Response to Referee Comment 1

Dynamics and Mechanics of Tracer Particles

by Colin B. Phillips, and Douglas J. Jerolmack

We thank M. Chapuis for a thorough and detailed review. This supplementary file documents in detail the response to Referee 1's comments for the discussion paper titled: Dynamics and Mechanics of Tracer Particles. Throughout the following text reviewer comments are in plain text, and our responses are in *italics* following a '>' symbol.

Start Referee 1's comments:

1. General comments

Line and page numbers refer to the "friendly printed" version.

a. Summary of the article

This paper presents a bedload tracing experiment in a small watershed located in Puerto Rico. Tracking results are linked with hydrological and hydraulic data, in order to provide further insight of particle individual displacement at flood scale and annual timescale. The influence of shear stress distribution and downstream sorting according to grain size are also considered.

b. General comments

This study addresses the challenge of understanding what is happening "during the flood" for individual coarse particles. In trying to explain this question, this really interesting study provides useful new field-based data that show good agreement with laboratory experiments. Thus, it provides strongly argumented results to support the ideas that most particle transport occur with single step within one flood (i.e. partial transport of bedload) and that particle displacement is only weakly correlated with particle size at flood scale but that particle sorting appears at annual timescale. In addition, this study presents "the first active field confirmation of the selective deposition theory".

The authors highlight that results presented here could be site-specific and thus call for further confirmation in other river systems. However, I think that this study presents a great attempt to provide explanation of bedload dynamics at different timescales. Since the new methodology used is carefully detailed, the publication of this paper will enable other studies to further test the hypothesis presented in other river systems and thus could significantly improve our understanding of river morphodynamics.

In addition, as a field-based study providing insights on bedload transport processes in river systems, this paper addresses relevant scientific questions within the scope of ESurf.

Because of these reasons, I think that this paper is suitable for publication in ESurf after moderate revision.

In addition to specific comments and technical corrections, it would be useful to provide quick information on the probable reasons explaining missing tracers. From a morphological point of view (for one survey a flood occurred before the survey was completed; but is there a main reason explaining missing tracers, such as burial; transport further than the prospection zone?). Also, I would recommend to adjust the title a little bit, to better emphasize that the aim of the paper is to better understand coarse particles behavior. For example: "Dynamics and mechanics of bedload tracer particles".

M. Chapuis

> *We thank M. Chapuis for her detailed and thoughtful comments. We agree that a more detailed discussion of why tracers are missing is warranted. We have reanalyzed the tracer data to better account for unrecovered tracers. In many cases missing tracers for one survey were found in a subsequent survey, suggesting that these tracers were either buried beyond detection or simply not detected during the survey. As tracers were sometimes deposited in clusters it is quite possible that we detected one and not both tracers during a survey. Though we were very careful to scan each found tracer from all approach directions to reduce the chances of this type of misdetection. This leaves 7% of the tracers in the Mameyes reach unaccounted for. We suspect that these tracers were always beyond the survey limit, buried and never re-excavated, or defective. We can't be certain to which of these three is most likely for the 7% of unrecovered tracers. We have added text to Section 3: Field Sites and Methods to more fully address the missing tracers.*

We *agree that the title can be adjusted to better convey that the study is solely focused on coarse bed load tracer particles. That the title originally omitted the mention of bed load is a surprising oversight on our part. We have adopted the modified title suggested by M. Chapuis: "Dynamics and mechanics of bed load tracer particles".*

2. Specific comments

Line 6 p. 431: "The rate of bed load transport is known to vary both spatially and temporally due to turbulence and granular phenomena such as clustering, bed forms, compaction, grain protrusion, and collective motion […]": only granular phenomena are listed as examples. You may want to add the influence of the discharge variability to account for "water-related" factors (cf. what you explain in line 17 p. 435)? In addition, it is not clear what difference you make between clustering and compaction. I would also suggest to talk about "grain protrusion/hiding". Does "collective motion" refer to the influence of a mixed bed material (influence of sand on gravel mobility for example) or only to the "shock effect" between pebbles? Generally speaking, you may want to detail/rework this sentence a little bit.

