Response to Referee 3 Comment

Dynamics and Mechanics of Tracer Particles

by Colin B. Phillips, and Douglas J. Jerolmack

We thank the anonymous reviewer for a thorough and detailed review. This supplementary file documents in detail the response to Referee 3'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 3's comments:

General comments
The authors performed a thorough field study and contributed valuable data and insights to the scientific community. Therefore, I suggest that the paper be published after a thorough revision. Much of the revision should focus on clarifying the text and avoiding jargon.

The paper by Phillips and Jerolmack is very long and should be shortened. A lot of information is squeezed into the paper which makes the text very dense and difficult to read, hence shortening means eliminating some chapters and expanding on the remaining ones. I suggest taking out sections 4.3 and 5.2 which are difficult to follow, described in an especially short-handed way, are speculative, and seem to be offset from what has been described in previous sections. The remaining chapters contain ample material for a paper.
> *We agree that this manuscript is on the longer side of those published in Esurf, however when fully formatted it is unlikely to be substantially longer than the average article. We disagree that any section is described in a shorthanded manner. Other Referees do not share the concern that the manuscript is disjointed or that each section is not developed adequately. Removal of the sorting results would represent a substantial loss as while it is an almost universally observed phenomena present in all tracer studies it is never mentioned or quantified. It is important to recognize that particles – even with minimal differences in size – sort. We will thoroughly attempt to shorten the manuscript where appropriate.*

The authors use the term “alluvial” quite liberally. The paper would merit from a more critical view and discussion of the term “alluvial”.
> *We remain uncertain as to where the term “alluvial” is used loosely. We have added text in the introduction where we explicitly define bed rock and alluvial channels using the definition of Turowski et al. (2008) to eliminate potential confusion caused by the usage of the term alluvial. Turowski et al. (2008) defines bedrock channels as “channels that cannot substantially widen, lower or shift its bed without eroding bedrock”, following from this definition alluvial channels are then channels that are freely able to adjust their geometry without eroding bedrock.*

Currently, most of the figures are poorly legible and need to be published in a substantially enlarged scale.
> *The landscape format of ESurf compresses vertical panel figures such as figures 2, 3, 6, 7, 8, 11, and 12. A good indication of their final readability can be gained by digitally zooming in, if the lines and text remain clear then the figure should turn out fine in the final version. The above mentioned figures will not be compressed when published in the regularly formatted ESurf..*

Specific comments

Abstract: A lot of different findings are reported in telegram style. Abstract space is limited, hence the authors present information too brief to be comprehensible at a stage in which the reader has not yet read the paper.

> *We disagree that the abstract is presented in an incomprehensible manner. The goal of this abstract is to provide a concise complete summary in line with the criteria of the journal. The other two referees comment that the article and abstract are well written.*

Referee 2:

"The field data are hardly won, the analysis are well performed, and the paper is well written"

Referee 1:

> *"Does the abstract provide a concise and complete summary? Yes 10. Is the overall presentation well structured and clear? Yes 11. Is the language fluent and precise? Yes (I am not a native speaker reviewer but this paper is clear to me)"*

L.8: "exponentially distributed" vague; perhaps: ... decrease exponentially with travel distance

> *Here exponentially distributed refers to the measured particle displacement lengths. This indicates that the displacement lengths follow an exponential distribution. The travel distances themselves follow an exponential distribution.*

p. 430, L.13: …weak correlation … positive or negative?

> *Tracer displacements show a weak negative correlation. We have added the word negative before correlation in the abstract.*

p. 431 L.4: comma after Unfortunately

> *We have added a comma following unfortunately.*

L.16/17: The finding by Wolman and Miller that effective discharge is near bankfull flow and near a flow at which the bed D50 size is mobilized is not general. Several studies have observed a difference between Qbf and Qeff and suggested that Qeff systematically increases with sediment rating curve steepness (e.g., Hayward 1980; Nash, 1994; Bunte, 1995, 2002; Emmett and Wolman, 2001; Vogel et al. 2003; Barry et al., 2008; Doyle and Shields, 2008; Quader and Guo, 2009). A similarity between Qbf and Qeff appears to be limited to fully alluvial systems in which almost all particle sizes present on the bed are routinely mobile and cause rather flat bedload transport relations with exponents Qb = aQ^b of about 3 or less, i.e., typically sand-bedded streams or otherwise highly mobile streams. The authors report partial transport for almost all of their observed transport events, suggesting that that stream is not highly mobile.

