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

Anonymous Referee #1

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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.

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. 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. 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?

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").

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.

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?

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. 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?). 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.

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 hemispheresâĂŤthis 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?

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

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.

Stylistically, I suggest combining lines 114-128 into one paragraph; I think it is better than having four very short paragraphs.

I confess that I did not check and verify the equations.

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.

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.

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?

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.

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).

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

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 inâĂŤthe text says on a step, but explain what that means. Resting on 3 or 4 grains of similar size?

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?

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.

300: 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 analyzed. 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.

303: 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.

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.

335: reword “the on the”

350: 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.

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

368: “of of”

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.

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