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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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We consider the review by R. Hodges as detailed, specific, constructive and thus very helpful, and we will bring up the related points with much appreciation in our revised manuscript. In particular, R. Hodges raises three major points, which we address in this reply and which we fully consider upon revising our paper: (i) mismatch between experimental results and our inferences, (ii) effects related to the protrusion of large clasts (please see also related comments by reviewer A. Wickert, and (iii) the way of how we have treated the translation of clasts at the incipient motion (rolling versus sliding).

Mismatch between experimental results and our inferences

R. Hodges and P. Carling correctly mention that results of flume experiments do show that clast imbrication can form when individual clasts slide upon each other (Carling et al., 1992), and when flows are subcritical (Powell et al., 2016; Burtin and Friedrich, 2018) with Froude numbers between c. 0.55 and 0.9. Also based on flume experiments Powell et al. (2016) additionally showed that the material can be entrained with φ-values as low as 0.03. R. Hodges and P. Carling mention other articles, where this point has been made, which we summarize below. For instance, based on field observations, Sengupta (1966) reported examples where imbrication was most likely initiated by the development of current crescents around pebbles that were embedded in sand, and that these processes possibly occurred during lower regime flows. Also during experiments, Johansson (1963) reported particle vibration before entrainment either through rolling or sliding. He noted that imbrication was formed at conditions, which corresponded to the lower flow regime during the flume experiments. Carling et al. (1992) additionally showed that the shape of a clast has a strong control on the thresholds for incipient motion, the style of motion, and the degree of imbrication. We summarize these findings in our revised article as these are valuable points. We then carefully examined the photos, which illustrate the experimental set up of some flume experiments. However, inspections of photos illustrating the experimental set up reveal that the surface grains are either flat lying on finer-grained sediments before their entrainment (Figure 3 in Powell et al., 2016), occur isolated on the ground (Figure 2.1b in Carling et al., 1992), or have a low degree of interlocking (Figure 3a in Lamb et al., 2017). Interestingly, the experiment by Buffington et al. (1992) followed a different strategy, where a natural bed-surface of a stream was peeled off with epoxy. They subsequently used this peel in the laboratory to approximate a natural channel bed surface (see their Figure 4), on top of which they randomly placed grains with a known size distribution. Buffington and co-authors then measured the friction angle of the overlying grains, based on which they calculated the critical boundary shear stress values φ. In all experiments, the surface morphology of the sedimentary material is flat and lacks topographic variations, which we found as reach-scale alternations of riffles, transverse bars and pools in the field. The low $\varphi-$values of 0.03, which appears to be typical of bed surface conditions that develop in laboratory flumes (Ferguson, 2012), as summarized by Powell et al. (2016), could possibly be explained by these limitations. Furthermore, and probably more relevant, the lengths of the experimental reaches are generally less and range e.g., between 4.4 meters (Powell et al., 2016) and 15 meters (e.g., Lamb et al., 2017). We acknowledge that the results of most experiments have been normalized through constant Reynolds numbers. This normalization also includes the experimental D50-grain sizes, which are similar to those we determined for our selected streams. Nevertheless, we find it really hard to upscale some of the results associated with these experiments to our natural cases where standing waves of 1 m, and even between 5 and 8 meters length may occur (our Figures 1B, 5B, 6B), which are not reproducible in the experiments. In addition, Powell et al. (2016) observed that the water surface stayed relatively stable during their experiments, and that the flows were steady and uniform without hydraulic jumps. This contrasts to our natural cases where upper and lower flow regimes alternate over short distances even during low-stage discharge. Accordingly, while low φ-values thus a lower flow regime might be appropriate for predicting the entrainment of the sediment particles in flume experiments, larger thresholds and thus larger φ-values are likely to be appropriate for our natural examples. We support this statement by our own observations where the large clasts in subaerial (during low water stages) and subaquatic riffles and transverse bars are closely clustered, and where most of them are embedded by fine-grained material. This requires that the some of the matrix has to be removed before the larger clasts can be dislocated, which actually refers to the hiding effect with large thresholds (see response below). In addition, high discharge flows at e.g. the Waldemme Emme River (slope >0.5°; imbrication is observed) is associated with evidence for standing waves and thus upper flow regime conditions, while high discharge flows of the Emme River (slope c. 0.4°, no evidence for imbrication) is not (Spreafico et al.; 2001; p. 53). We also support our statement by the field-based analysis of observations reported

by Mueller et al. (2005) based on bed load rating curves. We discuss this point in a separate section of our revised manuscript. We fully discuss these aspects in in a new section (lines 583-636).

