Earth Surf. Dynam. Discuss., 3, C743–C754, 2016
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Interactive Comment

Interactive comment on "Coarse bedload routing and dispersion through tributary confluences" *by* K. S. Imhoff and A. C. Wilcox

K. S. Imhoff and A. C. Wilcox

kurt.imhoff@umontana.edu

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(1). Pg.2 ln.5-9 : This sentence is rather long. Perhaps consider something along the lines of "We investigate sediment routing patterns in headwater confluences by comparing them to low gradient gravel bed river reaches to characterize how confluences alter the transport of coarse clasts."

We have revised the sentence as follows:

Line 14 We investigated patterns and processes of sediment routing through headwater confluences by comparing them to published results from lower-gradient confluences and by comparing the dispersive behavior of coarse bedload particles between headwater confluence and non-confluence reaches.

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(2). Pg.2 ln.16 : Tail analysis? I imagine you mean of the distribution of particle transport lengths, but at this point in the paper it is not clear what tail analysis means. Please add a couple of extra words of description.

We have revised the text as suggested:

Line 23 Stochastic transport modeling, tail analysis Analysis of the distribution of particle transport lengths, and use of a dimensionless impulse (I*) suggested that transport distance and variance growthvariation in the spatial distribution of coarse sediment particles are was enhanced enhanced by passing through confluences for a given flow strength.

(3). Pg. 3 ln.9: This probably needs another citation earlier than Phillips et al (2013), I would suggest Einstein 1937 especially since the next three references directly build on his work. Phillips et al (2013) could be placed after Haschenburger (2013) in line 14.

We have revised the citations as suggested by incorporating Einstein (1937).

(4). I suggest added additional references here such as Nikora et al., (2002) and Metzler and Klafter (2000) which I think is a good introduction and review into diffusive processes from the physics literature.

We have revised the citations as suggested.

(5). You've already introduced PIT as the shorthand for passive-integrated transponder on pg 5 ln 16. It makes more sense to introduce it here where RFID is mentioned, so you could remove the earlier occurrence without any loss of clarity.

We have revised the text as suggested to avoid repeating ourselves.

(6). This line suggests that all of your particles are larger than the D50, suggest adding a percentage of particles greater than D50 or saying that most particles used were larger.

We have revised the description of the tracer particle sizes in response to comments by both reviewers; see response to Reviewer #1 comment 4.

(7). Figure 3 b, c, d seem to be missing. Adding them in would make the figure larger or require several figures but it would be conceptually useful to see the grain size distributions for reach tracer set with their field site bed grain size. If the control reach is slightly coarser that would support the hypothesis that confluences result in enhanced transport.

This was an error left over from a prior version of Figure 3 that included photos and has been corrected. Grain sizes are similar among study reaches (Table 2); we have therefore chosen not to plot the size distributions for each reach separately.

(8). Pg. 8 ln. 4: I don't think you need to keep reminding people of what PIT means.

We have revised the text as suggested.

(9). Pg. 8 ln. 4-14 : This paragraph could be omitted. It seems adequate to state the manufacturer, tag size, and the maximum read ranges and then refer to a citation where the reader if interested can find more information.

To address this comment we removed the bulk of the paragraph in question and reorganized section 2.2.

(10). Pg. 9 ln. 10-23: Please report the detection range of the loop antennae that you used and manufacturer if known.

We have added identification of the manufacturer where we first refer to the loop antenna. We have also added text clarifying how we determined the height above the bed at which our antenna should be deployed.

(11). Pg. 10 ln. 21 : Here <X/D> is the mean displacement length, which may not necessarily be the step length. Thinking about transport as steps and rests the step length represents (statistically) the average single displacement length from start to stop. A particle may move multiple times during a flood and thus, the resulting displacement length is a sum of an unknown (usually) number of steps.

We agree that “displacement length” is a more accurate term and have replaced “step length” with that here and in numerous other places in the manuscript.

