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Earth Surface Dynamics Discussions

Interactive comment on "A bed load transport equation based on the spatial distribution of shear stress – Oak Creek revisit" *by* Angel Monsalve et al.

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This is an interested, well-conceived and executed study. The authors extend our understanding of bedload transport by developing a model that relies on local shear stress to predict local bedload transport rates, which is designed to replace models that only rely on reach averaged shear stresses.

While the manuscript is a fine contribution as is, there are some interesting observations about the nature of bedload transport and previous transport equations that the authors could emphasize more strongly. This would raise the discussion from a more technical level about how the data were collected and analyzed to include new

scientific knowledge that the author's results lead to. For example, it is perhaps not widely recognized that existing transport equations more or less require the that the entire shear stress distribution in a reach be scaled by a single parameter, the mean that is estimated by the reach averaged depth-slope product. While this assumption may be true in certain cases, for example in straight reaches with few large roughness elements, it is unlikely to be true for complex reaches with variable planforms and roughness characteristics. The author's results provide a mechanistic explanation for why bedload transport predictions using a single reach-averaged shear stress will be inaccurate for these conditions.

It is also meaningful that the author's equation is parallel to, but "lower than", previous equations. This means that most bedload transport occurs in relatively small, localized areas of the bed where shear stresses are higher than average. While not a new or surprising result, it has rarely been convincingly demonstrated by analyzes from specific field sites, or encoded in a method for computing bedload transport. This is a really interesting and important point that can be derived from the author's results. I would encourage them to make more of it in the manuscript.

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Enumerated below are some minor editorial suggestions, keyed to the text.

1. Line 95, "cobble-gravel". Please cite a source for these terms. For most geologists, "gravel" refers to particles > 2 mm, which is obviously not intended here. 2. Line 100. "riparian. . ." ZONE? Something missing in this sentence. 3. Figure 1. Please give lat-long for this reach so it can actually be found. A location map should allow readers to find the precise area. 4. Line 143 – "subsurface-based". Can you provide some explanation for choosing the sub-surface based equation? This is inconvenient because the subsurface distribution is much harder to measure, and it is philosophically confusing because the subsurface is not directly accessed by the flow. Justification? 5. Line 189 – grouped, not "group". 6. Line 193. Is the variable Fi defined for the

subsurface or the load? The two are not the same. It must be the former, because the latter is not known until qij is computed. Please clarify. 7. Table 1, mean shear stress - how does these values compare to the depth slope product? It would be interesting, even reassuring, to know. 8. Figure 3. I would like to see a map of the spatial distribution of predicted shear stress in the channel, scaled by the depth-slope product, just to be sure that the model predictions appear reasonable. I understand that this may seem unnecessary, but it would help the reader to better understand the results obtained during an important step of the computations. 9. Figure 4a: What trend would be obtained from the depth slope product and steady uniform 1-D flow? Do we actually need the 2-D model to make this correlation? 10. Figure 4b: should the right y axis label refer to the variable beta actually shown in the graph, rather than theta, which is not shown in the graph? 11. Equation 14 – should the dependent variable be beta, rather than Q, which mysteriously appears on both sides of this equation? 12. Line 260. Is a “complete shear stress distribution” the one that is predicted by the 2-D numerical model? Please be clear about this - it is an important detail. 13. Line 264. “relatively strong” - Please let the reader decide if the agreement is "strong". Just describe the correlation using the statistics. 14. Line 274 – “database”. Please explicitly note in the text that these results are presented in Fig. 6. 15. Line 280. This is useful, but a better test would be to try to reproduce data from a completely different site (of course outside the scope of this investigation – just offered as an observation).

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