Associate Editor D. Lague, review and recommendation of “Coarse bedload routing and dispersion through tributary confluences” by K. I. Imhoff and A.C. Wilcox

The authors have benefited from two in-depth reviews and they have answered many of the reviewers detailed comments. The reviewers agreed that this work is original and tackles an interesting question with new (and hard to get) data. Looking at grain transport in confluences is a very interesting idea and goes beyond the traditional bias of focusing on “simpler” trunk channels. I concur that this work should be ultimately published in Esurf. While the revised MS is now in better shape, there are unfortunately a number of important issues remaining that prevent publication at this stage. I highlight them below.

One of the main issue is that the authors have failed to answer the reviewer’s general comments in particular those of reviewer #2 which I find where spot on the main weakness of the paper: the statistical treatment of the data, and the fact that the MS’conclusions does not seem fully supported by the data. Let me expand on this:

While both reviewers have emphasized the novelty of the dataset and the scientific interest that studying grain transport in confluences represents, they both have, and in particular reviewer #2, strong reservations on how this dataset is exploited. Given the limited number of confluences and control site studied, the limited number of particles (believe me I understand that these are data really hard to get), and the short duration of the study (~1 flood), it is extremely difficult to draw robust inference from the dataset as to whether confluences significantly modify grain transport or not, in particular the tail of transport distances. In many places, the author’s statistical analysis of the data should be more rigorous (see detailed comments below). Given the data presented, it is not clear that grain dispersion is actually increased in confluences, or that confluences significantly (in a statistical sense) affect grain transport beyond differences in the spatial likelihood of deposition.

As stated by reviewer #2, I think the paper would actually be much better and will have more impact if the discussion of the heavy or thin-tailed nature of the right tails was omitted or at least significantly reduced. Simply because, the dataset (and in particular the limited range in transport distance which is essential to explore tail behaviour) does not permit examining this aspect rigorously enough.

Similar to reviewer #2, the authors should improve and discuss more thoroughly “how the transport statistics within the confluence differ from the control reach, as it currently seems that they are more similar than different” (i.e., fig. 7-8-9-10). In detailed comments below, I highlight some assertions in the text that are not enough supported by the data shown in the figure.

Improving on the statistical analysis of the data, and/or removing some unnecessary parts would strengthen the MS. It might however lead to significantly rewriting the discussion and in particular the conceptual model which assume that confluences affect grain transport while a proper demonstration of this phenomenon has not been given. The authors should close more rapidly their discussion and avoid conceptualizing too much given the data available to do so. Or they should introduce some quantitative modelling of the problem, but I think that would be a completely different paper.

For the above reasons, I recommend major revision with possible re-review.

Detailed comments

#340 – figure 6 : Particles are less frequent in the scour hole, but according to fig 6. they are not more likely deposited on area mapped as depositional bars flanking the channel than elsewhere in the bed. Either you need to change in fig.6 the way you map depositional bars, or you need to rephrase.

section 3.2 : I find that the comparison with EHS and GEM actually weakens the paper. The data dispersion (related in part to the small number and short distances travelled by the particles) make any model fit-able (even a simple power-law would probably fit better most of the distribution, or at least as well as the EHS and GEM). The only thing I gather from fig 7 is that particle displacement distributions are not significantly different in the control section than in the confluences. In the end, if you remove fig 7 and the associated text I don't think the paper's conclusion would be changed.

373- Fig. 8 : the cumulative distribution is an interesting graph, but I think the interpretation of the data is not rigorous enough. In fig. 8a, the presence of a distinct step must also be compared to the initial distribution. For Moose creek I agree that there is an enhanced transport in the confluence (i.e. a steeper part on the cumulative area, but also slightly before (look at the difference in the slope of the cumulative distribution before and after). You also start from a relatively continuous initial distribution which helps in documenting spatial gradient in transport distances. The problem is that Moose Creek is not fundamentally different from the control section (e.g., max transport distance are of the same magnitude, and you also have some small step in the distribution). For Martin Creek, the step in the post-distribution also corresponds to a step in the pre-distribution: if you simply translate the pre curve by about 5 m, you get almost exactly the post-curve. It would indicate no effect of the confluence (or very marginal).

In the end, only Martin Creek, in the lower confluence, seems to exhibit a conclusive evidence of an increased transport efficiency (and potentially dispersion) in the confluence: you start from an initial uniform distribution, and you have clearly a reduced likelihood of deposition within the confluence and an increased one downstream.

All of this is to say, that interpreting these 3 figures (8a,8b,8c), which are very interesting (because they do not entail any a priori model nor specific statistical treatment) and central to the paper deserve more than a simple paragraph.

381- Fig 9: this figure is indeed essential, but the interpretation offered by the author is a bit too simplistic. What it shows, as hinted by reviewer #2, is that the control section does not significantly differs from the lower confluence. One could certainly consider that the main trend is given by an exponential with a lower characteristic slope (not necessarily going through 1 at Xn=0) and that Moose and Martin Creek (upper) have some kind of reduced transport efficiency/ increased trapping efficiency. This is just to highlight that there's nothing conclusive here regarding the effect of confluences and the heavy-tailed/thin-tailed nature of transport given the limited range in normalized transport distances.

Fig 10 : this is probably the most risky and less convincing figure of the paper because the authors are trying to fit log-log tails over less than 20 % of a decade…to be valid, the estimate of alpha should have at minimum an uncertainty, and I suspect that it would be so large that nothing conclusive could be drawn from it…or more precisely, that the tail behaviour of confluences are not statistically different from the main control section at the … % confidence interval. I think fig. 10 could actually be removed (or the fits) as most of the paper's conclusion can be drawn from fig 8 and 9.

#Fig.11 : I have trouble understanding what is the physical meaning of a negative intercept. There is something odd here, as X/D should be equal (or very close) to zero for I*=0. Here again, the lack of error bars on the data and statistical analysis limits the conclusion that can be drawn from the data and weakens the paper.

Discussion:

#465 : you would not need the comparison with EHS and GEM to reach the conclusion that transport distance are likely thin tailed. Fig. 9 gives you this result.

#478: it all depends where you put your exponential fit. Moreover the control section has the same tail behaviour than the lower confluence.

#4.3 : this is interesting conceptually, but probably too far fetched given the uncertainty on the effect of confluences on sediment dispersion and transport. I would consider shorten this part significantly to stick with what the data can really tell.