> *Your comments piqued our interest as we were unaware of several of the references mentioned above, however we found that the referenced papers (among many others) do not provide enough evidence to suggest that in general the bankfull flood and the effective flood differ by any substantial margin. However, we do not want to cause a commotion over a line in the introduction so we will qualify the statement by saying that "many gravel rivers are known to adjust..." instead of just gravel rivers. Barry et al., (2008) finds that the calculation of the effective discharge is not sensitive to the choice of sediment transport equation, and makes no statement concerning a difference between bankfull and effective floods. Quader and Guo (2009) show that the effective flow is not the same as a flow corresponding to a particular return period (this study is also located in an urban basin where natural hydrology is questionable). This is a conclusion that Nash (1994) also reaches in showing that the effective flow does not always correspond to a flow with recurrence interval of 1 to 2 years. The recurrence interval of the bankfull flood is dependent on the climatic region, it is not restricted to flows with recurrence intervals of 1 to 2 years. Doyle and Shields (2008) along with Vogel et al. (2003)*

develop new metrics based on the total load, however the total load does not incise a landscape nor does it control channel geometry in bed load rivers. Neither of these two studies refute that the effective discharge is not the bankfull flood. The observation that sediment rating curves with higher exponents produce larger effective discharges is supported by Nash (1994), Bunte (2002), and Emmett and Wolman (2001), however a substantial drawback in each of these studies is that they use discharge and not stress in the magnitude frequency analysis. The distribution of discharge is not the same as the distribution of stress. Furthermore, the majority of the studies above (except Bunte where the sampling frequency of discharge is not reported) use daily average discharge values which do not capture the dynamics of the hydrograph in typical mountain streams and rivers. Daily average discharge preserves the magnitude of floods with durations greater than 24 hrs, and substantially reduces the values of flows with durations under 24 hrs. As an example using daily average USGS flow data for the Mameyes River produces a bi-modal distribution of discharge, which does not exist in the 15 minute instantaneous discharge records. The daily average discharge data are likely to introduce ambiguity into the computation of the rating curves and the conclusions drawn from them.

Partial transport is probably dominant time wise in the study stream, but partial transport is not dominant with respect to channel formation.
> T*he bed load regime is under partial transport for the effective flood (which in this case is the bankfull flood), and therefore it is dominant with respect to channel formation. We see no reason as to why intermittent bed load transport is incapable of forming a channel.*

p. 432, L.6: …sample … suggest: integrate
> *The two words have the same meaning within the context of the sentence and stylistically we choose to remain using "sample" as integrate is already used within the same sentence.*

L. 22: step-pool vs. alluvial: step-pool is not the antithesis to alluvial. The term step-pool describes a channel morphology but not the degree of bed mobility. Step-pool streams can be highly mobile–and alluvium they receive sufficient sediment supply. Step-pool streams with high sediment supply may route large amounts of coarse sediment while the steps may be preserved or be destroyed. By contrast, step-pool streams may also be almost entirely sediment starved an immobile. The term "alluvial" refers to a high degree of bed mobility that encompasses nearly all particle sizes on the bed. Alluvial gravel-bed streams typically exhibit pool-riffle or braided morphology with bars comprised of particle sizes that are routinely mobile, but even step-pool and plane-bed streams may become alluvial given sufficient sediment supply in relation to transport capacity.
> *In general we provide the terms alluvial and step pool to convey to the reader that these are two morphologically different streams, however we now see that the term alluvial can be construed to mean many things. What we hope to convey with the descriptions is that one stream is a particularly rough bed rock reach and the other is an alluvial stream with minimal bed forms. We have rewritten the lines to more clearly convey the stream types in line with the definitions adopted in the general comments above.*

P. 433, L. 11: …much smaller … suggest: much shorter L15: define "number density" of moving grains
> *We adopt the suggested word change. The number density refers to the number of moving particles per a given area. We've changed number density to "surface density (particles / area)".*

