMMENT 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.
REPLY As far as we can see, this a simple algebraic manipulation, i.e. (1a) + (2) $\rightarrow$ (1b). We have nevertheless modified the text for clarity.

COMMENT 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.
REPLY In our paper, we have addressed this in terms of our HSL, highly simplified reach., which adds the constraints necessary to evaluate MSRAA at field scale.

COMMENT 307.24 Again, the model was not originally developed to describe erosion in an entire reach.
REPLY We have responded by changing the wording from “cannot describe” to “does not describe.” The fact that the model does not describe entire reaches is a different issue than whether or not the model holds when the bed undergoes transitions between partial and complete alluviation. These transitions can be local.

COMMENT 308.3 reflect
REPLY done

COMMENT 308.4 Again, this is overstretching the model to applications that it wasn’t developed for originally.
REPLY We have added the sentence, “That is, the model was not designed to route sediment in the downstream direction.”

COMMENT 309.21 Which bedrock surface?
REPLY Sentence completely reworded for clarity

COMMENT 309.22 Which rough layer?
REPLY Again, sentence completely reworded

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

REPLY We now point out that this is a limitation of a 1D model, and quote Johnson and Whipple (2007) for the 2D case. More specifically, we have added following material. “Since it is a 1D expression of sediment conservation over a bedrock surface, it cannot capture 2D variation, which will result in a more complex pattern that that shown in Fig. 4, and in particular will provide more connectivity between adjacent pockets. This two-dimensionality is known to have an effect on the pattern of incision, as illustrated by Johnson and Whipple (2007). The extension of the formulation to the 2D case represents a future goal; some relevant comments can be found in the section “Discussion”.

COMMENT 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.
REPLY We revised a sentence for clarification: “Now let z be the elevation above the bedrock “base” as shown in Fig. 4, so that $z = 0$ at this base, and λ_p be the porosity.”

COMMENT 310.4 Why should all the hollows be filled with sediment to the same level?
REPLY We can see how the reader might be confused, and have modified the text. “It should be noted that in Fig. 4, no bed elevation variations are shown over part of the bed where alluvium is exposed. This is done only for simplicity, and reflects the condition that in the clast-rough case considered here, grain size is small compared to macro-roughness height. Fig. 4 also contains another simplification, in that all pockets are assumed to be filled to the same level by alluvium. While this condition not likely to be true at the local scale, it is a reasonable first approximation when averaging over an appropriately defined window. We have also modified the captions of Figures 4 and 5 accordingly.

COMMENT 310.17 This sentence does not make any sense to me.
REPLY There was an extra “for”, now removed. Thank you for catching this.

COMMENT 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.
REPLY We see that we have indeed confused the reader. Here is our modified text. “The form of the cover relation of Eq. (17a,b) serves to introduce the MRSAA model in a simple way. It does, however, contain a flaw in regard to the clast-rough case considered here. Specifically, p_c vanishes for the case $\eta_a = 0$. This implies that there no deep pockets in the bedrock which retain sediment that is not available for transport. This physical limitation places a limitation on the applicability of the MRSAA model, which we identify and use to amend the formulation in this section.
According to Eqs. (23b) and (17), as alluvial cover thickness η_a goes to 0, the cover fraction p_c also tends to 0, and thus the downstream-directed alluvial wave speed c_a tends to infinity. That is, alluvial waves of infinitesimal amplitude travel with infinite speed. In physical terms, this corresponds to a very few grains racing over a very smooth surface.”
We use these statement to motivate our modified formulation

COMMENT 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.
REPLY We hope to convince the referee of another context. Please refer to Fig. 1. The fact that something can be measured precisely does not imply that it is not random. Some kinds of

turbulence can now be measured precisely, but turbulence is still random. The number π has a precise definition, but the train of digits is random. And in the event, even though one could measure them, how could one possibly incorporate all the details of the variation of bedrock elevation apparent in Fig. 1 into a tractable model? Even Direct Numerical Simulation for turbulent flow around the roughness elements is not yet possible.

COMMENT 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.

REPLY We can again see why the reader is confused, and have extensively modified the text in several places. We reproduce some of these below.

