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Interactive comment on "Inertial drag and lift forces for coarse grains on rough alluvial beds" *by* Georgios Maniatis et al.

Georgios Maniatis et al.

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Comment: Even though the authors discuss in section 2 and 3 the frame conversion and its application (only for the acceleration data), they have not discussed its application in changing the critical force based on the particle's ordination as they claim to be doing above. At the same time, as is clear from their text and figure 2, FDcr and FLcr are kept fixed and unchanged, regardless of the particle's orientation, which invalidates their above claim (simply, the critical forces are not shown to be transformed into frame r as the authors claim above - this is also clearly shown in figure 2).

Reply: - As previously stated, we analyse all the forces in a fixed frame the axes of which coincide with the approximate parallel (x-y) and normal (z) direction of the flume

or the riverbed.

- The critical condition for the linear motion of the particle is given by the sum of the resultant forces acting on the centre of the particle being equal to (ΣF=0). For a particle highly exposed to the flow, this condition is satisfied by the hydrodynamic forces (FDr, FLr in the paper) being equal to the gravity forces (equation 9 and 10, see also responses to Reviewers 2 and 3 for differences with the rolling threshold and why it is not discussed in this work)

- Gravity has a fixed direction perpendicular to the stream bed. When gravity is analysed in a fixed frame (like the frame r in the presented work), its components are unvarying. Hence the magnitudes of the components of the fluctuating hydraulic forces when projected in the same static frame (again frame r in this work) should match the magnitudes of the gravitational components to satisfy the critical condition. The rotation of the gravitational component to the r frame is given by equation 7 in our paper.

- The components of gravity are not fixed when a mobile frame of reference is used (e.g. Valyrakis et al., 2010). For a frame of reference that is fixed to the centre of the particle (and rotates with it, the body frame in the paper under review), the effect of gravity along the direction of the resultant force will vary. This is not because the weight of the particle changes, but because the reference system moves and the resisting components of gravity depend on the orientation of the particle (the pivoting angle (θÎ£) in Valyrakis et al., 2010).

- we monitor the orientation of the particle constantly and project the components of the resultant force (resolved into FD and FL) to the static r frame. Local microtopography will affect the resultant force, but we have no way to decouple that effect from the other applied forces. At the same time, we know that the critical condition (ΣF>=0), does not characterise sediment transport and the resultant force has to be sustained for a certain period over to dislodge the particle and/ or sustain its motion. We propose that the resultant force will need to be sustained at least above the critical level for the hydrodynamic forcing (FDcr, FLcr) which is overall the critical condition for the motion of the particle (ΣF=0) and we calculate the impulses above that level.

Comment These comments are not contradictory rather they are intended to promote clarity for the presentation of the author's intended contribution. The authors here have significant fallacy in both their understanding of the framework they are presenting and their calculations which is at the crux of their analysis - which is best demonstrated in reference to figure 2-a: The FLr and FDr derived from the accelerometer's data refer to the total forces acting on the particle so the thresholding with and assumed (fixed) critical force is meaningless because the resultant force is the vector sum of the driving hydrodynamic forces (which are here unknown) and critical (resistance) forces which are also unknown (and both are wildly fluctuating) during transport. Simply, this type of thresholding has a questionable value (or relevance) to flow induced transport processes of solids. Even if (this is just a gross mistake and) the author intends to remove the thresholding, the physical relevance of the inertial impulses within the context of sediment transport or incipient entrainment is completely missing and would need be discussed.

Reply:These points are mainly addressed in the reply to the three Reviewers and the first reply here. In addition, it is necessary to note that FLr and FDr are not derived from the accelerometer data and they are not the total forces (denoted FD and FL in the manuscript under review).

Comment: Are the experiments shown herein the same or different to Maniatis 2017? The test bed around which the particle is positioned is not described in any detail: this is crucially important as it interrelates to the particle's transport once entrainment has initiated. For example, if the raised bed has limited length (<2m), to which the author refer to as the minimum transport distance, the entrainment processes described herein are more relevant to a particle falling from the raised bed rather than being transported over plain bedrock, as described in the manuscript. Also, the presence of rough or smooth bed upstream of the significantly raised microtopography would in-

volve the generation of statistically different flow structures compared to those acting on the particle for its transport, which renders these experiments not relevant to the body of work found in the traditional turbulence induced particle incipient entrainment literature, commonly referenced in this manuscript.

Reply: All three Reviewers have asked for more details regarding the flume setting (which was designed to address the case of D/depth close to 1). We will address this in a revised version. As for the characterisation of the flow; we do not measure the flow, we only measure resultant forces. Those forces are characteristic of the setting we tested, and more experiments need to be done in order to generalise to other settings. The intended contribution of this paper is introducing a framework that can be used for this application.

Comment: Still the author doesn't for some reason offer the flow depth at the critical flow conditions. (Just to clarify that the comment offered above, inquiries about the range of flows assessed at the lab, which indeed have been tested, as the authors comments- via implementing a rising hydrograph- so it is not clear why the author disagrees). Also, the authors in their manuscript describe 10 out of 12 measurements mentioned above, what were the reasons to discard two of the measurements? Are the authors showing the (eg aggregate) results for 10, 12 or just one of the experiments? Again, the experiments described in this manuscript are not relevant to the typical incipient motion literature, as the author agrees with the previous reviewer's comment: these are more relevant to boulder transport processes, rather turbulence induced transport of coarse particles (as a reader might wrongly infer by just reading the article's title).