(12). Pg. 11 ln. 20: Metzler and Klafter would be a good reference here. It is also important to consider that if your displacement data were heavy-tailed then rescaling it by the mean (<X/D>) wouldn’t be correct because the longer you waited for the variance or mean of the data wouldn’t converge. You might mention that you have tested the distributions to make sure that the mean is a meaningful parameter.

We agree, but look at it a bit differently. In testing whether the thin-tailed models applied to our data, we rescaled by the mean displacement and sought to determine whether it applied or not. Given the thin-tailed transport models we test, we considered the use of <X/D> to be appropriate.

(13). Pg. 12 ln. 1 : cumulative excess shear velocity rather than shear stress.

Text has been revised as suggested.

(14). Pg. 13 ln. 3: There are many rivers in Mueller et al. (2005), suggest rephrasing to say, Halfmoon creek, a river in Mueller et al. (2005) is similar to. . .

Text has been revised as suggested.

Line 306 The river in Mueller et al.’s studyOne of the study rivers in Mueller et al. (2005), Halfmoon Creek, is similar to our study site, as described above. with respect to channel dimensions, critical discharge, hydrology, elevation, and bed sediment characteristics.

(15). Pg. 14 ln. 8-9 : It would help if you could describe what Parrett and Johnson (2004) is and then cite it rather than just referring to it without context.

Text has been revised as follows:

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Line 159 We estimated the spring 2014 peak flow to have a 3.5 to 4-year recurrence interval, based on used the transducer data, along with Parrett and Johnson (2004) and analysis of a downstream US Geological Survey gauge, to estimate the peak discharge in 2014 as a 3.5 to 4 year event., flood-frequency regression equations developed for western Montana streams (Parrett and Johnson, 2004), and analysis of a downstream US Geological Survey gage.

(16). Pg. 14 ln. 12 : Rather than list the average recovery percentage just give the range reported in table 2.

Text has been revised as follows:

Line 331 We recovered of 7568– to 86% of the seeded tracers, depending on the reach (Table 2).

(17). Figure 9. Dashed line should be labeled as exponential. It would help to add color or make the points larger in size, as it's hard to distinguish between Martin creek lower and upper. While the best fit exponential might not fit (in least-squares) the cloud of data well, the data seem to be straight enough on a semi-log plot that an exponential doesn't look bad at all for describing the overall trend (perhaps because I am having trouble discerning the trend for each reach with the current symbols). This plot does tell us that the data seem to collapse fairly well after normalizing by a single parameter (this provides quite a bit of support for a thin-tailed tracer displacement model, and even the exponential distribution which has a single parameter, except for that single point at far right). It doesn't look like there is any data below 10ˆ-2, so maybe make that the lower bound and it'll be easier to see.

Some of the edits (labeled exponential, adjusted aspect ratio) are straightforward and we agree with their implementation.

It is certainly possible that the thin-tailed model is appropriate here. We note in the manuscript that the models fit the data reasonably well (except for the tails), and that

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power-law fits in Figure 10 suggest that the tracer data lies around the thin-heavy tail threshold. We have revised the text in an effort to more clearly make the points that (1) the control reach is thinner-tailed, and (2) confluences enact generally heavier-tailed distributions of particle transport relative to the control reach, but the enhanced dispersive effects could take place entirely within a thin or heavy-tailed regime as long as confluence particles are traveling further on average and experiencing heavier tails.

See revised manuscript (discussion)

(18). Figure 10. It's not clear to me why starting the tail at 80% (8*10ˆ-1) is correct. Given that each distribution has a substantial break in slope after this point, in the case of the lower martin creek it is close to the 20% mark. In terms of exceedance probability the first 10-20% is the left tail (low transport distances) and 20-80% is the middle of the distribution. Still some of the data may have lower slope than -2 (for reference it would help if a -2 slope were added to the plot), but only barely. This interpretation also seems to conflict with figure 9 in which the data seems to be relatively straight on a semi-log plot, which does not support a heavy-tailed power law distribution. It would also help to have the power-law fits to the tail region where you determine which ones are heavy-tailed or not. This could be done in the same way that Hassan et al. (2013) do their analysis. Something should also be mentioned about how tracer recovery percentage affects the scaling of the tail parameter (see Hassan et al., 2013). If you decide to assess the tail parameter, though, please describe the method that was used. At the moment it is not clear if the data were fit by a power law or a more rigorous approach like the Hill estimator was used.

