

Interactive comment on “Tracing bedload transport in a high-elevation, formerly-glaciated mountain basin” by A. Dell’Agnese et al.

Anonymous Referee #1

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Summary

This article compares the transport distances and virtual velocities of RFID sediment clasts in two contrasting reaches within a formerly glaciated catchment. These two catchments represent an upper valley with a lower gradient reach and a steeper lower reach experiencing ample transport. It is argued that previous glaciation has imprinted the current topography on the rivers indicating the long lasting effects of alpine glaciation. The article represents an exceptional amount of field work and a hard won dataset that the authors should be commended for. I have some reservations on the validity of the conclusions and how these results may tie into the broader questions, however I believe that the authors can suitably address these reservations without the need for much additional analysis.

C157

General comments:

I think the manuscript could benefit from at a more detailed comparison of the upper and lower reaches. At the moment the observations are presented with minimal discussion as to why sometimes reverse trends are observed (see specific comments below). As a starting point I would recommend (and very much look forward to seeing the results) treating the data cumulatively in conjunction with the flood to flood scale (such a treatment seems to parse through some of the inherent variability, see Bradley and Tucker, 2012 or Phillips and Jerolmack, 2014 for some recent examples). There is significant scatter in the data on the single flood scale (this is just the nature of variability in natural rivers and bed load transport, it can’t really be avoided), much of this scatter may be substantially reduced by comparing the average cumulative displacement for each flood against the cumulative duration that the tracers have spent above the threshold of motion. It would be interesting to see the resulting trends and if the US and LS have different functional forms or similar functional forms. The resulting trends would be similar to figure 11, but would instead include time on the independent axis rather than the categorical variable of flood type, likewise displacement would replace virtual velocity.

It would be interesting to know if the two river segments are adjusted to pass the water and sediment supplied. In that are both river segments adjusted to near threshold conditions typical of gravel rivers (Parker et al., 2007) or do their particular positions within the formerly glaciated terrain cause contrasting or similar deviations from the generally expected form. In a sense is the glacially imprinted landscape actually present in the stream dynamics at large. The figures seem to suggest that increases in slope are being compensated for by increases in particle size, and that would suggest that the supply of particles may be more fundamentally important to the longterm evolution of the topography (similar patterns are observed in Attal et al., 2015 for a catchment in the Sierra Nevada mountains). If true it would suggest (to me) that the river may adjust rather rapidly to imposed sediment and water, but that a limiting factor could be

C158

hillslope adjustment (or in this case the very steep valley sides) which is in turn limited by the rivers ability to remove sediment. In any case the degree of coupling between valley slopes and the channel is an idea that should be built on in the manuscript and discussed in more detail. I think the authors could test with their available data as they already have the cross sections to compute the needed parameters. One could compare these rivers to the recent global compilations (Li et al., 2014; Trampusch et al., 2014) of rivers and check if their dynamics are any different from other rivers. From the cross section surveys one could calculate the ratio of bankfull shear stress to the critical shear stress to see if the two river segments are adjusted to near threshold conditions (on average). At the moment the reliance on discharge makes the results difficult to compare between the upstream and downstream reaches and especially difficult to compare with other field studies, providing an analysis in terms of shields or shear stress will greatly extend the utility and scope of the current results. There is certainly a case to make against using a depth slope product to determine the shields stress (or shear stress) as the cross section is not necessarily constant and therefore computed values will vary quite a bit with channel geometry, however the same problem exists for discharge. I would suggest computing a reach average peak shields stress for each flood instead of discharge or perhaps if the authors wish to remain using discharge then perhaps utilize the width normalized stream power. Likewise the authors could compare their channels to the data presented in Parker et al. (2007) using discharge instead of bankfull shields stress, though I would encourage the use of shields stress.

Consider adding additional discussion on why particle travel distance is weakly inversely related to particle weight in US and has no relation with particle weight in LS. This is intriguing but it is not obvious as to why. The manuscript would also benefit from additional discussion on why snow melt floods are more effective in LS.

Some discussion should be added as to the effect of unrecoverable tracers on the dataset and the recovery percentages should be reported where appropriate. Additional discussion on the different populations of tracer particles should be added as

C159

well (or included in the supplementary), it is not clear which populations are used in the analysis. Questions to consider include: did the various populations behave in a similar manner or are newer populations more mobile because they have yet to be worked into the creek bed; do the first floods after tracer installation result in consistently larger displacements. Similarly the methods could benefit from a more detailed description of how the tracers were installed into the river (embedded by replacing a particle of like size, or simply placed on the bed).

The following comments and critiques are given in the order that they appear in the manuscript.

P.420 Ln.17: An appropriate reference for the various detection distances and how that is related to tracer size and orientation is Chapuis et al., 2014.

P.420 Ln.25: The work of Bradley and Tucker (2012) is relevant here as they record some of the highest recovery rates for yearly surveys.

P.425 Ln.20: If possible please provide the detection ranges for the RFID antennas used (horizontal and vertical).