> *We only list the granular phenomena as it is not presently clear to us what role discharge variability has in affecting bed load transport rates. Several studies (e.g. Marquis and Roy, 2012; Hsu et al., 2011) have observed discharge variability as a potential driver of granular phenomena, but the direct effect on the transport rate is still the granular phenomena. To our knowledge a concrete connection between discharge variability at the flood scale and the bed load transport rate at any given moment has yet to be established, thus we did not list other hydrodynamic phenomena. Clustering is the grouping of individual sediment particles, where as compaction refers to the overall bed. Increased compaction can result in less protrusion and greater frictional contact between grains. We have added the "bed" before compaction to further this distinction. Collective motion refers to the work of Ancey et al. (2008), where it is defined as the motion/ actions of groups of particles caused by particle particle interactions. We have added the term "hiding" following "protrusion". As other reviewers have requested that we shorten the overall manuscript we do not feel that we can explain each listed phenomena in detail and hope that a curious reader will pursue the references following the list of granular phenomena for more thorough explanations.*

Line 12 p. 431: "where the dominant transport regime is partial bed load transport, in which only a fraction of the bed is actively mobile at any time during a transporting event": then you

don't take into account for "bursts" related to turbulence effects? And also you don't take into account the statistical definition included in the definition of "threshold of motion" (cf. Parker 2004). You may want to rework this sentence to make it more precise since your are explaining it in the section 2.1.
> *We are uncertain as to how partial bed load transport precludes turbulent bursts. Floods with stresses near the threshold of motion generally do not fully mobilize the bed, and are thus in the partial transport regime according to Wilcock and McArdell (1997). Read alone P.431 Line 12 might leave a reader confused, however we feel that the following lines explain why floods near the threshold of motion confound our ability to get accurate bed load rates. Partial transport is both spatially and temporally discontinuous, which substantially complicates accurate field measurements. We have reworked this paragraph to enhance the clarity of this section.*

Line 15 p. 431: "Further confounding this issue is that gravel streams are well known to adjust their geometry to an effective discharge (Wolman and Miller, 1960), which occurs at a flow slightly above the threshold of motion for the median grain size (Parker, 1978; Parker et al., 2007); indicating that partial transport is the dominant transport regime within gravel rivers.": I agree that gravel streams adjust their geometry to an effective discharge that is above the threshold of motion for the median grain size (although do you have any reference for that? It would be useful for the reader to have an order of magnitude to refer to instead of saying "slightly above"). But I don't agree with the statement that partial transport is the dominant transport regime: in my opinion, it all depends on the hydrology of the river system, i.e. what is the "dominant discharge" frequency relative to the "morphogenic discharge" (the one "slightly above the threshold of motion") frequency. You may want to rework this sentence to clarify it.
> *We have reworked the above sentences to emphasize that they refer to most but not all gravel bedded rivers. We agree that the equivalence of the dominant discharge and the morphogenic discharge is not universally observed in gravel rivers. However, the data compilation work of Parker et al. (2007) demonstrates that in the large majority of gravel bedded alluvial rivers the channels are adjusted to near threshold conditions, which indicates partial transport as the primary regime for bed load transport. We have added further references and a numerical range following "slightly above the threshold of motion".*

Line 26 p. 431: you should also refer to active tracers (e.g. Busskamp, 1994, Chacho et al., 1989, Emmet et al., 1990, Ergenzinger et al., 1989 or Schmidt & Ergenzinger, 1992).
> *We did not intend to slight the work done by those using active tracers, but this work and the text surrounding these lines serves as an introduction to passive tracers. The analysis of active and passive tracers takes very different trajectories. In the text we do not mention active tracers as they are in general not low cost or long term monitoring solutions due to battery life. We have amended the text to specify that it concerns passive tracers only.*

Line 6 p. 432: "long term observations": refer to multiple morphogenic events surveys to be clearer, since "long term" point of view depends on the dynamics of the river systems.
> *We agree with the reviewer that "long term" is subjective when considering different climate regimes with varying flood recurrence intervals. We have removed long term from line 6 p 432 and line 25 p 431 and have rewritten the sentences to convey to the reader that passive tracers can sample over timescales that the typical bed load monitoring campaign can (i.e. the single to*

several flood scale).