We think there is some confusion as to what is meant by 80%. We refer to the 80th percentile of tracer transport distances, not the value of 8*10ˆ-1 on the y-axis. We simply tested various slope break locations in the vicinity of 80% and determined that for this analysis, the results (thin vs. heavy tail) were not sensitive to where we started the tail.

As stated earlier, we don't wish to assert that all confluences are heavy-tailed, but rather that in the case of the EFB they are "heavier" than the control reach, indicating a larger proportion of tracers traveling relatively far distances. We consider this important in itself, regardless of whether they all fell within the "thin" or "heavy" tailed distinction – the confluences seem to be associated with heavier tails, in which a greater proportion of tracers travel relatively far distances compared to the mean.

We found amending Figure 10 to show the tail extent, power-law equation, and slope of the exceedance tail to clutter up the figure. We went with an approach that shows the alpha (slope) of the decay next to each creek in the legend. We hope that this, combined with written edits to make clear that we are following Hassan et al. (2013)'s convention, are a satisfactory edit.

See revised manuscript.

(19). Figure 11. Other figures would benefit from a similar color and symbol scheme as used in this figure.

Figures 9 – 10 have been revised to add color and use a similar symbol scheme as Figure 11, though Figure 11 has now been removed – as we decided it muddied, rather than clarified, our results. This resulted in elimination of sections of methods, results, and a figure.

See revised manuscript

(20). Figure 12. Could you label which reaches the points are from? In the text (pg17 ln24) it states that the variance follows a power-law relation, but it seems that a linear line is plotted. Unless the exponent was left off of the equation given. Could you comment on why the relationship for the different populations of tracer particles should fall on the same linear line or why they are related linearly? In phillips and jerolmack (2014) tracer particles for their two field sites fell on two different linear relationships and in order to fit them onto a single curve the frictional resistance of the stream bed

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needed to be accounted for. Does normalizing I* by frictional resistance provide a better collapse of the data?

Labeling the reaches all on the plot makes the figure much more difficult to interpret. We revisited the figure, and have added the correct linear and power-law equations to the figure – there had been a mistake, with an incorrect set of equations shown in the original Figure 12 – we are grateful to have revisited it.

Normalizing did not significantly affect the collapse of our data, but we have mentioned it in Section 3.3.Section 3.3 also has reference to why the linear relationship occurs and what it means.

(21). Pg. 17 ln. 25 : the linear relationship in Lajeunesse et al. (2010) is for average step length against shields stress for constant flow, whereas the results given and those of Phillips and colleagues are for total displacement in unsteady flow. These are likely related, but it has not been shown nor is it obvious to me how they are related. Perhaps leave the Lajeunesse citation out of this line.

We have deleted the Lajeunesse et al. citation here, as suggested.

(22). Pg. 18 ln. 12-15 : It is not clear if these lines are suggesting that upstream geometry and bed discordance are minimal for morphodynamics in general or just for this river (because they are simple and minimal).

We have revised the text to clarify that upstream geometry and bed discordance are important variables in general but not at our sites:

Line 435 . . .reflect the controlling influences of θ and Qr; the simple upstream planform geometry and minimal bed discordance at our sites suggest that these factors have lessexert little influence on confluence morphodynamics in our study confluences.

(23). Pg. 20 ln. 9-12 and parts of the conclusion: If confluences enhance coarse particle transport more than the standard plane bed reach then shouldn't there be enhanced deposition between the upper and lower confluences or is the conceptual

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model hypothesizing that coarse particle transport continues to increase with additional equilibrium confluences? Did you observe enhanced deposition or a coarser bed between the upper and lower confluence sites? The conclusions suggest that the interpretation of the confluences is that they locally enhance transport of coarse material, so in order for mass to balance there should be a noticeably (if the effect is strong) coarser bed between the confluences. If you have the particle size data, it would be interesting to look at this.