P.434, L8: define "surface density"
> *Defined above.*

L. 21ff: …heavy-tailed statistics … summation of
exponential step lengths… Given that this section is meant to serve as an introduction to the topic, I suggest avoiding jargon. Rather provide a visual description of the distribution of step lengths.
> *This section is intended as an introduction to sediment transport dynamics, and not an introduction to anomalous statistics. We hope that the interested reader can pursue this topic in the associated reference of Hill et al., (2010). As it will be very difficult to shorten the overall length of the paper and provide complete descriptions for entire papers and sub fields.*

P. 435, L. 6: …two limits … you are not really proposing limits, but rather you include the full range of possibilities of motion between one little step and continuous motion. Perhaps "end members" may be a better term.
> *It is not clear to us how the proposed minimum and maximum possible transport distances are not really limits. If a particle is displaced in a flood it can either move a minimum distance or move for the duration of the flood. If they were end members of a range of displacements then both should be possible, however limit 2 is very unlikely to occur and cannot be exceeded, and is thus not really an end member but a limit.*

P. 437, L. 3: please explain what you mean by "equilibrium profile" A stable profile of longitudinal sorting in which particle sizes decrease downstream? I would expect that tracer particles sort according to size within the reach scale, reflecting natural spatial sorting mechanisms such as downbar and bankward fining. The authors should explain that they are not concerned with reach-scale longitudinal variability. Please explain: "elongation of this sorting profile"
> *This sentence and section describe previous experiments on particle sorting, not our results. An equilibrium profile indicates no net deposition or erosion, stable would also be an adequate definition. One would generally expect that spatial variability due to large morphologic structures could complicate a sorting profile. We generally do not observe this, however perhaps we do not have enough tracers to observe reach scale patterns.*
As more sediment was fed into the flume the front of the wedge of sediment progrades downstream, as this occurs the sorting pattern established in the beginning stretches downstream with the front. This results in an elongation of the initial sorting pattern.

L. 5/6 downstream (transport?) distance? Please explain: Extraction length L.8: extracted? Do you mean entrained or deposited? Please clarify. L.9: input or? Source L.13: "extracted by deposition"? Please explain L.15: …95% recovery at any time between floods or at the point downstream from which on no further entrainment occurred? L.16: …that the (insert) "longitudinal" sorting L.16: …of the gravel mixture (at any point X*? L.22: …standard deviation of the grain size distribution of the seeded tracer particles or of the tracers at a distance X* downstream? Given that Section 2.3 is meant to serve as an introduction, I would suggest clarifying and simplifying this section, and perhaps providing a sketch diagram.
> *Downstream from the sediment source. We have added text to lines 5/6 to clarify this. The extraction length represents how much of the coarse sediment in the initial sediment mixture remains upstream. An extraction length of 1 indicates that all of the coarse sediment has been deposited. Examples are given in the text following the introduction of the extraction length to give a reader intuition as to what this is. The end of line 8 specifies deposition, "... extracted (deposited).". Input refers to the experimental setup where as in a natural environment we generally consider sediment as having a source but neither would be incorrect, hence "In laboratory experiments and in natural rivers, L is taken as the distance from the input/source...". We have changed "is extracted by deposition is" to "... has been extracted (deposited) as...". This would refer to where 95% of the recovered particles are*

upstream of the surveyor's position. The point at which no further particles are entrained from would represent 100% recovery and would be L, but since recovery percentages are almost never 100% it is difficult to determine the leading edge. We have inserted "downstream" in the place of "longitudinal". The results of Fedele and Paola (2007) provide solutions for all locations along the profile. Sigma refers to the standard deviation at a location along X. We have noted that sigma refers to the standard deviation with respect to X*. This section introduces the various concepts of previous work and we provide the relevant references for the interested reader where there are several definition sketches.*

