Reply to Associate Editor's comments

We thank the AE for his comments. Below, we have given detailed replies (in *italics*).

Technical/minor corrections:

Review Mac Vicar
• „Accounting for bin width would in this case add -1 to the exponent, pushing it down to -1.8, similar to what we have observed at the Erlenbach."
> could you explain shortly whether and how you have integrated this in the final paper?
We have removed the relevant figures (Fig. 8 & 9 in the discussion paper), as well as the relevant discussion. We now refer to the Ain River distribution via the analysis made by B. MacVicar and parts of the subsequent discussion. The relevant text in the revised manuscript reads:

> *The observed mass distribution yields a well-defined power-law scaling with a scaling exponent of 1.8 (B. MacVicar, personal communication, 2013), which is similar to the one observed at the Erlenbach (1.84).*

In addition, we have now added a reference to the discussion paper and to the reviewers' comments for additional information.
• It it possible to include the scaling laws suggested by reviewer Bruce MacVicar, the Kolmogorov's (1941), "Dissipation of energy in locally isotropic turbulence in an incompressible viscous liquid" and Paiement-Paradis et al.(2003) "Scalings for large turbulent flow structures in gravel-bed rivers." Even if the analogue is not evident, as you point out, these papers describe two important hypotheses with respect to your scaling law.
>You point out that you assume gravel grinding is a more likely explanation for size reduction; could you shortly point out why - since the scaling law is a key finding of your paper.
As stated in some of the replies to the reviewer comments, we found that about two-thirds of the CPOM is denser than water, and is therefore transported as bedload near the stream bed. Thus, CPOM and gravel bedload are transported in the vicinity of each other and likely interact frequently. Due to the different hardness of organic material and rock, CPOM will probably suffer from these interactions, and will be reduced in size quickly. We have not provided details on this in the present manuscript, as we have a lot more data on this (including additional laboratory experiments) and are planning further studies and experiments. We are planning to publish this elsewhere.
• „10/4 – bimodal assumption seems uncertain. Branches are more likely to break off than whole trees. Scaling would then be affected by distance from source." The Erlenbach features active creep landslide complexes that regularly advect whole trees into the channel. We also expect that scaling is affected by the distance to the source, due to break-down of wood particles and changing channel-hillslope connectivity.
>You point out that channel-hillslope connectictivity is efficient (L238) but I couldn't find a statement on changing channel-hillslope connectictivity? – how does that systematically affect scaling?
The activity of landslides is seasonal, with higher rates of movement during winter and spring than in summer (Schuerch et al., 2006). In addition, qualitative observations suggest that channel-hillslope connectivity of an entire reach can be affected by extreme discharge events (see for example Turowski et al., 2009, Molnar et al., 2010 for discussions pertaining to the Erlenbach). However, in general there are few quantitative data available. The residence time of CPOM pieces in the channel is likely much longer than a single year, and therefore in the export seasonal variations may be averaged out. We have to leave a more detailed assessment to future studies. We did not comment on this explicitly in the paper, but we added a sentence in section 5.4 (More material should be available after severe storms, particularly when they occur in the growth season, or in autumn, when

broadleaf trees lose their leaves). The point is implicit from the context (landslide triggering by storm events is mentioned in a preceding sentence).

Reviewer #2

• P5l21: Please explain more precisely how the flow depth is used for extrapolation. I guess that different sizes of CPOM are transported in different manners, e.g. like bedload for larger particles, and like suspended load for the finer fractions. In this case, simply multiplying the cross sectional area of the trap with a factor to get the CPOM load for the cross sectional area of the stream would work for fines transported in suspension, but not necessarily for CPOM transported in a bedload-like manner. The channel bed of the section where the two traps were positioned was essentially separated in two compartments, the low-flow channel on the orographic right and a gravel bank on the orographic left. We had a trap on each of these two sections, which were assumed to be representative. We divided the transported mass by trap width and multiplied by the width of the relevant section to obtain extrapolated masses.

