The paper "The mass distribution of coarse particulate organic matter exported from an alpine headwater stream" by Jens Turowski at al. investigates coarse particulate organic matter (CPOM) transport and develops a rating curve of CPOM transport rates with discharge and scaling exponents which can potentially be used in the design of measuring strategies and in natural hazard mitigation. The paper provides important and novel findings which might significantly contribute to various scientific communities operating at the Earth Surface with a hydrological, biological, geological and geomorphological background. The paper is concisely written, the hypotheses are clearly stated and results and discussion are technically well organised and profound. The description of the methodology and a number of shortcomings in the introduction and the discussion have profited significantly from the reviews and are now stated in a less ambiguous way. Below, I have listed a number of suggestions derived from the review which I recommend to address more profoundly prior to publication.

The authors have responded carefully and in a detailed manner to the four reviews and a large number of minor revisions has been implemented in the paper. My only concern is that much of the communication can only be retrieved from the authors response file while many readers will in future only read and reference the paper itself. I know that addressing all reviewers' suggestions is controversial to keeping the paper concise. However, I suggest implementing a number of key comments shortly in the discussion to foster the future debate on the processes that underly the observed scaling issues.

Subsequent to these minor technical corrections, I recommend publication of this highly interesting paper and I expect it will have a significant impact in different communities.

Good luck with the revision

Michael Krautblatter

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?

- 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.

- „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?

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

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

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.

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?

- 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?

- 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.