P. 438, L.15ff: The 1.2 km long study reach is reported to be nearly straight with minimal meandering. It seems to be a plane-bed channel (which may have a meandering thalweg), but based on the Montgomery-Buffington stream classification, a plane-bed morphology is not expected at a stream gradient as low as 0.0078 m/m, a gradient for which a pool-riffle morphology is more typical. Could the authors comment on this apparent misfit? The bed is perhaps too coarse to permit that pools and riffles are shaped by the commonly occurring flood events. The coarse plane-bed channel seems to be another indication that the study reach should not be characterized as alluvial, but perhaps as a coarse scour channel.
> *This stretch of the Mameyes seems to be transitioning between the different morphological units presented by Montgomery and Buffington. Their classification system was developed using a limited set of observations within the Washington cascades range, and it is unlikely that they were able to capture the entire range of slopes that their proposed morphologies can exist at (though they do have several sites with slopes very close to 0.0078). The study reach is alluvial according to the recent definition of Turowski et al., (2011). We are not sure what a coarse scour channel would be, as this section of the Mameyes certainly looks alluvial.*

Figs. 3 a and 3b should be published at a considerably enlarged scale.
> *This comment is addressed in the general comments section above.*

P. 439, L.9: …variability in the (insert) surface grain size distribution, … The D50 and D84 can typically be determined relatively accurately from a pebble count, even if only 100 or 200 particles are collected. How many particles were collected per pebble count? L.14: What are the D50 and D84 sizes? Seems that the D84 is about 300 mm in both study reaches, but Fig. 1 is too small to actually determine those sizes. L.24: was the tracer deployment reach located at the start of the flood plain?
> *We have inserted "...surface grain size...". We collected a minimum of 100 particles at each pebble count, and have added text to note this within the methods section. There is a fair amount of scatter within the D84 values from reach to reach, therefore we used the grain size data at the reach and above and below the reach to create a trend from which we took the reach D84 from. The D50 is given in line 19 while the D84 is given following the introduction of equation 5 in the discussion section. Figure size is addressed in the general comments section. The tracer particles were installed at the start of the alluvial section of the mameyes which is just before the flood plane becomes fully evident.*

P. 440, L.19: …downstream of the "input?" location of the … L.21: …downstream of the start of the alluvial plain…Were the tracers not inserted just at the upstream end of what the authors call alluvial plain? L.21ff: Statistical values…I do not understand that sentence. Please clarify what you did and why you did it.
> *We have rewritten line 19 to clarify that the pebble counts start at the starting location of the tracers. There is approximately a 200 m difference between the start of the tracers and the start of the alluvial plane. We have rewritten this line to make it clear where the sorting analysis starts from. We have rewritten line 21 to make clarify how the statistical values used in the sorting analysis (mean and standard deviation of the tracers) were calculated (we have also moved this line to the results section*

so that it can coincide with the figure).

P.441, L.3: … 95% of the tracers recovered … at any of the 5 or 6 individual surveys with and increasingly longer L for each survey? I suggest preparing a new fig. that in analogy to Fig. 5 shows the (linear) decrease of fraction mobile vs. U*c and placing that fig. where the two methods for determining U*c are discussed.
> *The sorting analysis is only preformed on results from the final survey. We have added text to line 3 noting this. We do not see the need for an additional figure that is essentially a duplicate of an already existing figure. Instead we have added text to refer the reader to the appropriate sections.*

P. 442, L.22: (insert) "Shields" stress
> *We have added "Shields" before "stress" in line 22.*

P. 443, L.8ff: In 5 out of the 6 flood events, only between about 5 and 40% of the tracers moved, hence only a small percentage of the bed D50 particles are typically mobile. With no bars visible in the photos of the study reach, I don't see how those few mobile D50 particles act to shape the channel. Channel morphology is set by the large and typically immobile large cobbles and boulders. I doubt that such a channel should be classified as alluvial. It seems to be a scoured channel bed over which particles typically smaller than the bed D50 size move without much effect on determining channel morphology.
> *The data in figure 5 and sections 4.1 and 4.2 only represent single flood events. There are larger floods that move larger than the D50, and the tracer particles represent a range in particle size centered on the stream D50. Flows that are large enough to move the D50 are also large enough to move the particles that are at the bottom of the stream bank, and hence these flows can shape the channel.*

