Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-35-RC1, 2018

Earth Surface Dynamics Discussions

Interactive comment on "Clast imbrications in coarse-grained sediments suggest changes from upper to lower flow regime conditions" *by* Fritz Schlunegger and Philippos Garefalakis

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Received and published: 28 May 2018

Schlunegger and Garefalakis provide a nice look into the hydraulics of the imbrication fabric in modern and ancient gravel-bed river sediments. I have recommended "major revisions" for this paper because while I find it of real interest, there are some omissions and errors in the mathematics that concern me. I have tried to go through them as best I can, which has led me to believe that they are solvable, and that the authors should be encouraged to go back through their work and perform a more thorough analysis of the appropriate threshold shear stress values to use. These are noted in the following paragraph here and in the enumerated suggestions.

Two omissions by the authors, while incorrect in themselves, may at least partially offset one another. The authors consider the Shields stress on particles with a grain size of the D_{84}, without including hiding effects in their equations to compupte the critical Shields stress. This would act to reduce the critical Shields stress. However, the authors also do not consider the fact that channel-forming Shields stress must be above the threshold for initiation of motion. Parker (1978) considers this stress to be at 1.2 times ϕ in self-formed gravel-bed rivers, as is supported by Phillips and Jerolmack (2016). Pfeiffer et al. (2017) show that some rivers have a channel-forming τ_b/ϕ ratio that may be even greater. I predict that the alluvial fan examples would be closer to the Parker (1978) and Phillips and Jerolmack (2016) ratio, and that the artificially-confined rivers may exhibit higher channel-forming shear stresses. Whether these applied stress multiplication factors matter or not depends on whether it is the critical Shields stress or the channel-forming Shields stress that matters for the question at hand.

I am not sure if the end result, after applying these corrections, will be the same as the authors posit now: that is, that imbrication forms in the near-critical–supercritical transition. It may well be; I simply do not know. Therefore, I would like to ask the authors to carefully revisit the mathematics and improve their explanations. I would be happy to review a revised manuscript.

Line-by-line:

12. What does "presumably" mean here?

19. What kind of "bed roughness values" are these? Please also note units, if needed.

43. considered to record

62. justifications → justification

92. More precisely, the shear stress exerted by the fluid on the bed (shear stress is not an intrinsic property of the fluid)

93. inertial force

95. You include "x" as a subscript of D in the denominator but not in the numerator. Please be consistent. (Also, i is typically chosen for size classes, if this is the intent of including it, as it seems to be.)

96. gravitational acceleration

99-101. You are mixing the use of ϕ as the Shields stress (any applied stress, but made dimensionless by $(\rho_s - \rho)gD$) and the critical Shields stress for initiation of motion. Please clarify here; I think you want the latter definition.

103-106. I think that you will need a reference for this claim, and it may be good to discuss which grain sizes will be more likely or less likely to be entrained, as this becomes important in heterogeneous mixtures.

107-108. Lamb et al. (2008) compile the relevant data from that time.

112. 84th percentile; D_{84} is the size class at that percentile

120. Wong and Parker (2006) noted an error in M-P M's original analysis and suggest a value of 0.0495 for critical Shields stress. (In fact, they suggest two values, with the one that I am writing being for maintaining the 3/2 relationship with transport.

122. A channel-forming flood must exceed the threshold of motion, and this equation therefore cannot be correct. For many rivers, the Parker (1978) criterion of channel-forming discharge at approximately 1.2 times critical holds. See Phillips and Jerolmack (2016) and Pfeiffer et al. (2017) for a more recent discussion. This and the previous comment must be propagated through the paper.

Furthermore, the MPM relationship that you invoke here is designed for only one size class of gravel that comprises the river. This may be appropriate in some cases for the D_{50}, but does not include the extra boost of mobility given to large grains as a result of protruding from a finer-grained bed. This "hiding factor" is important. It will reduce the effective Shields coefficient (phi), and I expect that not including it will cause your Froude number estimates to be anomalously high.

Finally, you are missing a g in this equation. I have checked and you do not seem to propagate this error, so it is probably just a local typo.

126-129. Your reason for this relationship working is about the hydraulic radius, but the other important piece is the steady, uniform flow assumption.

134. 1 "s" in Weisbach

153-155. Manning's n is a function of grain size; see Gary Parker's work (Parker, 1991) or his e-book. This is also cited (perhaps more conveniently) by Wickert and Schildgen (2018, Eq. 13); you can rearrange this equation to solve for Manning's n.

178-179. Yes! At incipient motion. I suggest that you use this wording instead of "channel-forming" unless/until you are discussing floods that move significant sediment and reshape the channel.

212. calculation of (instead of "to calculate")

213-214. Do you mean that backwater effects become important?

224-226. Is this a qualitative description of the hiding factor? If so, it would be nice to see estimates better quantified, as the Froude number of the depositional conditions is key to your conclusions

230. It could be good to note that your "roughness" is Darcy-Weisbach friction factor, to be unambiguous.

238-241. This may be true, but I am calling this into question on the basis of your using the D_{84} without a hiding factor (see above comment).

242-250. See Lamb (2008) and update this paragraph; I do not think the Shields parameter increase will be as extreme as the Mueller study alone shows.

263. Artificial river banks can fundamentally alter the flow hydraulics and the self-regulation of channel width. This artificial narrowing can increase flow velocities and

alter the Froude number. Do you know that your knowledge of the hydrograph, the bed shear stress, and the age of the imbrications are all consistent with being from either before or after the modifications were made?

320-321. I do not see how a floodplain would confine a gravel-bed river, especially on an aggrading alluvial fan. Could you please explain or change this statement?

349. A general comment on the data section: your focus in the writing is more on the non-imbricated sediments in the geological record and the imbricated sediments in the modern rivers. I think it is important to make clear to to the readers that you have both conditions from both environments at the very start.

<I have stopped making English usage corrections at this point. Several more minor errors follow, but the English is overall quite good.>

432-434. These are the forces driving particle motion, but weight also operates on the particle.

439. Could you use the long axes of the particle in this equation as the lever arm? You have measured them, it appears.

467. Are flow velocities really higher on steeper slopes? Or do roughness and shallower overall flow decrease the velocity proportionately?

471. My reading of the Lamb et al. (2008) study was that it included a significant data-driven component, which has a large compilation; my impression is that you are not taking into account this compilation and instead prefer the field measurements from Mueller (2005). This choice needs justification.

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Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2018-35, 2018.