The comment is insightful, although we do not have the data to evaluate morphodynamic linkages in a mass balance framework. We have revised the last sentence of the conclusion to suggest that linking tracer studies with measurement of bed texture and elevation would further clarify the issues we examined here. We have also revised other elements of the discussion and conclusion in the context of our responses to reviewer comments to clarify our key points.

See revised manuscript

(24). It is nice to see the tracer displacement data published with the paper and in general, the supplementary data and explanations are well done. Figure S5 should probably be shown with straight lines rather than curved lines. I encourage the authors to upload their tracer data to a digital repository as well (Figshare comes to mind because it is free and provides a citable DOI).

We have uploaded our data to a repository, but it was incorrectly displayed in the original link. It has now been fixed, and the tracer pre and post-flood locations can be accessed.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1509, 2015.

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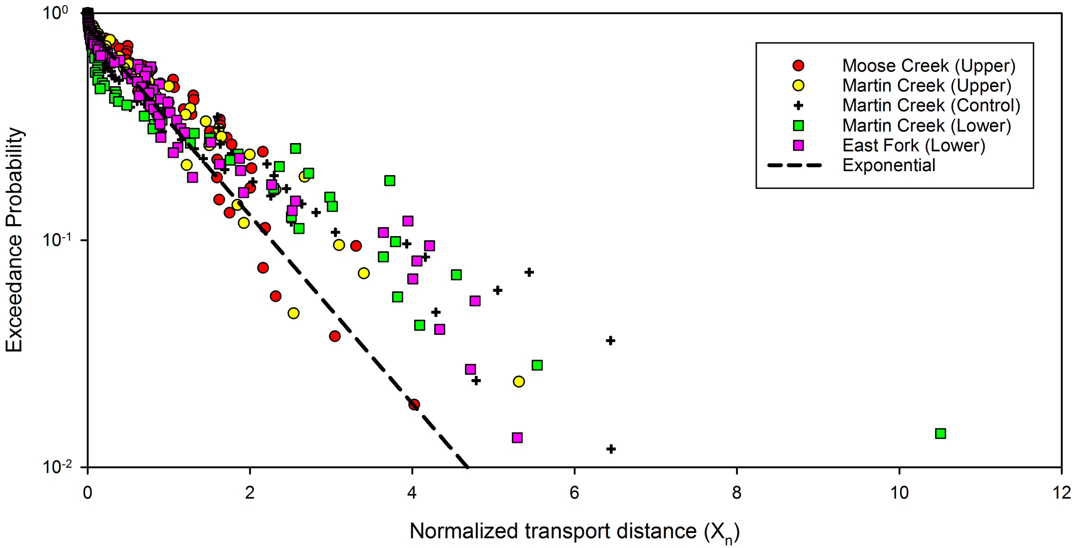

Fig. 1.

