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Interactive comment on "State of the Art Study of Influence of Bed Roughness and Alluvial Cover on Bedrock Channels and Comparisons of Existing Models with Laboratory Scale Experiments" *by* Jagriti Mishra and Takuya Inoue

Anonymous Referee #1

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Review of Mishra and Inoue: "State of the art study of influence of bed roughness and alluvial cover on bedrock channels and comparisons of existing models with laboratory scale experiments"

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In this contribution, the authors aim to review and evaluate several recent models for how alluvial cover on the bed of a bedrock channel evolves as the ratio between bedload flux and bedload transport capacity changes. They present the different models,

and then they describe a set of new flume experiments they conducted to evaluate the models. They also re-analyze some previously published experimental data in order to compare the models. They show that the effectiveness of several of the evaluated models depends on whether bedrock roughness is less than or greater than the roughness of the bedload sediment. They further show that only some subset of the evaluated models that can match observed "runaway alluviation" type behavior for smooth bedrock beds.

This paper attempts to do something that is important, which is to compare a fairly wide variety of recently published models for how fluid flow, sediment transport, and bed roughness interact to set alluvial cover in bedrock channels. As such, the paper could make a useful contribution to ESurf. However, the paper in its current form is not complete enough to warrant publication, and there are several major changes that would greatly improve its eventual impact.

I very much hope that my suggestions can help the authors present this study in a more complete way. I want to stress that this is important work and will make a nice contribution once it is more thorough and polished.

I focus more on general comments because the paper requires a substantial amount of restructuring and additions. I follow with the more salient line comments.

General comments:

These could all be boiled down to one point, which is that the writing needs to be heavily restructured and expanded so that readers can understand and rigorously evaluate the work.

1) This is billed as a review paper, and indeed it has the potential to be a very nice evaluation of several existing models. However, the portion of the paper that actually reviews previous work is extremely short. The entirety of the review is contained in less than 150 lines of text (section 1). For each model that the authors propose to evaluate,

there should be a more complete description of how the model actually works, what any major assumptions are, what the key parameters are, and perhaps most importantly for this paper, what are the key predictions that each model makes that distinguish it from the others being evaluated. As an example, lines 116 to 125 present a very abbreviated description of Johnson's (2014) roughness model. I have read Johnson's paper two or three times over the years, and I still found this summary of their work hard to follow. The same goes for the work of Turowski and Hodge (2017). The quick summary of their work does not tell the readers almost anything about how their model was derived, except by some unspecified probabilistic approach. The papers being reviewed here are without exception very thorough pieces of work; each model description should be accompanied by at least a paragraph helping readers understand the model in greater detail, with specific reference to what diagnostic outcomes are expected from each one. Of course there is no need to re-do the derivations, but a little bit of extra explanation would go a long way to helping readers understand.

2) Along those same lines, there are really two different types of models being investigated here. There are the Inoue and Johnson models, which address the interplay between roughness and critical shear stress. Then there are the other models, which if I'm not mistaken look at sediment cover as a function of the ratio of sediment flux to transport capacity without dynamically modifying the critical shear stress. This fundamental distinction between model types is not clear in the introduction and review.

3) The methodology by which all relevant quantities are calculated is not clear. For example: how was k_sb calculated? Line 112 gives an expression for it, but it seems to me that if that expression was used, there would be a perfect correlation between sigma_br and k_sb because you just multiply by a couple of parameters. Then in section 2.3, Manning's equation somehow comes into the calculation. Why is Manning's n calculated? How is it used? If it is used to determine a bedrock roughness parameter, how are the weird dimensions of Manning's n reconciled such that both quantities in Figure 2 are in meters? It may be that I am just not understanding, but I am familiar

with this literature. If I don't understand then other readers may also have trouble.

4) Similarly, the experimental methodology in general needs to be more thoroughly explained. Section 2.4 is a good example of this. Measuring the critical stress is important to ultimately testing Inoue's and Johnson's models, but line 185 for example is not clear at all about how the ultimate values used in figure 8 were measured/calculated.