Fig. 6: Caption: Why "Frequency magnitude distribution...?" and not simply cumulative frequency distribution? The term "frequency magnitude distribution" makes it appear as if a Wolman and Miller magnitude-frequency plot was being referred to. Inset to fig. 6b: the authors should use the same scale for I* in the inset and the main figure. L.23 … threshold of motion (insert) "of the bed D50 size." L.29 … threshold of motion (insert) "of the bed D50 size ?"
> *Figure 6 has frequency on the dependent axis and shear velocity magnitude on the independent axis. At the suggestion of Referee 2 we have changed it to "magnitude frequency". The inset has the same range as figure 6b. The point of the inset is not the shape of the distribution but that the peak coincides with the bankfull flood. The threshold of motion was determined from the fraction mobile data which incorporates particles with a range of sizes (0.5 to 2 times the D50) and thus it is not the threshold of motion for just the D50. To avoid confusion we have added the subscript one to U*c figure 6 a & b and added U*c1 following the text "threshold of motion". This way a reader will know what is being referenced.*

P. 444, L.12: what are the Q, U*, tau*, and I* of the characteristic flood? I suggest indicating those values in all diagrams of Q, U*, tau* and I*.
> *Indicating each of this quantities in Figure 6 will only make an already dense figure more cluttered. The values of the characteristic flood are given for both U* and shields stress on line 21 p. 443. We will note the characteristic discharge and I* in this section in the revised manuscript.*

L. 21: What are the characteristics to those two flood with shorter transport distances?
> *The floods in question were near the threshold of motion and did not mobilize many particles. This is noted in the discussion section.*

L.24ff: We plot the dimensionless tracer distance … Please show the plot in a figure.
> *These results are plotted in figure 7a. We have added a reference to figure 7a to this line.*

P. 445, L. 17: suggest "increases" rather than "scales"
> *Here we believe scales is a more representative description of what is occurring. The mean displacement has a fair amount of scatter and therefore we use scales with instead of just increases.*

L.19: … less embedded … that raises the interesting question of how deeply the tagged D50 particles were embedded in the bed. The authors may not know how deeply particles were embedded, but perhaps the authors could provide a value of how many of the recovered tracers were found on the surface (were the tagged particles painted as well?). The authors must know something about this subject to make the statement that tracers from population 2 were less embedded than those from pop. 1.
> *This comment is addressed within the response to referee 2's comments. We summarize the response here. The particles were not painted. The two wands that were used had different detection depths (10-20 cm and 50 cm). From this we could infer if a particle was buried beyond the first wands detection limit or not. At the final survey no more than 6% were buried. Since the tracers in population 2 were placed on the surface of the stream bed a year after population 1 we observed that they were not worked into the stream bed like the first population.*

P. 446: Could the authors refresh the reader's memory about why a linear relationship is expected or reasonable between X/D and I*?
> *A linear relationship is reasonable as the particle velocity is linearly proportional to the excess shear velocity. The cumulative particle displacement is a sum of all of the particle displacements. The sum of a series of linear responses remains linear. We will add additional text to section 2 (theory) to build the readers intuition on this subject.*

P. 447, L.3: … All (insert) "particle size" distributions Fig. 11: should be substantially enlarged for publication. I also suggest plotting Fig. 11 b in log-log scale to better compare the number of data points that lie above and below, and right and left of the values of 1 on the x- and the y-axes (plot a dashed line for Di/D50 as well). Seems to me that except for the NW quadrant of the plot, there are about equal numbers of data points in each quadrant (about 20 in NE, 30 in SE, and 25 in SW). Does your interpretation still hold if there are about equal number of data points in each of the three quadrants (=except the NW one)?
> *We have inserted "particle size" into line 3. Figure size is addressed in the general comments section. We have added a dashed line for Di/D50. Plotting figure 11b in log log does make the figure clearer as there is less than one order of magnitude difference in both directions. Given that there is expected to be a fair amount of noise around the expected step length all particles within 0.5 of the dashed line should not be included in the counts for the East West quadrants. This would account for the scatter in both directions as the minimum expected step length is 1. This reduces the counts to about 7 in NE and 20 in SE. This fits with our interpretation that particles that have moved farther do so by taking multiple steps instead of longer steps, and that smaller particles are able to be re-entrained more easily and therefore can take more steps.*

L. 13: should be a comma, not a semicolon behind Eq. (1)
> *Indeed, we have corrected this.*

L. 25ff: the information on the tracer starting locations should be moved forward in the text to where tracer deployment is described. Fig. 12 needs to be substantially enlarged in the published paper.