Protrusion effects

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. We fully acknowledge in the revised manuscript that these relationships have been explored in flume experiments (e.g., Kirchner et al., 1990; Buffington et al., 1992) and through field-based studies complemented with experiments in the laboratory (e.g.; Johnston et al., 1998). These studies resulted in the notion that the entrainment of the largest clasts most likely requires lower flow strengths than the shift of median-sized sediment particles. As a consequence, conditions for the incipient dislocation of these clasts could be predicted with φ–values that might be as low as 0.03 or even less (Buffington et al., 1992; Johnston et al., 1998). Related φ–values have, for instance, been applied for mountainous streams where the supply of sediment from the lateral hillslopes has been large (van der Berg and Schlunegger, 2012). Our calculations predict that an upper flow regime doesn't establish at these conditions (φ–value of 0.03). However, we consider it unlikely that the formation of the imbrications, as we did encounter in the analyzed Alpine streams and in the stratigraphic record, were associated with thresholds as low as these. We base our inference on the observation that the analyzed gravel bars form a well-sorted arrangement of densely packed and clast-supported fabric of large particles. In addition, in our case, the base of most of the large clasts are partly buried within a matrix of fine-grained material, which is not the case in flume experiments as the inspection of Figure 2.1b in Carling et al. (1992) and Figure 3s in Lamb et al. (2017), shows. This results in a high interlocking degree of sediment particles within all bars we have encountered in the field where imbrication occurs. As a consequence, a dislocation of any of these clasts and thus a rearrangement of this fabric most likely require that the finer-grained material has to be removed first to dislodge the clast (hiding effect). The consequence is that larger thresholds have to be exceeded, which is mainly accomplished through high-discharge events with large flow strengths (Mueller et al., 2005). We thus propose that the use of φ–values of c. 0.05, which is commonly used for the entrainment of the D50 (Paola and Mohring, 1996) may be adequate for the calculation of the hydrological conditions associated with the fabric we have encountered in our examples. Other arguments for our preferred selection of φ–values are outlined in the section above. We fully discuss these aspects in a new section (lines 499-543).

Rolling verus sliding

We realize that our section on rolling versus sliding and as a consequence larger versus smaller thresholds has not been based on sufficiently solid grounds. We will thus remove the related section from our paper.

Unfortunately, we lack the quantitative data to properly address the dip angles of imbrications and the density of imbrication. We thus tuned down related statements as some of them are not fully supported by the evidence available to us.

Point-by-point response to questions and concerns raised by the reviewer:

Reviewer's comment:

Having read the previous two reviews, I agree with the points that they raise. I've also looked at the authors' responses. However, I'm still unconvinced by the argument that imbricated fabrics only form under super-critical flows, and less convinced that strong imbrication will only occur at the specific location of the transition between sub- and super-critical regimes. I agree with Carling that it is not clear from the paper whether you are claiming that imbrication occurs when Fr > 1, or only at the locations where flow is transitioning at a hydraulic jump. If it is the latter case, then how do you reconcile the widespread occurrence of imbrication across bars with the limited spatial extent of hydraulic jumps? Could you predict the spatial occurrence of hydraulic jumps and see

whether that matched the spatial occurrence of imbrication?

Our response:

We suggest that imbrication occurs when Fr > 1. Clusters of imbricated clasts might then result in hydraulic jumps, as is particularly observed during low water stages. We have clarified this issue in the revised version of our manuscript.

Reviewer's comment:

There are some flume studies that are relevant to your work which demonstrate imbricated fabrics forming in subcritical flows. Burtin and Friedrich (2018) demonstrate imbrication in flows with Fr = 0.54 and 0.55 (calculated from their Table 2). Powell et al (2016) demonstrate imbrication in flows with Fr $\approx$0.60 to $\approx$0.94, with the amount of imbrication not varying with Fr. (Fr is calculated using their stated slopes, depth and roughness ratio, and your equations 6 and 8). Are these data consistent with your argument?

Our response:

We have discussed this issue in a separate section in the discussion (new section labelled The formation of imbrications in experiments). Please see our explanations above.

Reviewer's comment:

I think that Figure 3 could be clearer, and is potentially misleading. Panels A/B and C/D show different things; Fr values in A/B and imbrication in C/D. By using the same colour scheme across all panels you are equating imbrication with Fr > 1, but it's hard to tell whether the data support this. I can see that as slope increases, Fr is likely to be > 1 and more imbrication is observed. The pattern with bed roughness is less clear. In B Fr > 1 is most likely at intermediate roughness, however the imbrication all occurs at high roughness. The sites with no imbrication occur at the sort of roughness values that correspond to the highest Fr values; therefore the two patterns don't look similar to me. Why not calculate the Fr values for entrainment of D84 in the field and rock deposits, and see whether you get a consistent pattern between the Fr value and whether imbrication is observed?

Our response:

We have separated the two figures and changed the colours in order to avoid this confusion. We have not specifically calculated the F-values for the field data mainly because this will heavily depend on channel slope and the bed roughness values, and particularly on the φ-values. However, we modified the discussion to make the linkage between field-based observations and modelling results more transparent and clearer. Please see lines 640-692 of our revised manuscript.

Reviewer's comment:

I would have liked to see some attempt to quantify the amount of imbrication that is observed in the field and rocks. In your response to Carling you refer to shallow and strongly dipping grains, and suggest that the former might form under sub-critical flows. If this is the case, then your argument is not as simple as imbrication equals super-critical flows. You would need a more robust method to quantify the amount of imbrication, and a dataset to determine the relationship between imbrication amount and flow regime.

