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In this manuscript, the author discusses the implications of the idea that the threshold of motion is an evolving function of sediment supply. This leads to a re-definition of the threshold as a state variable in analogy to thermos dynamics. The concept is interesting and provides a fascinating change of view. My major concern is that the author does not make the above-stated re-definition explicit and uses the term threshold of motion somewhat interchangeable between the new and the old version. That makes a sometimes confusing read and can be rectified by clarifying the writing and making explicit statements. Further, I think the model is insufficiently put into physical context, and the various mechanisms that can relate sediment supply to the threshold of motion are scattered amongst the different parts of the manuscript. This can be streamlined and clarified. Some further comments to this effect follow in the next few paragraphs.

Thank you for the constructive comments. Following the recommendation that the physical processes causing changes to thresholds of motion be described in more detail and combined in one place, the biggest change I made to the manuscript is moving parts of two sections that discuss previous work on evolving thresholds of motion—part of previous section 3.2 that discussed the sand dependence of reference stresses in the Wilcock and Crowe (2003) model, and also most of previous section 4.1 ("Comparison to previous work"), which discussed Recking (2012) relations—into the introduction. These are now section 1.1. In this way I have one section that better describes the many various physical controls on thresholds of motion.

I have also made the "redefinition" of thresholds more explicit, in two ways. First, I have slightly modified my notation: in the previous version I only used τ_c^* as the threshold variable. In the new version, I have added variable $\tau_{c(q_s)}^*$ to specifically indicate the new sediment flux-dependent model. In addition, I specifically describe the model as a redefinition of the concept of thresholds of motion (new lines 641, 646, 735).

Because of how Word changes line numbers in the "track changes" version of the manuscript, I note that the line numbers refer to the revised manuscript that does not show all of the edits.

The physical explanations that have been proposed for the observed dependence on the threshold mostly relate to properties that the author summarized as bed state controls. Recking

argued that the observed variability could at least partly be connected to changes in interlocking and armoring (see e.g., his figure 6), and Bunte et al. related the variability to bed stability, which is also dependent on properties such as interlocking. There are two possible explanations that are directly dependent on transport conditions: collective entrainment, in which moving particles mobilize stationary ones by knocking them out of their position. This mechanism has been advocated recently by Ancey and co-workers in a series of paper and demonstrated in 2D-experiments (e.g., Ancey et al. 2008; there are newer articles also available), but is highly debated by researchers working on 3D systems. The second one is the effect of fine material (sand) on the mobilization of gravel (e.g., Curran and Wilcock 2005). Although the latter could be argued to be a bed state control (the sand falls into pockets between gravel grains and therefore reduces roughness). I think the physical mechanisms that lead to the equations derived in the paper need to be better worked out and discussed, and the difference between bed state controls and direct controls of sediment supply need to be clarified. I am also not sure whether the equations actually differentiate between these two mechanisms.

I have worked explicit descriptions of these processes and citations into the manuscript, both in section 1.1 where previous work is reviewed, and also section 2.1 where the new model is presented conceptually.

The mechanism described by the author (during erosion, grains in pockets that are least stable move first, while during deposition grains stop in pockets that are most stable) could arguably be also classified as a bed state control, as it is depends on the availability of pockets of a certain degree of stability.

Good point. I am now more clear that my categorizations of threshold controls are not absolute, that the controls are interrelated, and that many controls could be categorized in different ways (section 1.1; new lines 84-87, 92-95 for example).

Further, the described mechanism in my mind only holds if either the supplied grain size distributions systematically change, or if deposition / erosion lead to systematic compaction or loosening of the bed. Consider a bed of a single grain size. By depositing a single grain, clearly it fills a pocket, but it also creates new pockets. It can be plausibly argued that the average state of the bed (roughness etc) does not change systematically in this way.

Finally, if the mechanism holds as described, there would be a feedback to roughness: deposition in stable pockets reduces the number of stable pockets, which means a smoother bed and higher flow velocity, which in turn makes each of the pockets less stable (similar to the effect of adding sand to a gravel bed, see Curran and Wilcock 2005). This would be a feedback limiting the variability of the threshold.

Good points. To address this, I have expanded the description of feedbacks in section 2.1 (the conceptual model). I now explicitly say in this section that there are physical limits of how much bed roughness and other controls can change (new lines 260-265). These limits were already built into the model equations before, but were previously not described well enough conceptually.

31 Please give some references for the statement here.

I added five references (new line 29-31)

48/50 Two consecutive sentences that are both starting with ‘in practice’.