Line 1 p. 435: you can also refer to Liébault et al., 2012 for field measurements with RFID tracers.
> *We do not refer to the results of Liebault et al. (2012) in the context of the displacement distributions as the reanalysis of the same data in Hassan et al. (2013) does not support the distribution determined in Liebault et al. (2012). Therefore we do not know which interpretation is correct.*

Line 19 p. 436: I think it would be worth specifying in which cases this statement applies (e.g.: no armouring, no sediment transport discontinuity).
> *We agree that there are several cases where downstream fining is not observed, and thus we qualify this statement with "near universal". The cases where downstream fining is not observed tend to be spatially limited to a small area of the catchment and even in these cases the majority of the river from source to sink displays downstream fining.*

Line 5 p. 438: specify how many tracers were equipped.
> *This information is specified on p. 439 on lines 10 and 11.*

Line 20 p. 439: specify on how many particles you made the Wolman pebble count.
> *Each pebble count contained 100 particles. We have added text to specify the number of particles per pebble count.*

Line 24 p. 439: instead of giving the numbers in the text, I would suggest to make a table out of them (columns: field site, D50 bed, population #, D50 tracers, deployment date, survey dates, recovery rates); same suggestion for line 7 page 440.
> *We are not sure that such a table is likely to add additional clarity to the manuscript without added a certain redundancy and taking up substantial space within the manuscript. In that several columns of the table will either be exceptionally dense or the table will need to be 7 columns by a minimum of 10 rows. This information is also available online with the data sets for interested parties. We have added a web link to the text directing the interested reader to the data sets and associated read me files.*

Line 1 p. 440: what do you mean by "following flood"? I guess you refer to a flood that prevented to complete the survey, but try to make it clearer
> *In this case our initial survey was cut short due to another flood that occurred during the survey, preventing us from continuing the survey due to safety concerns. We have amended the text to make this clearer.*

Line 12 p. 440: since detection limits greatly depends on the wand type and RFID tag type (cf. Chapuis et al., DOI: 10.1002/esp.3620), it would be useful to specify the series number of the wands and RFID tags used in this study.
> *We agree that the detection limits depend on the tag and wand used, however the tuning of the wand sets the detection distances. As the wands were constructed by Oregon RFID and then further customized for this field work there is no relevant serial number. Instead, we report the detection limits for the two wands used as they were empirically determined in the field prior to*

each survey. We have added additional text specifying the type of PIT tag (32 mm HDX) used as this information could be pertinent to future researchers wishing to resurvey these tracers. We have cited Chapuis et al., (2014) as a reference for further understanding wand detection limits (the omission of this relevant citation is due to its publication being after the submission of this Manuscript).

Line 21 p. 440: "Statistical values for the tracers represent the spatial average at the center of eight linearly spaced bins, where the number of bins was determined to ensure enough tracers within each bin for accurate statistics": this sentence and its aim are not very clear to me. Maybe you could reformulate it?
> *We have reformulated the above sentence to enhance reader clarity on how the mean and standard deviation of the tracers for the sorting analysis were calculated.*

Line 6 p. 441: you assume steady and uniform flow: do you take into account the influence of this strong hypothesis in your analysis?
> *We do account for potential side effects of using the depth slope product in the following paragraph, and in various locations within the manuscript where appropriate. It is because of this assumption and the difficulty in measuring the threshold shear velocity that we caution the reader that the coefficients from fitted lines are highly variable, but that the functional forms determined are robust. We agree that the assumption of steady and uniform flow is indeed a strong assumption, however it is an assumption that – while not often written – is implicitly made throughout field sediment transport studies as a prerequisite for using the depth slope product. We explicitly state it to remind the reader where the determination of the shear velocity comes from.*

Line 21 p. 441: starting from this line, maybe the rest of the paragraph should be moved to the discussion section.
> *We disagree and believe that the discussion of the shortcomings of the dimensionless impulse is useful to understand before reading the results. In this way a reader can understand the limitations of the results with greater clarity. These shortcomings of the dimensionless impulse are also very much a methodological issue.*

Line 26 p.443: I doubt the accuracy of Q is as high as 0.01 m3/s. you may want to reduce the number of decimals accordingly.
> *We agree and have reduced the number of decimal places for the given peak discharges.*