P.427 Ln.6: How was the integral treated when the threshold of motion doesn't fall on a sampling point? Was the hydrograph interpolated between sampling points and then integrated?

P.427 Ln.9: Could you describe the digging tests in LS to determine the active layer.

P.429 Ln.1: Are all of the following Qmax values listed for intra or inter survey floods. For the intra-survey flood, was it flooding during the survey?

P.429 Ln.1-25 & P.430 Ln.16-28 + P.431 Ln.1-24 : Could these lines may be turned into a table with columns: survey date, Qmax, %mobile, mobile classes, maximum travel distance, rain/snow flood, percent of total number of tracers recovered (or other columns). At the moment the information is difficult to assimilate due to the large number of events and list like nature (I am not sure there is a good way other than a

C160

table to describe this information). Could you also report the percentage of the total number of tracers installed that were recovered for each flood. (These tables could also be put in the supplementary material along with the other supplementary material already present).

P.430 Ln.6-7 & figure 7a: My interpretation of Figure 7 is that for larger floods the degree to which a lighter particle travels the farthest increases. This doesn't really say anything about selective entrainment. How representative is the maximum travel distance given that there are very few heavy tracers, in that could much of this trend be due to sample size? Tracer displacements tend to be well described by exponential (Phillips and Jerolmack, 2014) or gamma functions (Bradley and Tucker, 2012), for a small number of tracers we are not likely to observe large displacements because they are rare. For larger populations of tracers we are more likely to be able to sample the tail of the distributions and observe large displacements. I am not sure if what I am suggesting is the case, but caution should be used when drawing conclusions based on a small number (compared to the number of lighter tracers) of heavy tracers. A less biased indicator would be the 90th or 95th percentile displacement, but the bulk of the data seem to show relatively equal displacement distances. It would be useful to provide the number of tracers within each boxplot in figure 7b (a number above or below the top whisker would suffice I think).

P.432 Ln.10: Could you add in the discussion a possible explanation for why the W2 weight class has such a high median transport distance as the rest of the weight classes are remarkably consistent with each other.

P.432 Ln.11: Without additional information figure 8 does not necessarily demonstrate equal mobility. It does demonstrate that transport distance does not depend on particle weight. Are similar percentages of the tracer weight classes mobile. Both figure 7 and 8 are very intriguing, but it is figure 6 that tends to show if there are mobility differences.

P.432 Ln.15-19: It is not clear to me why the threshold discharges should be the same.

C161

Are the cross sections the same or has the slope and particle size adjusted just so that the same amount of water is now capable of moving larger particles? Please if possible estimate what these discharges would be in terms of shields stress for a representative cross section or lacking a stage discharge curve what are the representative shields stresses for the bankfull discharges?

P.432 Ln.21-23: Are immobile tracers included in the mean transport distance? It is not clear from the text so far where they are included (other than figure 6) and excluded in the analysis.

P.434 Ln.18-26: Could you specify or hypothesize as to the origin of the valley step, it seems to be an essential piece in decoupling the upstream and downstream sections of the river. Is it glacial in origin?

P.437 Ln.12-19: This is a rather long and confusing sentence. These points would be better emphasized as several separate sentences.

P.437 Ln.20: These ideas are not integrated into the body of the work. It could be true, and I think the authors may be able to make a case for the claim that reduced transport in the US reach will provide fewer tools to erode the knick zone (valley step) separating the LS and US reaches and thus the decoupling between the two reaches will persist, but this argument is not clearly made.

Figure 2. A scale bar or mention of scale in the caption for each photograph would be useful.

Figure 3a. It would be useful to have the CDF for the tracers on this plot as well. At the moment it is not as insightful to compare the tracer grain size histograms to the stream cdf (it is not the point of 3b to compare to the stream, but it is nice to be able to graphically understand what part of the bed distribution the tracers sample).

Figure 3c. Is there a reason why the fitted functional relationship is not shown?

Figure 4. Please add a line delineating where (approximately) the threshold is located.

C162

Figure 6. Could you put the discharge or another metric associated with the flood (shear stress, stream power, or duration) in each plot. A number in the upper right would be useful i.e. $Q=nn$ in the upper right of each plot (or better yet the shields stress or width normalized stream power). Where are plots J and K or are they mislabeled?

Figure 10. Consider adding to the discussion of this figure why the mixed events result in the lowest virtual velocities. It would be logical (maybe) to think that they should be between the snow melt and the rainfall floods. Is the trend for the rainfall induced flood in part A significant? Overall these are very interesting results in that it seems that particle virtual velocity is nearly independent of particle weight, this result is, to my knowledge, novel and undersold in the manuscript.

Figure 11. Typo in caption ("Snomelt").

References cited in review: Attal, M., Mudd, S. M., Hurst, M. D., Weinman, B., Yoo, K., and Naylor, M.: Impact of change in erosion rate and landscape steepness on hillslope and fluvial sediments grain size in the Feather River Basin (Sierra Nevada, California), *Earth Surf. Dynam. Discuss.*, 2, 1047-1092, doi:10.5194/esurfd-2-1047-2014, 2014.

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C163

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