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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.

g.maniatis@brighton.ac.uk

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Comment: The manuscript "Inertial drag and lift forces for coarse grains on rough alluvial bed" presents an interesting method for calculating forces acting on grains in motion in mountain rivers. I think the topic and approach is fascinating as has potential to greatly improve our understanding of transport and flow. My suggestions for improvement mainly focus on (1) explaining more of the conceptual physics and assumptions, (2) simplifying some of the writing, (3) explaining a little more how the flow conditions are perhaps not typical of gravel transport in mountain rivers, and (4) changing figure 1 to illustrate the actual experimental setup of the experiments.

Reply: We sincerely thank the author for the thorough review and the supportive com-

ments. We set out to address all the suggestions in a revised version. Hopefully this will become clear in our following responses.

Comment: I appreciate the physics framework that the paper is written in, but as a non-physicist I think it would help to have a little more explanation of some of the underlying conceptual ideas (translation into geologese?). Here is an example from the very beginning: To me, the title clearly claims that it explores drag and lift forces for particles ON the bed. After puzzling through it and trying to think through the physics, I don't think this is correct. The methods proposed only apply to forces acting on a particle when it is in active transport, not when it is on the bed. Maybe to a physicist, having "inertial" in the title would make it obvious that the grain isn't actually resting on the bed, but that wasn't the case for me. I am still not sure what the "inertial" part is supposed to tell me. I don't think this is just a matter of semantics; it's a matter of communicating clearly to the geoscience community. Many papers have been written (and that the authors appropriately cite) using force sensors that measure drag and lift forces for grains on the bed. This paper is doing something different, which is unique and fascinating-to measure forces on grains while they are in motion–but those differences should be made clear.

Reply: We appreciate the value of explaining the concepts using language that will be familiar to the reader. Some of the underlying concepts we present will be more familiar to people with backgrounds in hydraulics or physics, but we consciously target the audience of this journal (broadly the audience working on geomorphological and geological problems). In response to this comment we have made several revisions to the manuscript to improve its clarity.

The term "inertial" in the context we present primarily means that the reference frame we use to analyse particle forces is static, i.e. that the frame of reference doesn't move with the particle. This is not a common approach in sediment transport but is necessary to enable us to interpret the accelerometer measurements: these are meaningless if untransformed. In addition, the term "inertial" means that, in the reference frame we use, there are no so-called fictitious forces. This observation is very straightforward for the hydraulic forces (and the forces from the surrounding particles) acting on a target particle but needs to be specifically stated for gravity because gravity is a fictitious force for a free moving accelerometer (Appendix Figure A1a).

Finally, it is very important to clarify that inertial sensors measure forces only when the particle moves. The advantage of this is that the motion of the particle is captured explicitly (the sensor solves a very complicated force balance for us). The disadvantage of this is that there is no way to “decouple” the acting forces without complementary measurements (no information about the hydraulic drag component, for example).

All the above, which overlap with comments and suggestions from the other two Reviewers, are addressed in the revised manuscript.

Comment: I suggest adding a paragraph or whatever describing forces on particles from the flow when the particle is stable on the bed, when it is moving but still has its mass partially supported by contact with the bed, and when it is fully entrained by the flow, and what the accelerometer-based calculations actually measure.

Reply: This paragraph will be added and will include the description of the rolling threshold of motion as suggested by Reviewer 3 (we will revise the structure of section 3 of the paper). As we say above, it is not possible to describe all the conditions in a quantitative manner (this would require detailed hydraulics measurements which we don’t have), but we can clarify further which are the force components captured by the sensor.

Comment: Is the way to describe it that the measurements are not forces acting on the particle, but the net force that the particle responds to, from the combination of fluid and solid contacts along the particle boundary?

Reply: This is exactly the case; the only difference is that we use the term “resultant” for the force.

Comment: I think that simplifying the language in many places could also make the manuscript easier to read and understand. As part of this I suggest the authors take the time to go through it again and get rid of some superfluous words (such as “proven” in the second line of the abstract, maybe “regime” in the first line, and other places throughout. “has historically been proven” could also be replaced by “is”).

