Review of manuscript "Bedload transport controls intra-event bedrock erosion" by A. R. Beer and J. M. Turowski

Review by Phairot Chatanantavet, Lehigh University USA

I like this paper, and think that it deserves publication in your journal after some minor revisions I have listed below. This paper is the first that measured artificial bedrock erosion rate in the field setting with state-of-the-art technology. It is a nice short paper and all figures are suitable and necessary – Figure 1 is very effective. I like the fact that the study tested so many incision model types based on the sediment effect terms and exponent uses. I think the Appendix section is very effective. The method section is short and may need more detail and clarity. I think the results and discussion sections are good. The abstract and introduction are well written, but the conclusion section needs to be polished, I think, and maybe added the points I make below.

I am not sure if I agree with the other reviewer that Hy should be called hydraulic parameter instead of sediment effect since it includes the sediment motion threshold. It is arguable. I am fine either way. It is kind of clean to say they are all "sediment effect" terms.

Main comments

- The idea of the suspension term (Se) in your equation 1 was proposed by Sklar and Dietrich 2004 without any empirical data to support the term (first arisen in their saltation length formula), and this term incorrectly affect the saltation length for bigger grains. They just came up with it to support their idea that all grains (cobble or sand) should be suspended when shear velocity exceeds particle fall velocity, which is not necessarily true (from my flume observation as detailed in Chatanantavet et al. 2013 JGR-ES). And I have calculated a few times before also. For example, in Johnson and Whipple 2007 data. If you use the hydraulic data with high transport stage in J-W 2007, and use the formula in Sklar-D 2004 to calculate the step length Ls, you came up with Ls about 2 meters in the 2.5 m long flume which is unrealistic. Later, they seemed to realize this and completely dropped the suspension term in Lamb, Sklar, and Dietrich 2008 (e.g. the Lamb et al's model). I am glad to see that your results in P61L1-3 supports my claim. I think it would be great if you look into details of this in your discussion. This terms does not seem right nor needed, as you can see your results in table 2 and figure 2. The SA model needs to drop this term once and for all.
- When you state/imply that the stream power type models work as well as the SA model by Sklar-D 2004 (e.g., P67L7), beware that 1) the exponents in the former have been said to be site specific or process specific, but in the SA model the exponents are fixed from physics based analysis, and 2) importantly in your study you ignore erosivity k, which is a black box in the stream power models but in Sklar-D model, it is quantifiable and derived analytically from sensible parameters (rock properties etc). Basically what you are testing here is only the exponents of each sediment effect term in your specific site of tools dominated channel. So, beware not to let this aspect escape in your summary of the results.

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Minor comments

- I think since the title is short, it should be noun instead of a sentence, i.e. "Bedload transport controls in intra-event bedrock erosion" may be better.
- P 55, L26; throughout the paper, Fe stands for "fraction of exposure" in previous works, as opposed to Fc "fraction of cover". I understand that you mean "cover effect" but beware and try not to confuse the readers. For example, in Figure 1c, you wrote "cover term"; people may misunderstand it as a coverage fraction, but in fact it is Fe, fraction of exposure where 1 means fully exposed bed. Pls check throughout the paper. You may say in p55 L26 that Fe is defined as fraction of bedrock exposure, describing the cover effect.
- Section 2; I think you may want to quickly state some basic information here about the Erlenbach Creek without demanding the readers to look at the previous papers, e.g., the slope, the channel width, D50, D90 (but I saw later that you listed D50 in section 3). A range of flow depth during the study period, if possible, should be listed somewhere.
- P58L10; "before" is a better choice of word than "until"
- P58L10; unknown; hence, only the ….. is a correct grammatical use of "hence".
- P59 L 17; discharge but a unit of velocity. Did you mean m3/s? Check somewhere else in the paper too.
- P59L18; Shields (with capital S)
- P59L19; Figure 1 therein …. Use the word "therein" to distinguish from your own figure 1.
- P59L21; were scaled to unity …. is a better choice of word.
- P59L18; the critical Shields stress here is unusually high especially on smooth bedrock surface. Lamb's formula was derived from alluvial channels for the most part. Pls discuss or recheck this why it was so high. Did you have to estimate the associated flow depth? Was it reliable data?
- P59L24; Pls add a sentence justifying why you need to show three separate simulation time periods.
- P61L4; again, see above. When you say "the cover term Fe averaged at 0.91". it sounds like the alluvial cover fraction is at 0.91; in fact, it is the bedrock exposure is at 0.91. Rephrase this.
- P61L1-3; this is great. It proves again that the suspension term proposed by Sklar-Dietrich 2004 was incorrect. It had no empirical data to support it. See main comments above.
- P61L19; It sounds odd to optimize the exponents for the SA model, which is from physics based analysis. The exponents should be fixed. Maybe mention that as well.
- P63L25; explicitly state "exponent" 3 instead of 1
- P66L26; the threshold of suspension may be only suitable for sand size (e.g. 2 mm) with high flood flow, but Lamb et al model took care of that by proposing the inadequacy of Sklar-D model (downplayed step length and thus the suspension term) but promoting "turbulence intensity" as a major role in bedrock abrasion by suspended load. Maybe you can discuss this a bit.
- P67; cover effect is more pronounced in a channel scale. So, when you test this term in your process model with concrete slab in predominantly smooth bedrock reach, it does not show much besides some patch of gravel collide sometimes resulting in slightly non

fully exposed bedrock in your fig 1c. I am not sure though if this is equivalent to alluvial cover traditionally studied in many previous works, which are more in a channel scale. In your abstract, it maybe even makes more sense to state that your study was under an artificial bedrock reach and there was no permanent static cover. Your cover was more like dynamic alluvial cover. (I see later that you said something like this in P64L10-14. Pls elaborate more.)

- P68L17; bedload dependent model
- P68L18; parameterization is misspelled
- P68L26; "in lieu of" is a better choice of word than "actually"
- P69L10; initiation is misspelled
- Table2; it is odd to see that you optimized the exponents for the SA model which is physics based as stated previously. You thus obtained unrealistic exponents of 20 and 13. The first one of 20 was because the suspension term is problematic as mentioned above. Also, the cover exponent of -1.4 for LD and AB is odd. Maybe this is because the Fe term was intent for channel-scale or static cover, but you are applying it for dynamic cover here in a process scale.
- Figure 2 and 3, y axis, "deviation from" not "deviation to"
- τ^*/τ^*c is commonly defined as "transport stage".