Our response to the comments and suggestions by reviewer A. Wickert

Our response to the general comments

We are grateful for this very constructive review by A. Wickert as it allows us to clarify two major concerns, which we mainly base on the limited explanations from our side. As a first point, the reviewer outlines to the differences between threshold flow strengths that are required to either (i) mobilize individual clasts on a gravel bar (incipient motion of sediment particles), or to (ii) modify the shape of a channel (channel-forming process). The reviewer correctly mentions that channel forming Shields stresses are up to 1.2 times larger than Shields stresses at the incipient motion of individual clasts, and that this aspects warrants a careful consideration. We agree on this and address this point in the section where we outline the methodological approach, and in a separate section where we discuss the consequences. In particular, the relationships that we denote in equation (1a) have been proposed for floods magnitudes and related shear stresses at the incipient motion of individual clasts (Shields, 1936; Paola et al., 1992; Paola and Mohring, 1996; Tucker and Slingerland, 1997).

$$\phi = \frac{\tau_{cDi}}{(\rho_s - \rho) g D_i} \qquad (1a)$$

Here, τ_{cDi} denotes the critical shear stress, or alternatively the Shields stress at the incipient motion of individual clasts with the grain size D_i. The constants ρ_s and ρ denote the sediment and water densities, and g is the gravitational acceleration. The shear stresses, however, substantially differ for the case of channel forming floods, as correctly noted by the reviewer. At these conditions, channel forming Shields stresses $\tau_{channel}$ are up to 1.2 times (Parker, 1978) above the threshold for the initiation of motion τ_{cDi} as mentioned by A. Wickert in his review. Pfeiffer et al. (2017) additionally showed that some rivers have a $\tau_{channel}/\tau_{cDi}$. ratio that is even higher. The consideration of channel forming floods thus requires larger thresholds and thus a modification of equation (1a), which then takes the form expressed by equation (1b), as noted by A. Wickert:

$$\phi' = \frac{\tau_{channel}}{(\rho_s - \rho) g D_i} \approx 1.2 \frac{\tau_{cDi}}{(\rho_s - \rho) g D_i} = 1.2\phi \qquad (1b).$$

The results of the calculations then show that the Froude numbers critically depend on the selection of the threshold conditions for the transport and evacuation of sediment. We have considered this aspect through the assignment of different ϕ-values, which in our case vary between 0.03 and 0.06 and also include an upper bound of 0.1. If we apply 1.2 times larger Shields stresses for channel forming floods, as theoretical and field-based analyses and have shown (Parker, 1978; Philips and Jerolmack, 20916; Pfeiffer et al., 2017), the consequences on the outcome of our calculations are minor. In fact, a 1.2-times larger threshold will increase the ϕ–values (equation 1b) to the range between 0.036 and 0.072. Recalculations show that this will not change the general statement that water flow may shift to upper flow regime conditions for streams where channel gradients are steeper than ~0.5°±0.1°, and where the relative bed roughness exceeds a value of ~0.06±0.01. In addition, because imbrications mainly form as pivoting clasts come to a rest behind a large and stable constituent, our selection of the critical Shields stresses for the incipient motion of sediment particles rather channel-forming Shield stresses is likely to be valid. Indeed, while channel forming floods are mainly associated with equal mobility of sediment particles within a gravel bar, the formation of an imbricated fabric involves the clustering of individual clasts only.

The second point addresses possible protrusion effects that need to be considered for estimates of critical Shields stresses upon the entrainment of D_{84} and larger clasts. Indeed, as imbrications mainly involve the largest clasts of a gravel bar, possible protrusion effects could influence the outcome of our calculations, which thus warrants a careful consideration, and which we present in the revised manuscript. In particular, it has been proposed that the entrainment of the largest clasts most likely require lower flow strengths than the shift of median-sized sediment particles, particularly in cases where the material sorting is poor. Related ϕ–values may be as low as 0.03 as proposed by Lenzi et al. (2006) and van der Berg and Schlunegger (2012) for mountainous streams where the sorting of the material is poor and where the packing of large clasts is low. Our calculations predict that an upper flow regime is very unlikely to establish at these conditions (Figure 3 of the manuscript). However, we consider it unlikely that the formation of imbrications with steep dip angles, as we did encounter in the field, were associated with low thresholds. We base our inference on the observation that imbricated clasts in general, and the analyzed gravel bars in particular, form a well-sorted arrangement of large particles, which form a densely packed clast-supported fabric. This results in a high interlocking degree of sediment particles within these bars, which in turn requires that large threshold conditions need to be exceeded to shift the material. We thus propose that the use of ϕ–values of at least 0.0495, which is commonly applied for the entrainment of the D_{50}, is adequate for the calculation of the hydrological conditions associated with the fabric as we have encountered in our examples.