Our response:

Unfortunately, we don't have quantitative data to properly constrain these observations. We therefore frame our hypothesis around other arguments. We provide evidence from the field, documenting that sites with imbrications are also the reaches where we observed supercritical flows during high stages. Please see lines 665-677 of the revised manuscript.

Reviewer's comment:

As with Carling, I'm also unconvinced by the argument that grain rolling is necessary for imbrication to occur. I would have thought it possible for a grain to be entrained by sliding, and to slide or flip into an imbricated position on deposition. There is also evidence that beds can undergo some restructuring at sub-critical flows, which has potential to include imbrication.

Our response:

We agree and we have removed this section.

Reviewer's comment:

19: I'm not convinced that this description of a threshold is consistent with Fig 3 and later parts of the paper, in which you describe Fr values decreasing again at high slopes and roughness values.

Our response:

Our results reveal that imbrications possibly record supercritical flows provided that (i) $\varphi-$values are larger than c. 0.05, which might be appropriate for streams in the Swiss Alps; (ii) average stream gradients exceed c. 0.5±0.1°; and that (iii) relative bed roughness values, i.e. the ratio between the water depth d and the D84, are larger than âĹij0.06±0.01. We have clarified these points in the revised manuscript.

Reviewer's comment:

119: I agree with Wickert that you need to consider hiding effects. The stated Shield's criterion values of 0.03 to 0.06 normally refer to D50, and in the case of hiding effects (i.e. in most gravel beds) then the Shields value of D84 would be less than for D50. In your response you argue that imbricated grains would be harder to move, and therefore a higher value is appropriate; however, if you are considering how grains become imbricated from a non-imbricated bed, then you don't need to make this adjustment. It's important to address this issue, because the dimensionless critical shear stress that you use affects whether you reach super-critical flows in Fig 3. If a value less than 0.047 is most appropriate, then it doesn't support your argument about the importance of super-critical flows.

Our response:

We agree that hiding and protrusion effects are relevant, and that these have a measurable influence on the φ-values and thus on the outcome of our calculations. We discussed the related effects in a separate chapter. We actually find that because most of the largest clasts are either embedded in finer-grained particles, or form well-sorted and densely packed clusters, the finer grained material has to be removed before the largest grains can be entrained. This actually calls for the consideration of hiding effects with larger thresholds. We have discussed this point in full detail in lines 499-543.

Reviewer's comment:

122: Don't include 0.047 in eq. 3; use φ instead as this is consistent with what you show later on when this equation gets combined with others in equations 9 and 10.

Our response:

This has been done.

Reviewer's comment:

225: You do refer here to the idea of sorting, and therefore hiding, effects affecting the value of φ, but this would be better explained earlier on when you are considering the appropriate value of φ.

Our response:

This has been done.

Reviewer's comment:

305: I assume that you are looking at exposures that are parallel to the flow direction, but you don't state whether this is the case. The amount of imbrication that you observe is likely to be affected by the direction of the exposure with respect to the flow direction.

Our response:

We are looking parallel to the flow direction. We have clarified this point.

Reviewer's comments:

350: It might be useful to have a summary of which exposures shows imbrication and which didn't.

Our response:

The sites/sections which bedrock with or without imbrications are shown on e.g., Figure 2 in Garefalakis and Schlunegger (2018) and in Schlunegger and Norton (2015). We refer to these articles where sites of the sections including the corresponding units are shown and illustrated.

Reviewer's comments:

373: It's not obvious to me how eq. 1 explains the decrease in Fr at high slopes and high roughness. This could be more clearly explained. See Lamb et al. (2017) for analysis of the relationships between flow resistance, flow depth and slope.

Our response:

We acknowledge that we have not correctly interpreted these trends. Indeed, the tendency towards lower Froude numbers for a channel gradient >1° (φ >0.05) and a bed roughness >0.3 (φ >0.05) is somewhat unexpected. We explain these trends through the non-linear relationships between slope, water depth, the energy loss within the roughness-layer, and the velocity at the flow's surface.

Reviewer's comments:

423: Changes with slope depend on whether flow depth and hence relative roughness also changes.

Our response: Indeed. However, we have removed this entire section, because the following statements about rolling/sliding have not convinced P. Carling either.

Reviewer's comment:

449: I don't follow the argument here. I think that you're arguing that because of the pivot angle, then φ should be greater than the typical 0.03 to 0.06? You don't need imbrication to get pivot angles greater than 5 to 10âŮę though. Most gravel grains have higher pivot angles; see Kirchner et al. (1990), Buffington et al. (1992) and Johnston et al. (1998) among others.

Our response:

We have removed this entire section as none of the reviewers has been fully convinced by this.

Reviewer's comment:

583: Where or how are the data available?

Our response:

Actually, the grain size data and other material (Table 1) we have used have already been published, and all new material we have used are presented this article. This means that this particular statement becomes obsolete, so we have removed it.

Thank you very much for this careful review.

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