Rearranged and combined sentences to remove the repetition.

55 maybe add ‘typically’ here

Done (new line 60).

57 yes, but slope is a proxy for other parameters such as roughness, rather than a direct control

I agree; this is now stated directly (new lines 63-66).

53-74 Turowski et al. 2011 demonstrate both the large temporal variability of the threshold and its control by grain and bed properties for several mountain streams. Chen and Stone 2008 explained some of the variability of measured bedload transport rates with local sub-sampling of the overall grain size distribution, leading to spatially varying thresholds of motion. This is also related to recent work on patch dynamics.

I have added description and reference to these works, and also now state that patches influencing thresholds of motion and transport (new lines 66-69, 99-101).

77 I am not sure whether I totally agree. See major comment.

I have now clarified how I categorize controls on thresholds of motion, simply for the sake of describing controls in an organized manner. I have also added a separate category of sediment flux controls (new lines 84-87, 142-170).

93 comma missing after (vertical position)
Added comma

136 Individual grains each have a different threshold…
Done (cut the word “will”)

142-143 inconsistent: does tau*_c follow a probability distribution (implying it is a random number) or is it constant?

I have clarified the relationship between distributions of threshold values for a population of grains on the bed surface, and the single threshold value that would best describe transport when applied in a bedload transport equation (new lines 229-239).

145 and following: overuse of future tense: Progressive erosion entrains… grains tend to preferentially deposit…

I have changed writing to be present tense, here and elsewhere.

147-148 This makes intuitive sense. Are there any data on this?

I wish there were, but I am unaware of data showing this. I hope to collect flume data on this in the future. I have addressed this comment by adding "I assume" to make it clear that this is an assumption of the model (new lines 244-246).

148-149 I am not entirely convinced by these arguments. It assumes that deposition systematically changes bed-averaged roughness. See major comment.

The reviewer is right, it does generally assume that bed-averaged roughness changes. I now clarify in this section that there are limits to how far thresholds of motion can evolve (new lines 260-265).

158-160 Unclear why it was necessary to make this point. Please elaborate.
I have cut this part.

207 unit missing after 4.
Added.

208 does the use of 'initial' imply here that slope was changed during the experiments?

I have rearranged text to now have the callout to Figure 2 sooner, which shows (minimal) slope evolution during the experiments. The data were included in the Figure in the previous version, but it was less clear. (new lines 319-325).

227 What does 'very low' mean here?
I have reworded the text to say that flux dropped by approximately 3 orders of magnitude, and also have a callout here to Figure 2a that shows how the transport rate changed through time. (new lines 343-344).

260 The hiding function exponent…
Added "The" as requested.

294 Which experiments? New paragraph, reference is unclear.
Edited to be clear what data is being talked about (new lines 405-407).

300-312 Curran and Wilcock 2005 should be cited somewhere here.
Added (new line 414).

304 change 'with no' to 'without'.
This wording was cut during editing.

332 Undefined abbreviation RMSD.
Done (new line 433)

352 Please give the full reference.
Done (new line 451)

462 Turowski et al. 2011 should be discussed in this chapter.

I now reference this work in multiple places in the manuscript. This particular section of text has been moved to the introduction.

489-492 So, how does the model relate to the data, then?
I have expanded this paragraph a bit to more to explain how the model can be consistent with and explains previous observations of Recking (2012) and Bunte et al. (2013), but at the same time the model does not depend directly on sediment flux, but on changes in sediment flux. (new lines 561-570).

584 There needs to be at least a brief description of Phillips' concept; it cannot be assumed that the reader is familiar with that paper.
Done, new lines 648-652.

589-605 The comparison with thermodynamics is interesting, but I wonder in how far it is novel. In the end, in river morphodynamic modelling, channels have been treated using concepts similar to state variables and state functions, they just have not been explicitly called such. Note that recently Furbish and co-workers applied concepts from statistical mechanics to bedload transport (e.g., Furbish et al. 2012, series of 4 papers in WRR and JGR).
While I thought the previous version acknowledged that similar ideas have been implicitly used, I now state this directly (new lines 647-648). I now cite the Furbish work earlier in the manuscript (new lines 217-221); it does not bear direct relevance to rate and state variables, though is excellent work applying ideas from physics.

610 This statement involves a redefinition of tau*_c, and this should be made crystal clear.
I now explicitly state that the threshold of motion is defined as a state variable (new line 641, 646, 677).

Fig. 4, caption: typo in matching, 3rd line.
Corrected.

References

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