Point-point response to specific questions

12. What does "presumably" mean here?
We replace this by 'one of the most'

19. What kind of "bed roughness values" are these? Please also note units, if needed.
This is a dimensionless unit, which we define in order to avoid confusions

43. considered to record
Corrected

62. justifications → justification
Corrected

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

93. inertial force
Corrected

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.)
Corrected and adjusted

96. gravitational acceleration
Corrected

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.
Clarified and Corrected

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.

We add a reference to Hattingh and Illengerger (1995) in the revised manuscript and discuss the issue related to protrusion effects.

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

The related reference is added

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

Corrected

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.

We are grateful for this comment and correct the text and the calculations accordingly. We do not fully understand the point regarding the 3/2 relationship and thus recalculate the Froude numbers for ϕ=0.0495.

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.

We address this aspect (differences between the Shields stresses for channel forming floods and for the incipient motion of individual clasts) in the revised manuscript. Please see our response above.

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.

This comment also addresses the major concerns formulated at the beginning of this review (hiding and protrusion effects). Please see our response above. We greatly appreciate the detection of the typo (missing of a 'g'), which we have corrected.

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

Yes indeed; we refer to this important boundary condition.

134. 1 "s" in Weisbach

Corrected

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.

Yes indeed; the results are consistent with our approximation and we have thus referred to the Wickert and Schildgen (2018) in this context. However, in the framework of Jarrett's (1984) work, our statements are still correct.

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.

We proceed as suggested as it clarifies the situation

212. calculation of (instead of “to calculate”)

Corrected

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

According to Whipple (2004), whom we cite in this context, step-pool channels do have distinct steps in their thalwegs and may have frequent chute/pothole pairs. This is markedly different to the channels with longitudinal and transverse bars as described here. Therefore, we see it unlikely that step-pool channels can be considered in the context of our paper. This is the major reason why we do not consider channels with a gradient above 1.2°, when step-pool channels are likely to be found.

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.

This addresses the second major point of this review (hiding and protrusion effects). Please see above of how we have handled the related concern.

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

This refers to the relative bed roughness that we define in the abstract of the revised manuscript. We clarify this issue in the figure caption.

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).

Indeed. Please see above of how we handle this point in our manuscript (hiding and protrusion effects).

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.

We agree, but nevertheless we refer to Mueller’s (2005) analysis for the sake of completeness, but we do place the major outcome of Lamb’s et al. (2008) work in this context.

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?

The parameters and the imbrications were all from the time after the modifications have been made. Therefore, our considerations are internally consistent regarding the timing of the anthropogenic corrections and the streams’ responses. We mention this in the revised manuscript.

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?

Corrected.

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 the readers that you have both conditions from both environments at the very start.

We greatly appreciate this supportive comment and make a related statement in the revised text.

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

We greatly acknowledge the time the reviewer has invested in this.

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

Yes indeed. This is also the reason why we write: An individual grain then begins to move if the resulting fluid force F_{fluid} exceeds the submerged weight F_g of the sediment particle with grain size D.

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

We rather prefer to focus on the b-axis, first of all for consistency purposes, and second mainly because the related hydrological equations have been calibrated with b-axis datasets.

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

A larger roughness, which is likely to be associated with steeper slopes, can indeed reduce the flow velocity. We thus removed the reference to flow velocities.

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.

Yes indeed. We correct this and add the following statement: This might be an overestimate of the ϕ=dependency of slope (Lamb et al., 2008), but it does show that ϕ-values larger than the commonly used ϕ=0.0495 might be appropriate where channels are steep.

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