Reply: We accept this comment, it is true that parts of the paper can be simplified, and we will address that in a revised version

Comment: I think the manuscript would be more clear if the authors define drag and lift forces. It helped my understanding to look up physics definitions: drag is simply force parallel to the direction of fluid motion, and lift is force perpendicular to the direction of fluid motion. I did not realize that drag and lift were defined so simply (thanks, I learned something new). I suppose these are sort of defined on line 192, but this is not nearly obvious enough; I did not realize that these were definitions. In particular for lift, I thought it implied differential pressure on the top or the bottom of particles from different velocities (i.e. Bernoulli), and I was confused because it seemed like the authors could not know this since they have no measurements of fluid velocities or pressures. I realize I was wrong, but my point is that drag and lift are terms that bring preconceived ideas about the flow.

Reply: The fact that the Reviewer had to go through that enquiry makes the clearer definition of forces necessary. We will address that in section 3 of a revised manuscript.

Comment: I couldn’t quite figure out from the equations whether the water surface slope is accounted for; i.e. are the lift forces the authors calculate actually perpendicular to the mean flow direction, or are they instead parallel to the vector of gravitational acceleration? Figure 1 indicates that they are perpendicular/parallel to mean flow, not horizontal or vertical. Is a small angle approximation required, or embedded in the equations?

Reply: The resultant lift force (FL) is perpendicular to the mean flow direction. We will address that by revising significantly Figure 1 in order to include a clearer definition of the frames of reference and more details about the flume experiments (e.g. critical depth) as suggested in both the following comments here and the comments from the other two reviewers.

Comment: Another point that I think needs to be discussed, and limitations explicitly pointed out, is that the experimental conditions of the flume experiments are not representative of typical gravel transport Table B1 says that the flow depth was 0.1 m, which is essentially the same as the sphere diameter of 0.09 m. Grains the same size as flow depths are relevant to boulder transport, for example. I can accept the conditions as being informative even if not typical for gravel-bed systems, but it does mean that many of the scientific results, like the relative importance of lift and drag forces, may not be more broadly applicable.

Reply: We want to thank the reviewer for highlighting this. It is crucial to clarify that all the experiments in the thesis of Maniatis 2016 were designed to address transport at grain diameter – flow depth ratios close to 1 (D/H approximately equal to 1) for particles sitting on top of the surrounding bed. This is common for the transport of large grains in mountain streams [Schneider et al., 2014, Lamb et al., 2017] and the results should only be interpreted in this context. In Erlenbach, the D/H ration was also approximately 1 if a sphere of equivalent volume to the ellipsoid is assumed but the ellipsoid c-axis/H ratio was close to 0.3. Although the Newton Euler model that we present is general, we have no intention to claim that the results can be generalised in different transport conditions. We will clarify that in the introduction and in all the parts of the paper we can.

Comment: The Shields stress is very low for typical thresholds of motion (0.013), which my guess is related to the high protrusion and fact that the grain blocks basically all of the flow depth. Another factor in the flume experiments is probably that the hemispheres mounted on the flume bed only spanned a length of 0.5 m, which means that the boundary layer velocity profile was not anywhere close to developed. Somewhere,

the paper should say where within this distance the test particle was placed (i.e., what was the distance between the upstream edge of hemispheres and the starting position of the test particle?).

Reply: We agree with all those comments and our response above regarding grain protrusion and flow depth applies here. We will describe in more detail the flume experiments (adapting parts of Maniatis 2016) and we will revise Figure 1 in order to include a sketch of the flume, the position of the bed of plastic hemispheres, the position of the test particle and the critical depth. The flume bed upstream of the test section was covered by gravel leading to rapid development of uniform flow for several metres upstream of this section. There was a roughness transition between the gravel bed and the test section, leading to flow acceleration. Although the flow was non-uniform, which is very representative of natural conditions, the measurement of forces by the sensor can be tested in these conditions.

