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Earth Surface Dynamics Discussions

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

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Many thanks Maniatis for the response. Apologies for late reply due to being busy with other involvements. Please find my reply below to your response marked as reply.

More specifically Comment 1: The resisting forces (FDcr and FLcr) are not fixed nor are equal to the initial resisting force (which can vary significantly even for small changes in particle orientation) while the particle is transported (as has even been shown for the case of incipient entrainment) [1].

Response: That is true, the forces acting on the particle are neither fixed nor equal to the initial position. However, after calculating successive orientations (using the angular velocities) they can be transformed to a static frame (frame r in the paper) and

this is what we present (sections 2 and 3 of the paper).

Reply: 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).

Comment 2: Lines 210-215: The authors' claim that impulses can be calculated directly from particle's motion (sensor's readings) is not valid, as according to Valyrakis et al. [3] and Celik et al. [4] the flow impulses (or energetic flow events) impart momentum (or energy respectively) for a particle's motion at a certain efficiency (depending on the characteristics of the flow structure driving the particle's motion). Thus, the impulses the authors refer to are not flow impulses according to the theories being cited [5,6,2]. Response: The commenter's conclusion here is correct. This paper doesn't quantify the same impulses to the works he cites (Lines 210 to 216 of the paper).

Comment 3: It would be interesting to have flow hydrodynamic measurements so as to enable comparison of the inertial impulses the authors estimate with flow impulses.

Response: Here it is clear to the commenter that inertial impulses are different to the flow impulses. This contradicts significantly his comments above. However, it is true that this link is important. I am looking forward to reading the commenter's contribution from the experiments he conducts.

Reply to comments 2/3 above: 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.

Comment 4: Details around the flow conditions in the controlled flume experiments are missing. In particular: The flow seems to be non-uniform because of the locally raised bed where the particle rests and also the presence of a smooth bed upstream this section combined with the short length of the raised bed, render the flow not fully developed.

Response: There are hydraulic measurements presented in the appendices. We didn't have the capacity for detailed flow measurements, but that there are no physics to suggest that this affects the accelerometer model we present and the measurements for the conditions we captured. Here it is useful to look at the comments from Reviewer 1 who mentions explicitly that the conditions are closer to boulder motion rather than gravel. This is a very useful observation which doesn't affect the calculations but their interpretation. And I agree that more work is needed on that front and repetition of the experiments under varied conditions.

Reply: line 254: 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 involve 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.

Comment 5: The flow depth and the range of flow conditions tested are not mentioned; this is even more important if the flow depth is of the same order of the particle's size, as in this case the particle may interact with the free water surface and the mechanics of entrainment are different from what the traditional hydraulic literature on incipient motion is discussing.

Response: This is not true. Firstly, we didn't' test a range of flow conditions in the lab, we repeated one experiment 12 times (the hydraulics are presented in the appendices). Secondly, the mechanics of entrainment we present are exactly the same to the literature (Lines 169-174 in the paper). They are just linked to an accelerometer model and rotated to a different frame of reference in order to make the accelerometer measurement comprehensive.

Reply: 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).

Comment 6: The authors do not measure any flow hydrodynamics that could be linked to the sensor's metrics they present. Bed shear stress which is based on the bed surface slope is mentioned but it is not commented on how bed slope value was obtained (measured or estimated and how) Response: The slope was measured in the flume and estimated in the field (and that is documented in the paper). But I will insist that this has nothing to do with the validity of the definitions and the measurements as the commenter argues from the start of this commentary.

Reply: 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).

Comment 7: For the ellipsoid there is a strong effect of the orientation of the initial placement on the dominance of the forces and the resulting mode of entrainment. More emphasis on this dependency could be discussed in this works.

Response: I apologise for the repetition: There will be a big effect on the numbers derived under different orientations, but the same (or a similar) model should be applied. And the model we present accounts for the orientation specifically since we can measure it directly (quaternions).

Reply: the authors have misunderstood my commentary. I am not discussing whether their method can be applied to different orienations (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 expect the high range of acceleration recordings -if indeed needed- to be matched by a high frequency of recordings too (eg 500 Hz or more)?

Comment 8: For the field work there is no comprehensive description of the flow and bed surface characteristics over which the particle is being transported.

Response: It is not easy (or even possible sometimes) to take detailed flow measurements in shallow streams. For the stream we conducted the experiments (Erlenbach) there are numerous references in the literature where the commenter can find a lot of details about the topography and the bathymetry. We just placed the sensor on a plain bedrock and the conditions were typical of a riffle and pool setting. We also remove the first second from the measurements to minimise the effect of the local topography, we are interested into the forces during transport. For the purpose of this paper (demonstrating the calculation of inertial impulses) the slope and the shear stress should suffice for an understanding of the hydraulic forcing.

Reply: 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 microtopogrpahy 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?

New comments:

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? Is the framework the authors intend discussing refer to incipient entrainment (described in the lab) or transport processes (apparently relevant to the field work). 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? Given that the resistance (highly dependent on the resting microtopogrcounapy) 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? 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?).

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