Review of *Decadal-scale decay of landslide-derived fluvial suspended sediment after Typhoon Morakot* submitted to Earth Surface Dynamics

Ruetenik and co-authors present a very interesting assessment of multi-year suspended sediment flux data from around Taiwan and assess how suspended sediment discharges and its relationship to hydrology is affected by typhoon Morakot. They compare these data with an inventory of landslides that were triggered by the typhoon and make inferences about the timescales of sediment evacuation from landslides after extreme events.

I found this paper to be a very well written and presented contribution that just leaves me with a few suggestions and comments that I invite the authors to consider.

In particular, I wonder about the conclusion that landslides are driving the change in rating curve parameters. You show that many of the catchments have excess sediment yield that is above the landslide yield (by orders of magnitude) – e.g. Figure 8. Does that mean that you are measuring activation of non-landslide parts of the landscape during typhoon Morakot? If you are measuring a substantial proportion of sediment discharge from non-landslide parts of the landscapes, would it be possible that the change in the rating-curve parameters is driven by non-landslide parts of the landscapes just as much as the landslide parts? The correlation with landslide intensity could then be due to a co-variation of Typhoon Morakot intensity with landslide intensity. If you plotted Figure 8 and Figure 10 with, for example, rainfall intensity from Morakot instead of landslide intensity, would you find a similar result?

I suggest to give a bit more space to the impact and the implications of looking specifically at suspended sediment transport. In particular, I wonder about the conclusion that the "periods of elevated sediment transport efficiency after landslides should persist from years to decades" (L26-27). Isn't it possible that bedload transport in larger floods will be elevated for many more years? In regards of the discussion of previously measured sediment evacuation times in L36 – 37: As far as I understand, at least some of the references that are cited look specifically at bedload transport (Croissant et al., 2017; Yanites et al., 2010), so the times of export may be quite different. Finally, on a minor point, when comparing suspended sediment yield and landslide yield, in Figure 8 and in the associated discussion, I presume the landslide yield includes all grain sizes, so should be a bit lower when compared with suspended sediment yields, right?

I wonder about whether north-south changes in lithology could underly some of the observed north-south trends. My intuition is that this effect should be minor and it is also hard to test for, but maybe worth adding a line about lithology somewhere.

Line comments

L8-9: I can imagine that some readers do not have an intuition of what changes in the coefficient and exponent of the rating curve mean in terms of process (or maybe they do not know what these parameters represent). If there is a way to describe the changes in words and/or define the parameters, that might help. (e.g. instead of saying that the coefficient was a

factor of 5 higher, you could say that the suspended sediment transport for a given discharge was a factor of 5 higher etc.)

L17: "Shortly after […]". Sounds like a repetition of information from previous sentences – may be streamlined.

L37: With a brief look at the cited reference, I can only see estimates for evacuation times of 250 – 600 years, not thousands of years. Also, as mentioned above, these are, as far as I understand, estimated for bedload transport, not for suspended sediment transport as suggested in this sentence.

L110: If eight basins show no landsliding due to Morakot – why not use these eight and instead add the other basins to the group that have landslides?

L248 / Figure 4: As far as I understand from the definition in L182, the pre-Morakot values are averaged across the entire period pre Morakot. Here, you have another pre-Morakot value that is just the part of the year 2009 before Morakot (empty circle in Fig. 4) –A different designation for these different measurements would be clearer.

L296: That sentence doesn't work – maybe the "and" needs to go?

L337/Figure 8: By plotting the figure in log-space. You are discarding negative values. I wonder what'd happen to the correlation, if you plot it in linear space? Do the catchments with negative values fall on a similar trend?

L340/Figure9: Can you plot the pre Morakot $\tilde{a}$-values in the figure, for example as a horizontal/shaded underlay? That would be great to see if the values recover to the previous value or even overshoot.

L377 – 78: Given the high uncertainties in Fig 10b, I wonder what the likelihood is that there is no difference in the values at low and high landslide intensity. Would it be possible to add a statistical test here?

L384 – 386: My understanding of Chen et al. (2020) is that by 2016, the ^{10}Be concentrations are basically not statistically distinguishable from before Morakot when considered across all catchments. Of course, there are some catchments that have lower ^{10}Be than before Morakot but there are also some that have higher concentrations. This sentence suggests to me that the effect of Morakot is still clear in 2016.

L395: Here and between figures and the text, you switch between $log(\tilde{a})$ and $ln(\tilde{a})$. I presume that log is the $\log_{10}$ and ln is $\log_e$? Is there a reason for considering the natural logarithm and sometimes the base 10 logarithm?

L407/Figure 11: Given the poor fit in panel c, I wonder if the goodness of fit should somehow come into panels b and d? Also, the "strength of the relationship" sounds a bit like a goodness of fit criterion. Maybe you can find a different wording? For example, "sensitivity of $\Delta log(\tilde{a})$ to landslide intensity.

I hope that the comments are helpful, and I remain with best wishes to the authors and editor.

Aaron Bufe

References

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