Line 18 p. 445: "Despite the second population of tracer particles being less embedded in the stream bed, its <X/D> follows the same trend as the first population when plotted against I*": do you infer from this result that the "embeddedness" of tracers does not influence their displacement? Can we go further and hypothesize that particle settling does not affect the "first displacement" data?
> *The data show that for this field experiment, the initial differences in embeddedness between the two populations is not apparent at the annual scale. We believe that it would likely be an overstatement to hypothesize that the first placement data are not artificially more mobile than the stream bed, however the data show that this first placement effect is not a first order control on tracer displacement at the multi-flood timescale.*

Line 22 p. 445: "Using all permutations of tracer surveys does require the assumption that the sequence of floods does not exert substantial control on the mechanics of particle displacement": this assumption is pretty strong indeed, is there a way to find what could feed this assumption (in fig. 8 for example)? Also, explain what in fig. 9 does not support this assumption.
> *The data show that to a first order particle displacement is controlled by the total applied stress to the river bed. The above assumption follows from a momentum conservation framework. There is reason to believe that flood sequence may exert substantial control through its alteration of the threshold of motion and sediment supply (e.g. Monteith and Pender, 2005; Turowski et al., 2011; Hsu et al., 2011), however we do not observe this. In figure 9 there is a fair amount of scatter above and below the linear best fit line, which could be attributed to flood sequence or embeddedness effects or other environmental factors. Sediment tracers that purely behaved in a momentum conserving manner would follow the line with no scatter. We are generally hesitant to speculate on the origins of the scatter in figure 9 as there is little basis for which to favor one phenomena over another. We have added additional wording to this section to clarify how this assumption does and does not affect the data analysis.*

Line 12 p. 447: "When we compare the displacements during a flood to the expected step length calculated from Eq. (1); it is clear that large particles have displacements that are close to the expected step length, while a significant number of small particles have much longer displacements (Fig. 11b)": I am not sure to agree with the interpretation of results of fig. 11b, especially I don't find it "clear": there are fewer "coarse" particles compared to "fine" ones (comparing with the D50), which explain why there are few coarse particles that had long displacements. But there is a large number of "fine particles" that experienced a single step. Although I agree that your following explanation about "large particles experiencing one single step while fine particles experience multiple steps" (line 18) is "convenient", I am not convinced. Maybe you could try to discriminate between particle which D>D50 and those which D<D50 and see if the difference in Xi/XS ratio is statistically significant to support your interpretation?
> *We have statistically analyzed the mobility difference in the lines preceding line 12 on p. 447 and in figure 11a where we find that smaller particles are more mobile. There are not enough mobile particles at the single flood scale to further test mobility differences among the mobile population. Unfortunately in near-threshold floods undergoing partial transport the majority of particles do no move. In figure 11b we can see that only three particles greater than the D50 have moved farther than two times the expected step length, while 12 particles smaller than the D50 have moved beyond this distance. These observations coupled with Figures 7b and 11a demonstrate a mobility difference based on particle size. We believe that a size dependent mobility difference (figure 11a), thin-tailed displacements (figure 7a), and a propensity for the majority of particles displacements to coincide with the expected step length (figure 7b and 11b) is ample evidence and not just a 'convenient' interpretation to indicate that larger displacements for smaller particles results from multiple steps. We have modified this section to improve the clarity of our results. We have rewritten the paragraph to make clear to a reader that the majority of particles irrespective of size have displacements that are close to the expected step length. We add an additional observation to state that larger particles (D_i>D_50) are less mobile and have not moved as far as the smaller particles. We have added additional material to*

the discussion section to show the reader where our interpretation of smaller particles being easier to remobilize during a flood stems from.

3. Technical corrections

Line 27 p.430: suggested change: “While […], coarse bed load transport sets the limiting rate […]”

> *We do not believe that the suggested change will result in a more readable manuscript.*

Line 13-14 p. 432 and in the whole text: “mobile fraction” instead of “fraction mobile”

> *The term “fraction mobile” is a short hand of “fraction of tracers mobilized” from the preceding sentence.*

Line 2 p. 433: “hydrologic forcing quantification” instead of “quantifying hydrologic forcing”

> *We do not believe that the suggested term will enhance the readability of the manuscript. To our knowledge there is no general term for quantifying hydrological forcing and have thus chosen “quantifying hydrologic forcing” as the simplest means of explaining the following section (Section 2.2 dimensionless impulse).*

Line 4 p. 433: “at particle scale” instead of “at the particle scale”

> *We do not believe that the suggested change will result in a more readable manuscript.*

Line 17 p. 434: “at field scale” instead of “at the field scale”

> *We do not believe that the suggested change will result in a more readable manuscript.*

Line 20-21 p. 435: is there a typo? A word might be missing: “by the product of shear stress magnitude and duration, the impulse”

> *We do not see a typo in the specified sentence.*

Line 12 p. 439: “A smaller population of 51 tracers was installed” instead of “A smaller population of 51 tracers were installed”

> *We have made the recommended change.*

Line 9 p. 441: “effect”: is that a typo for “affect”?