Comment: Similarly, it is unclear whether the result of lift and drag becoming uncorrelated from the lower flow (flume) to the higher flow (field) conditions represents the difference in flow depth relative to grain size or the difference in flow intensity. Also, would be helpful for Table B2 to have a calculation of tau* for comparison. Reply: For the first part of the question, we observe a generally weak correlation between the resultant drag and lift forces. The highest correlation was observed for the spherical particle in the flume (R=0.44). For the ellipsoid the correlation is consistently week (from R=0.17 in the lab to R=0.018 in the Erlenbach during transport). To answer why the correlation becomes even weaker in the field we would need a series of incipient motion experiments in the field and that was not possible. For the conditions we captured, the sensor was constantly in tractive motion after released. However, in the discussion we refer to the work of Shih and Diplas, 2018, where we see that the differences in turbulence will have a very strong effect on the applied forces. For the second part of the question, we will provide tau* and remove the metrics of Table 1 that rely on τb which probably complicate the comparisons (highlighted also by Reviewer 2).

Tables B1 and B2 will be combined.

Comment: I am a little perplexed by the measured particle protrusion of 0.045 m, i.e. half of the grain diameter, of both the experimental sphere and also the hemispheres. This means that the test particle was basically resting on the flume bed (or very close to it) in addition to resting on four surrounding hemispheres, right? And it also means that the hemispheres were not spaced as closely as possible, but spread out in order to allow the test grain to be down low, right? Figure 1 is a conceptual diagram that does not match the actual experimental design. This should be changed so that Figure 1 is drawn to match the experimental conditions. The experimental grain should be on a bed of hemispheres, not full spheres, the spheres should cover a length of 0.5 m (so I guess 5 hemispheres?), and the spacing and placement of the test grains should be appropriate so that it reflects the actual grain protrusion. Table b1 says the protrusion is half of the particle diameter, which means the particle was essentially resting on the flume bed? I also suggest indicating the approximate water surface on the figure, since it is basically at the top of the test particle. Table b1: was the protrusion the same for the ellipsoid?

Reply: Although the hemispheres where glued in positions that allowed for a 0.045 m protrusion of the sphere, the sand layer made the protrusion higher (by partially filling the gaps between the hemispheres) and non-uniform around the test particle ($\approx$ 0.050 m). The ellipsoid was only supported by the hemisphere bed and was fully exposed to flow. We will provide a clear sketch of the sensor position in Figure 1 using relevant material from Maniatis 2016 (draft figure attached).

Line 25: suggest simplifying wording of "multi-variate two-phase flow defined by a range of interacting complex subprocesses. . ." Line 34: change "it's" to its Reply: We accept the comments, we will correct and re-phrase.

Comment: Line 85: I'm not sure its useful for the authors to give their opinion that not being able to measure position has significantly limited the IMU use. That may be true

but there isn't any way to know if that is the main reason, and does it matter? The current paper doesn't solve this problem, it presents a new way to use the devices for another problem. Also, I think the tone of this paragraph is unnecessarily dismissive of previous work done using similar devices. I don't know what "best considered to be preliminary" is trying to say, other than to belittle this work. The authors come across as arrogant. To me, these works show that there is a benefit and potential of pulling different kinds of information from instrumented particles.

Reply: We have no intention of being dismissive of previous work. The comments about position and the wider use of IMUs is based on several discussions we had with colleagues from all over the world regarding this technology. Many previous studies are pioneering in developing the approach, but they rely on several assumptions about inertial sensors that are often not consistent. One of those is the prospect of deriving the position of the particle from acceleration measurements. This is not possible with current technology due to error accumulation during the integration of accelerations. Another common assumption is that calibration of accelerometers can be achieved under free fall. This is not possible because accelerometers cannot measure free fall (because all the gravity components are constant) and such result relies on the programming of the sensor which for many end users is a black box. Finally, it is often assumed that it is possible to derive directional information (direction of rotation for example) without analysing the frames of reference explicitly. We show the theory behind such analysis and hence why it is required. Many of the published results from early implementation of IMU sensors are no-longer considered reliable by the community, and those that are robust need to be read and interpreted carefully after consideration of the above limitations. We will rephrase our discussion of previous work, but we strongly believe that an open discussion needs to begin around this technology in order to make sure that we actually realise its potential.

Comment: Stylistically, I suggest combining lines 114-128 into one paragraph; I think it is better than having four very short paragraphs. Reply: We accept this comment, we will combine the paragraphs.

Comment: Lines 199, 202: I think the critical drag and lift force equations should have their own equation numbers, even though they are simple equations. They are important for understanding the analyses, and it was annoying to have to go back and hunt for in- line equations.