>I couldn't find this statement in the paper – could you consider implementing your methodological statement shortly since this is important for your measurement bias

We had substantially revised this section in response to the reviewer's comments and now added an additional sentence ('To obtain transport rates over the whole flow width, we divided the transported mass by trap width and multiplied by the width of the relevant section (i.e., the gravel bank or the main channel), and added the values for both sections').

>this also applies to the next statements of reviewer #2.

We are a bit at a loss here. Reviewer #2 made two further statements related to the methods we used. The first of these statements was concerned with the measurement/calculation of the total CPOM volumes sampled in the retention basin. We have already shown in our previous reply that all relevant information is given in the manuscript. We are aware that the information is somewhat scattered (some in the methods section, some in results and analysis), but we believe this is necessary. For example, it is not possible to describe the extrapolation to 0.1g in the methods, since the scaling distribution is necessary to do this. The second of the statements was concerned with the assumed threshold of 5000l/s for the retention basin data. We have reordered the sentences in the revised manuscript, and they now read:

> *Substantial volumes of LWD have been observed in the retention basin after events that exceeded this discharge, whereas only a few LWD pieces have been found in the retention basin after events with lower peaks. Thus, to obtain a representative discharge for the retention basin data points, we assumed that large pieces of wood are dominantly transported at discharges higher than 5,000 l/s, using measurements at 1-min resolution.*

The only further information that could be given are the pictures of the basin included in the reply letters. However, we believe that this is not necessary.

Reviewer Jeff Warburton

• Basket samplers sample the complete flow (for pictures see Rickenmann et al., ESPL 2012). Bedload traps sample the complete water column (that is from the bed to the surface). The sampling efficiency is only determined by sampled minimum and sampled maximum size.

> Problems arising from the two sampling strategies and sampling efficiency are key concerns of the reviewer. I would be happy to see some of the straightforward explanation presented in your author's response in the paper – of course in a concise way since the present description could also be misleading for future readers that do not consult your author's response.

We added the explanations explicitly in the methods section.

Reviewer #4

• L3 p.13. I would say they are not correlated at all. We changed to 'significant correlation'.

>Could you provide a more detailed answer to this?

We used Kendall's tau rank correlation coefficient and R2 from a linear fit to assess significance and stated this now in the manuscript. However, the absence of a significant correlation is evident from the graphic.

• L17-L22 p. 9, there are no references to support your statement here. They should be introduced in the introduction part to support hypothesis tested related to factors controlling scaling factor. These are some very general statements to open the discussion. No changes.

> These "general statements" seem to present state of the art assumptions for your system – however have these been stated and referenced in the introduction?

No. First, we are not aware of any literature on this, and we believe that we are the first authors to make these points in the context of CPOM distributions. Second, the statements are really very general, and, as we believe, trivial. We have not made any additional changes. Below, we quote the commented sentences for the sake of comparison:

> *Various processes in the stream work together to produce the observed mass distribution of CPOM particles from the original mass distribution of organic material supplied to the stream. Coarse particulate organic matter enters the stream either as litter fall directly from the trees or blown in by wind, or via the stream banks either as material advected into the channel by landslides and snow creep, or flushed into it by overland flow.*

The first sentence merely states the distribution is the result of complex interacting processes. The second sentence lists possible ways CPOM may enter the stream. Here, we added a couple references. We do not see why this material needs to go into the introduction.

• L5-7 p. 13 The forest cover in basin is fairly variable between catchments. We would have expected here a potential relationships which is not. One of the key issues is also the representativity of your samples. Can we expect an effect of seasons? type of floods? High event-based variability is not explored or discussed. These are all good points. Forest cover alone is not sufficient, as one would expect the distance of the forest to the stream to play a role. We have extended our description and discussion, and provided some reasoning.

>I could not find out how you addressed event-based variability … - could you shortly comment on this.

We added sentences in the first paragraph of section 5.4 (It seems reasonable to expect CPOM supply to vary in time. More material should be available after severe storms, particularly when they occur in the growth season, or in autumn, when broadleaf trees lose their leaves).

Note that the measured scaling exponent do not show a seasonal trend, although the data are probably not sufficient to make a final assessment. In particular, most of the bedload trap data for low discharges was measured during snow melt, while the basket samples were taken during storms in summer and autumn.