5) Also on section 2: please consider stating very clearly in this section what the structure of your experimental design was. Meaning, what changed between each "run" in a given group (say, the set of "run1" experiments). From Table 1 I gather that each group of runs is for a different roughness condition, and then the sediment feed rate was varied within each roughness condition, but information this fundamental to the paper should not have to be hunted down in a table.

6) It is in many cases not clearly why certain decisions were made (i.e., little explanation or justification is given). For example, why can't the other models be tested against Chatanantavet and Parker's results (Figure 11)? If there is some obvious reason, then that's fine and it can just be stated. As it is, it is hard to tell what the rationale was for many choices made in the experimental setup and analysis.

7) For the results: there is a lot of information that is only defined very late in the paper that should be in the introduction/background. For example, the word "hysteresis" does not even appear in the earlier sections, but becomes a focus of much discussion later in the paper. Similarly, the definition of smooth and rough beds that appears in line 300 should be moved to very early in the paper.

8) Discussion/conclusions: The conclusions report what the study found, but they do not do an effective job of zooming out and telling readers how this improves our understanding of bedrock-alluvial river processes. What does it mean that both the Inoue and Johnson model can reproduce the experimental results? What is the implication of that fact that the Turowski and Hodge model can replicate the results, but needs some parameter adjustments? There's an opportunity here: the success (or not) of various

models should tell us something about how we should be modeling these processes in the future. It would be worth trying to distill for readers what we have learned from this exercise.

9) It's the editor's place, not mine, to decide to what extent this is a problem, but I feel that I need to point it out: the English language writing and usage in this paper is flawed. I appreciate that writing in a second language is difficult, and that our community benefits greatly from having viewpoints from all over the world. There is no reason why the English has to be perfect. However, in this paper the writing is in many places difficult to follow. This unfortunately makes it very hard to understand what the authors are trying to say, so the impact of what could be a very interesting paper is hidden behind confusing language. Primarily these issues relate to verb tense, word choice, and sentence structure. My suggestion is that the authors use an English editing service, or find a native speaker who will carefully go over the paper.

Line comments (not including English usage comments, please see #9 above):

18: This rationale for the study is interesting; it would also be good to mention the more "traditional" geomorphic importance of bed cover, which is that it ultimately is a control on river and landscape evolution over geologic time.

30: I believe the reference is Johnson and Whipple 2010.

40: We should not still be in the Introduction when some of the candidate models being tested in the paper are introduced. As noted above, please try to devote an expanded subsection to each relevant model so that the reader can tell what is actually being tested.

50-55: The discussion of Hodge and Hoey feels out of place. It is obviously a relevant paper, but try to state specifically why you are discussing it here.

72: The same goes for the Aubert paper; relevant work, but it is not a candidate model you evaluate, so going as far as to reproduce on of their equations is a bit of a distrac-

tion for the reader.

Section heading 1.1: consider revising this header to better clarify what you mean.

109: As discussed before, please separate the descriptions of the different models. At the very least with a new paragraph, but ideally in their own subsections where you can more thoroughly discuss how the models work and the predictions that each model makes.

156/160: If the mortar is non-erodible, how can the bed be protected from "further erosion?"

Table 1: I don't remember seeing the Froude number defined anywhere. This could be done in a caption for table 1.

171: See general comment #3; I am curious to know what the purpose is of calculating Manning's n, and how the units are reconciled for any application of the values.

185: The wording here is confusing; rephrase for clarity

Figure 5 and others: Please use the variable symbols complete with subscript, i.e. ksb instead of ksb

Figure 6: say that the black line is the 1:1 line

Section 3.3: this is a compelling result, but it would be good to add a couple of sentences about what the implications of this result are for the model comparison.

Section 4: Am I wrong in thinking that Johnson's model needs to be calibrated before it can be compared against the data as presented on Figure 8? If so, section 4.2 should come before the description of Figure 8.

Figures 9 and 10: Why is the Turowski and Hodge model compared separately from the others? It's not necessarily bad, but it would be good to explicitly state why this was done.

Notation table: please provide units for all quantities

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