Reply: We will give present separate equations and explain the difference between the rolling threshold defined by the torques of equation 6 in revised section 3. Comment: 215: I think this point should be made more prominently, and explained, earlier in the paper: that the fluid is not being measured. When I got to this point I became confused; I was still assuming that the paper would compare fluid measurements and forces on the grains, and it did not make sense that the fluid part was ignored.

Reply: We will address the comment early in the paper and make sure that the reader understands that we don't measure hydraulic forces. We will also change the notation in order to clarify that the hydraulic components (FDr and FLr at the moment) are not measured and that the hydraulic threshold conditions are estimated from the arrangement of the slope (hence also not measured).

Comment: 226: Why 50 hz? More to the point, is there a physical argument that this sampling rate is sufficiently fast to capture the forces acting on the grain? In any case, the authors need to explain why this sampling rate was chosen. Would a slower or faster rate also work?

Reply: This frequency was chosen after considering displacements of 15 particle diameters per second (Drake et al., 1988) which is adequate for capturing the dynamics of the particles if collisions and strong interactions with the bed are excluded (different type and higher frequency piezoelectric sensors are more suitable for the full measurement of impacts).

Comment: 233: 0.5 m long is very short relative to lengths needed to develop boundary

layers (i.e. velocity profiles). How much distance was there between the test particle and the upstream end of the hemispheres? I presume that the upstream flume surface was planar other than the 1.5 mm sand? It seems to me that the possible effects of this this should be addressed or at least acknowledged somewhere.

Reply:The test position was 4.5 m downstream from the flume entrance (see previous comment on flow development). We will clarify that in a revised description of the experimental conditions.

Comment: 245: Give a little more explanation than just "section 4" for where these numbers came from. (in the caption to fig2 it says it used equations 9 and 10).

Reply: We accept this comment and will address it a revised version.

Comment: 252: Give citations and a more complete explanation of why entrainment is defined by this displacement.

Reply: This is the point where the particle has been fully dislodged from its initial position. It distinguishes full entrainment from sub grain diameter vibrations that do not fully dislodge the particle (but are also captured by the sensor).

Comment: 265 or around: I realize the slope is in table b2 but somewhere here say the key numbers, especially slope of 0.1 and flow depth of 0.15 m. Also describe something about the pocket or pockets the grain was placed. In the text says on a step, but explain what that means. Resting on 3 or 4 grains of similar size?

Reply: The difference between the field and flume data in that the flume records the forces when the grain starts to move, but the field only records forces when the grain is already moving. The ellipsoid was released over plain bedrock and the first second of the measurement was removed to eliminate bias from the transport time series (so the effect of the initial arrangement on the measurements from the field should be minimal). We will add the slope and the depth during the experiments in this sentence/paragraph.

Comment: Figure 2: in panel e, I think the Flcr and Fdcr lines are flipped. In panel a, the part labeled "vibration" really represents grain transport and rotation, right? See also line 291. To me vibration implies a grain wobbling or rocking back and forth in place. How would you define grain vibration as similar or different from grain transport? Is grain vibration just being defined as just transport over a distance less than one diameter? A grain rotating as it is transported?

Reply: The FLcr and FDcr are defined from the orientation of gravitational forces over the specific slope. Without knowing the local bed slope around the grain, we cannot predict which component will have a higher magnitude until the orientation of the slope is defined (we do that using the initial quaternion as stated in each panel). It is however useful to note that these are the norms of the force components and FDcr is defined over an x-y plain, which means that the 3D version of this diagram could indicate different threshold conditions. Grain vibration is defined as sub diameter motion that doesn't result in entrainment, e.g. the grain rocking within its pocket. This was defined visually for the flume experiments and used to calculate the critical impulse probability.

Comment: It seems a little surprising to me that at the end of the paper, the analysis and argument is made that lift forces are more important to entrainment than drag forces, but in Figure 2 panel A and D the drag forces clearly exceed their respective thresholds much more than do the lift forces. I don't know if it is just these examples that are shown or that I'm interpreting something wrong.

Reply: Figure 2 has examples from both the sphere and the ellipsoid in both flume and field conditions. When we collated the force data from the ellipsoid (Figure C1, Appendix) it became clear that the effect of the lift component is more pronounced for the ellipsoid than for the sphere (for both the laboratory and the field experiments).