"The specific case we consider here is one for which a) the bedrock surface is rough in a hydraulic sense (as opposed to a hydraulically smooth or transitional surface; see Schlichting, 1979), and b) the characteristic vertical scale of bedrock elevation fluctuation about a mean value based on an appropriately defined window, here denoted as the macro-roughness L_{mr} of the bedrock, is large compared to the characteristic size of the clasts constituting the alluvium. We use the term "macro-roughness" so as to clearly distinguish it from hydraulic roughness, which is specifically defined in terms the logarithmic velocity profile. Inoue et al (2014) have introduced the terms "clast-rough" and "clast-smooth", the former referring to a bedrock surface roughness that is large compared to the characteristic size of the alluvium, and the latter referring to a bedrock surface macro-roughness that is small compared to the size of the alluvium. Here we consider the clast-rough case."…

"The form of the cover relation of Eq. (17a,b) serves to introduce the MRSAA model in a simple way. It does, however, contain a flaw in regard to the clast-rough case considered here. Specifically, p_c vanishes for the case $\eta_a = 0$. This implies that there no deep pockets in the bedrock which retain sediment that is not available for transport. This physical limitation places a limitation on the applicability of the MRSAA model, which we identify and use to amend the formulation in this section."

"According to Eqs. (23b) and (17), as alluvial cover thickness η_a goes to 0, the cover fraction p_c also tends to 0, and thus the downstream-directed alluvial wave speed c_a tends to infinity. That is, alluvial waves of infinitesimal amplitude travel with infinite speed. In physical terms, this corresponds to a very few grains racing over a very smooth surface."

"This unphysical behavior can be resolved by considering the bedrock elevation variation in a statistical sense."

COMMENT 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.

REPLY See the material immediately above for what we hope is a much better explanation of the general idea, In regard to infinity, we have reworded as follows. "Instead, in so far as bedrock elevation variation has a random element, which can be seen in Fig. 1, the appropriate conditions are $p_c \rightarrow 0$ as $z \rightarrow -\infty$, and $p_c \rightarrow 1$ as $z \rightarrow \infty$. Here the symbol "∞" is mathematical shorthand for "appropriately large".

Please understand that we are talking about a probability distribution. Even a Gaussian distribution has tails, and so do the pdf's used in Lague (2010) for discharge and sediment supply. Please see the figure below from Parker et al. (2000), representing the PDF of bed fluctuations of an alluvial bed covered with dunes. The meaning here is that when the flow has removed all the sediment than it can remove, there are deep pockets, not infinitely deep. where residual sediment will remain, as explained immediately above.

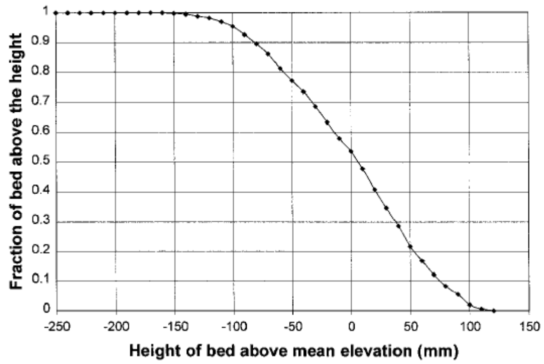

FIG. 7. Function $P_s(y)$ Denoting Fraction of Bed Record Measured at Point That Was above Level y, Where y Is Measured Relative to Mean Bed Level, as Determined for Run 33

COMMENT 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.
REPLY Please look at Fig. 1 and the probability distribution for a bed covered with dunes immediately above. Both dune elevation and elevation of bedrock with macro-roughness elements of Fig. 1 are amenable to a probabilistic characterization.

COMMENT 317.21 Maybe, but the model does not describe incision near waterfalls, and thus the theoretical description breaks down at this point.
REPLY We have modified the text as follows. "The model thus implicitly predicts the formation of a hanging valley."

COMMENT 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.
REPLY Allow us to explain. The original data set used by Meyer-Peter and Muller is much more extensive than that of Fernandez Luque and van Beek. Wong and Parker (2006a) re-analyzed the MPM data and obtained the coefficient and exponent used herein. Of course it is unclear as to whether or not they are valid for bedrock-floored channels. But if one is going to use such a relation as a first-order approximation, one ought to try to use the best relation available.

COMMENT 321.5 Lague, ESPL 2014, has provided a recent review of natural slope-area scaling
REPLY Reference now added

COMMENT 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.

REPLY It seems clear enough to us. We originally had a table, but because of variations between the cases, it became less readable than the text.

COMMENT 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.
REPLY As noted above, we have reworded not just here, but in a total of 15 instances to meet the referee's valuable suggestion.

COMMENT Fig. 12 & 13: The current versions of the figure is so small that it is unreadable.
REPLY We have corrected this in the revised version.