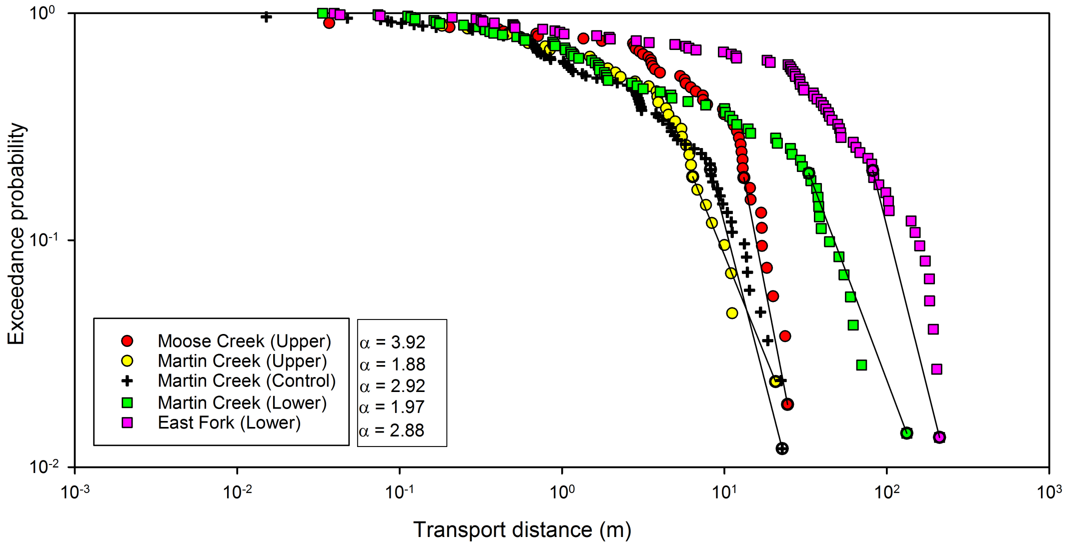

Fig. 2.

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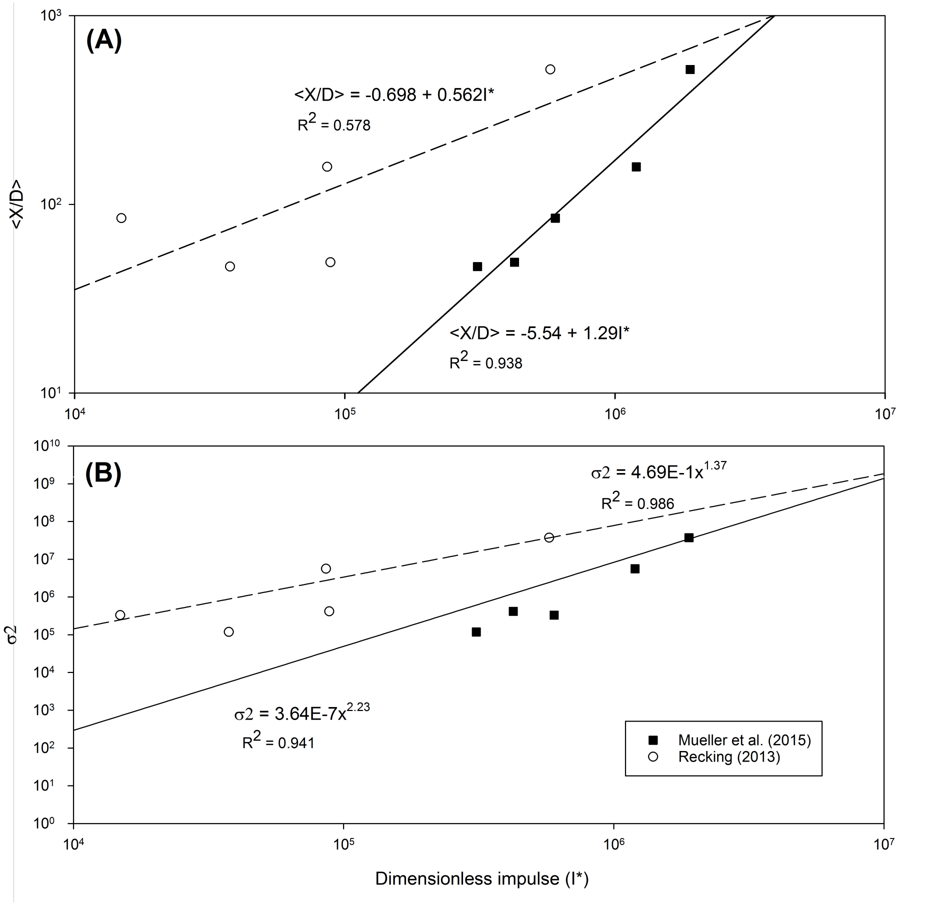
(A)
<X/D> = -0.698 + 0.562I*
R2 = 0.578
<X/D> = -5.54 + 1.29I*
R2 = 0.938
<X/D>
(B)
σ2 = 4.69E-1x1.37
R2 = 0.986
σ2 = 3.64E-7x2.23
R2 = 0.941
Mueller et al. (2015)
Recking (2013)
σ2
Dimensionless impulse (I*)

Fig. 3.