Reply: The flow depth at mean critical discharge was 0.10 m and we are going to provide the exact critical depths for the sphere and the ellipsoid in the revised manuscript. 12 experiments were presented in Maniatis' PhD, 10 of which are used in the manuscript. The initial orientation was not measured to sufficient accuracy in the first two runs, and so they are not used in this paper. This does not affect the calculations in his thesis but could affect the results in the manuscript. No aggregate results are shown in this manuscript.

Comment: Could the author detail how the slope of the flume was measured? (if the experiments were conducted at the 0.9 m wide flume of the University of Glasgow, which I am also using, the maximum bed surface of the flume, which I have measured, cannot reach the mentioned slope of 0.02 as claimed (!) (line 230).

Reply: The slope was directly measured using a surveying level, and the measurements were repeated and validated by other users. Note that the flume has been moved to a new location since these experiments were conducted.

Comment: the authors have misunderstood my commentary. I am not discussing whether their method can be applied to different orientations (which could be done using quaternions or Eulerian angles. etc), they simply do not discuss the dependency of the initial orientation of ellipsoid on the resisting forces (and subsequently the derived impulses). It will also be of interest to elaborate if the application in the field is intended with a given initial orientation or any will do and why? Also, the authors choose to use a very expensive accelerometer of up to 400g, while only recording at a frequency of 50Hz, can the authors discuss the utility of such a design choice (why not use a smaller range for the accelerometer which has less cost?). Also, what is the advantage of recording at such a high acceleration range over such a relatively low frequency (one would ex- pect the high range of acceleration recordings -if indeed needed- to be matched by a high frequency of recordings too (eg 500 Hz or more)?

Reply: The orientation of the ellipsoid affects the application of the surface forces and the rolling mode of transport, but we don't discuss this here (see comments from Reviewers 2 and 3 and our responses). Moreover, we measure the tangential forces (responsible for the turning moments around the centre mass of the particle for the fixed axes representation we use here) to be negligible in comparison to the linear forces (and we demonstrate that in a revised manuscript). For the second issue, high range

does not automatically justify a high sampling rate. We chose the 50Hz frequency after back calculating from previous grain displacement measurements [Drake et al., 1988]. Higher frequencies are more suitable for capturing accurately particle-particle interactions and impacts which is not discussed in this work. High range is necessary because if smaller range sensors are used in natural conditions they are going to be saturated by particle interactions and impacts (even if the focus is not on those) and the data will be either lost or unusable.

Comment: It is appreciated this is not easy to do, but the resisting forces during incipient entrainment as well as transport are significantly dependant on the initial resting micro- topography and bed topography over which the particle will be advected. Also, FDcr and FLcr in the beginning and during transport will depend on these features, which make them important to be detailed. For example, was the particle initially positioned at a plane bedrock or a riffle-pool topography (which part specifically)? Is the transport taking place at the flat bedrock (or pool?) or over the drop from the riffle? In this case, what is the relevance of the inertial impulses to the plane of the bedrock slope and bed shear stresses which the authors claim above to be relevant to the transport processes?

Reply: All those questions are asking about the dependency of critical forces on microtopography, which is covered in previous replies here. In the field, grain translation was initiated over plain bedrock.

Comment: It is not clear if the field test the authors discuss truly refer to incipient motion conditions or full transport processes; for example, for how long was the particle immobile under the free flow, before being entrained?

Reply: We refer to both incipient motion and full transport. The manuscript clearly states that the particle was released on an area of exposed bedrock and was immediately transported in Erlenbach.

Comment: Is the framework the authors intend discussing refer to incipient entrainment (described in the lab) or transport processes (apparently relevant to the field work).

Reply: The framework is applicable to both incipient entrainment and transport.

Comment: Given that the resistance (highly dependent on the resting microtopography) in the lab experiments and the field tests is different (or not measured exactly in the field), then what is the value of normalising the inertial impulses from the field with the impulses from the lab?

Reply: For the relationship between resistance and microtopography; refer to replies above and responses to Reviewers. The impulses from the field are not normalised by the lab impulses; they are normalised by the mean Impulse in the field. We will clarify the wording in the revised paper.

Comment: Also, for the lab experiments: was the particle stopped because the flow was not able to push it further or it is stopped because it reached near or at the tailgate? To that goal, can you comment on the distance of the test section from the tailgate (ie how much bigger than 2 plus meters is it?).

Reply: There were some experiments where the sensor moved up to end of the flume and some experiments (for example, as in Figure 2 where the sensor stopped within the section with hemispherical roughness elements). In none of the experiments did the sensors hit the tailgate. We will summarise additional details of the experiments (from Maniatis 2016) in the revised manuscript (covering also the requests from the 1st reviewer).

Comment: Usually experiments are used for helping calibrate conditions in the field: what is the practical utility of the authors' lab experiments? What is learnt from this sensor in a quantifiable manner? And how does it physically relate to the processes and past literature of sediment transport?

Reply: It was not a goal of the study to use the flume experiments to "calibrate" field conditions. Rather, the flume and field experiments help to illustrate the particle be-

haviour under different "boundary" conditions. The other elements of the comment are addressed in the discussion and in the revised version of the manuscript.

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