> *We have added additional text to section 3 to clarify the starting location of the tracers. Figure size is addressed in the general comments section.*

P. 448, L.2: … deposition of the coarsest particles (insert) "in the reach upstream". L.11: … spatial decrease (insert) "in" L 16 … exponential (insert) "function"
> *In the context of this sentence there is not really a reach upstream, we are simply looking at the decrease in the D84. As a mixture of particles moves downstream the coarsest are deposited. We have deleted the s on "decreases" and added "in". We have inserted "function" following exponential.*

P. 449, L. 10: … to scale exponentially (with what?).
> *The distribution of particle velocities follows an exponential.*

L.15: instead of … most like tracer displacement" I suggest most likely displacement of a particle of the bed surface D50 size
> *We have measured tracer particle displacement, and from this we might be able to assume that particles of a similar size on the bed behave similarly. However, we do not know and therefore do not equate the bed displacement to the tracer displacement. See response to Referee 2.*

P. 450, L.13 ff: Indeed, … and what are the depth, Q, v, etc. of the characteristic flood and bankfull flow?
> *We have added the depth and discharge at bankfull to the revised manuscript. We do not have a measure of the stream velocity.*

P. 451, L. 13: … effect on flow resistance, (delete) "that" the resulting... L. 14: … resulting particle transport (insert?) distance?
> *We have deleted "that" and added "distance".*

P. 454, L. 14: ...that tracer remain (delete "s")
> *We have deleted "s".*

P. 455, L. 2: It may well be that the frequency of flood events drops below some commonly occurring flood) (e.g. Segura and Pitlick 2010), and that that commonly occurring flood (be that the characteristic or the bankfull event) just starts to transport the bed surface D50 size. But how do the authors then infer that the characteristic (or bankfull) flood performs the maximum geomorphic work? The characteristic flood in the study stream moves some (typically 5-40% it seems) of the bed D50 particles over a few meters per flood event. Such marginal particle motions do hardly have a significant channel shaping effect. There are no reported bars comprised predominantly of particles in the D50 size range and that could subsequently be shaped by the characteristic flood. All that the characteristic flood in the study stream seems to accomplish is to let a few D50 particles scoot a few meter further downstream over an otherwise very rough and mainly immobile bed. I presume that the plane-bed morphology of the coarse bed was shaped by some rarely occurring large event that moved the D84 and larger particles. This kind of channel and bedload movement is often observed in US Rocky Mountain streams in forested watersheds.
> *This comment is answered in several locations above. A summary of those answers follows. The tracer particles range from the bed D16 to the Bed D84. Floods that transport the tracers are also capable of eroding cobbles along the stream bank, and thus shape the channel. We do not see the absence of bars as an indication of a flows inability to shape a channel. Bars are generally indicative of a sediment storage and high sediment supply. If there was a rarely occurring large event that shaped the channel it would have eroded the stream banks, and the channel geometry would reflect this large*

flood (it doesn't).

I am not surprised by the relative similarity in scaled tracer travel dynamics between the Mameyes and the Bisley 3 sites. Both sites have similar particle size distributions, and both sites transport some D50 particles during commonly occurring floods. The only difference is channel morphology (step-pool vs plane-bed: the lower gradient reach is to flat for step-pool sequences to develop), but there there seems to be no major difference in bed mobility.
> *We remain surprised that the tracer dynamics can be tightly collapsed in what are very different looking streams by accounting for the threshold of motion and a frictional resistance term. We have never seen it done before.*

> *Additional references cited*
Turowski, J. M., N. Hovius, A. Wilson, and M.-J. Horng (2008), Hydraulic geometry, river sediment and the definition of bedrock channels, Geomorphology, 99(1–4), 26–38, doi:10.1016/j.geomorph.2007.10.001.