> *We have changed “effect” to “affect”.*

Line 24 p. 441: is there a missing worf after “critical”?

> *We have added additional wording to make clear that we are referring to the critical stress.*

Line 13 p. 447: “calculated from Eq. (1), it is” instead of “calculated from Eq. (1); it is”

> *We have rewritten this sentence in response to a preceding comment.*

Line 11 p. 447: I would suggest rephrasing the sentence “At the single-flood scale there does not appear to be a significant dependence of displacement length on particle size”, for example as follows: “at single-flood scale displacement length does not significant depend on particle size”.

> *We do not believe that the suggested change is grammatically correct.*

Line 1 p. 452: “we have” instead of “we’ve”
> *We have change the contraction “we've” to “we have”.*

Line 14 p. 452: I would suggest completing the end of the sentence with something such as “but more work is needed [to support this hypothesis]”; if possible, it would be useful if you could also describe how you would further explore this explanation.
> *We have added the suggested change. Unfortunately we are not convinced that passive tracer particles can answer this question.*

Line 20 p. 454: “sorting seems to result” instead of “sorting seems to results”?
> *We have removed the extra “s”.*

Figure 1: you may want to specify flow direction in the legend of fig. 1e (e.g. from left to right) since the South-North direction is not obvious compared to the channel direction.
> *We have added additional text to the caption to specify flow direction.*

Figure 2: it would be useful to indicate when deployments and surveys occurred.
> *We do not believe that this would be useful, should the reader be interested the information is contained in the freely available and permanently archived data files. We have provided a link to the files within the section.*

Figure 2b: do the gray lines correspond to periods where no flood occurred? If so, I would make them clearer (they are difficult to see). But if it is likely that large stage variations occurred, I would suggest deleting them to make sure the reader is not misled.
> *As stated in the legend the gray lines represent missing data. We do not have records of what occurred during these periods, however precipitation records do not indicate that large variations in river stage occurred during these periods.*

Figure 6: there is a typo in (b): “(b. inset)” instead of “(c. inset)”
> *We have corrected the typo.*

Figure 11b: add the n (number of tracers) corresponding to red crosses. What do you mean by “single tracer” or “multiple tracer”?
> *We have added the number of red crosses to the figure caption (n=214). The figure caption explains that the solid symbol represents that multiple tracers are plotted on top of each other.*

Figure 12b is too small, it is difficult to read.
> *We have moved the equations from figure 12b to the text.*

> *Additional references cited*

Ancey, C., A. C. Davison, T. Bohm, M. Jodeau, and P. Frey (2008), Entrainment and motion of coarse particles in a shallow water stream down a steep slope, Journal of Fluid Mechanics, 595, 83–114, doi:10.1017/S0022112007008774.

Hsu, L., N. J. Finnegan, and E. E. Brodsky (2011), A seismic signature of river bedload transport during storm events, Geophys. Res. Lett., 38, doi:10.1029/2011GL047759.

Marquis, G. A., and A. G. Roy (2012), Using multiple bed load measurements: Toward the identification of bed dilation and contraction in gravel-bed rivers, J. Geophys. Res., 117(F1), F01014, doi:10.1029/2011JF002120.

Monteith, H., and G. Pender (2005), Flume investigations into the influence of shear stress history on a graded sediment bed, Water Resour. Res., 41(12), W12401, doi:10.1029/2005WR004297.

Turowski, J. M., A. Badoux, and D. Rickenmann (2011), Start and end of bedload transport in gravel-bed streams, Geophys. Res. Lett., 38, 5 PP., doi:201110.1029/2010GL046558.

Wilcock, P. R., and B. W. McArdell (1997), Partial transport of a sand/gravel sediment, Water Resour. Res., 33(1), 235–245, doi:10.1029/96WR02672.