Comment: I'm not sure what "scale difference" means. It seems to me that the main difference is that in the flume experiment the particle was resting stably prior to entrainment, and this entrainment was analysed. In the field case, the particle was just always and fully transported, and so the data just represent particle transport over a

rough bed without any data on the particles going from resting to mobile. Also the hydraulic conditions were different.

Reply: This is a point also raised by Reviewer 3. Our intention was to point out that in the field we should have different turbulence scaling than in the lab, and that this could partly explain the behaviour of the ellipsoid (and specifically the dependence on the lift component which is even more pronounced in the field). We understand that it needs further clarification and we will do that in a revised version.

Comment: 03: Explain how you know that the distributions are heavy tailed. I presume this is coming from the Weibull, gamma, lognormal fits presented later in figures D1 and D2? If you're going to say heavy tailed in the results section you have to explain it there. You could just remove this mention at this location I think without losing any understanding.

Reply: This is purely observational from the histograms of Figure 5. The fitting in the appendices concerns the calculations presented in Figure 6. We will re-phrase to "right -skewed" and clarify that in a revised version.

Comment: Figure 3b, right panel: the blue line (lift) looks like an odd or poor fit to the data, because there is a whole cluster of blue data points well above the line. Are there a lot of blue points hidden by the red points? Plot the data in some way that data points are not hidden, such as smaller symbols or open symbols.

Reply: We will replot Figures 3 and 5 in order to show the hidden points in the scatter graphs too.

Comment: 335: reword "the on the"

Reply: We will do

Comment: I realize that this is not the subject of this paper, and I am not sure the authors have enough data to really figure this out, but it would be interesting to know how well cumulative impulse from a given hop scales with transport distance of that hop.

Reply: This is very interesting but to calculate hop distances using our measurements we would need to integrate the accelerometer measurement and that is very unreliable at the moment. We plan experiments where position is calculated externally which could address this point.

Comment: “Extended analysis” is not a very informative section title; suggest changing to something different.

Reply: This section is going to be embedded in the discussion as suggested here and in the comments from the other Reviewers. 368: “of of”

Reply: Will delete.

Comment: A final point that I think the authors should acknowledge is that these calculations are untested. They do not know how correct these force measurements actually are. I think that is fine as long as it is stated; I would suggest saying that future work should explore and try to validate the accuracy of these measurements in some way.

Reply: We performed a calibration of the sensor (Maniatis 2016). This included a number of shaking table tests and rolling drop experiments. The main issue is that such a calibration is very sensor specific and not necessarily applicable to any other sensor (in terms of the actual corrections). We acknowledge that this technology develops very rapidly, and we may be able to derive much more accurate (in terms of force magnitude) results in the future. We are a bit more conservative about the derivation of the direction of forces which requires a completely different sensor set up (which is also significantly more expensive). This is the reason why we insist that similar experiments need to be repeated, in a number of settings and after unbiased calibration.

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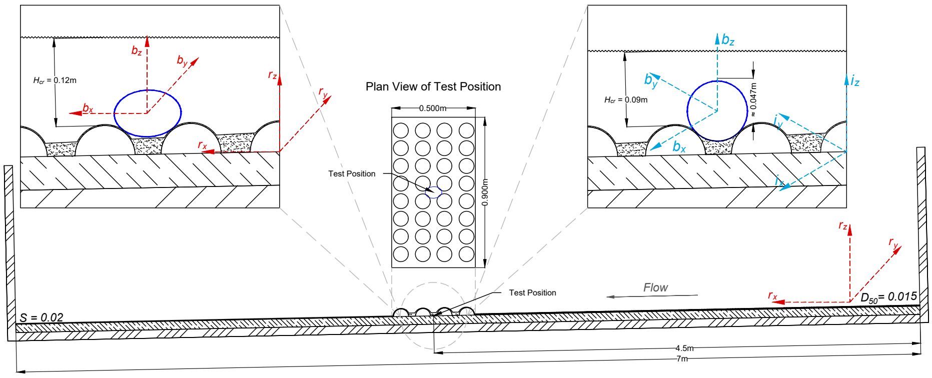

Fig. 1. Flume_arrangment_draft