Dear Mr. Seitz,

Reading your reply to the reviewers and the changes made in the manuscript I found that there are still some aspects, which need some polishing. Hence, I suggest minor revisions before the paper can be published. Please address the following mostly specific comments.

Best regards

Peter Fiener

Line 190. Omit ‘moreover’ at the beginning of the sentence.

Line 196 and following. Explain/define the terms ‘functional diversity’, ‘functional traits’, ‘species-specific functional traits’, ‘tree species richness’, and ‘tree species identity’ when first used in the text. After this definition use the terms consistently. Personally, I think the term ‘tree species identity’ is somewhat confusing. Why not just using ‘tree species’.

Line 201. ‘Hotspots of biodiversity and woody plants’ does not make sense. There could be ‘hotspots of biodiversity’ but not ‘hotspots of woody plants’.

Line 203. Omit ‘in China’.

Line 205. Replace ‘generally’ with ‘often’

Line 226. The term ‘sediment discharge’ is somewhat misleading as I guess you measure the total sediment delivered per event. A discharge indicates a rate. Therefore, I suggest using ‘surface runoff volume’ and ‘sediment delivery’ throughout the text.

Line 227. ‘Species identities in the leaf litter cover’ is unclear.

Line 227. ‘Leaf species’ is unclear. Do you mean leafs of different species?

Line 218-251. As already indicated by rev#2 I suggest to further shortening this paragraph. Actually, it seemed to be somewhat unstructured.

Line 256. Change ‘are closely’ controlled to ‘can be monitored in detail’.

Line 278-279. Give reference for soil properties.

Line 284-296. This paragraph is somewhat confusing. I suggest omitting the information regarding the 566 plots and just focus on the 34 plots. To make the different settings in the different plots clearer I strongly suggest adding a table presenting the following details: plot no., slope, SOC, …, vegetation properties (see line 337 to 339), species in each plot etc. Do not give any additional information in the appendix as this makes it hard to read.

Line 298. ‘initial sediment…’ is unclear.

Line 300. I assume that the throughfall is highly heterogeneous

Line 311 + 317. Omit ‘(n=170)’

Line 311 ff. Give generally more details how the different variables were determine. It is unclear which variable is determined for the entire plot (e.g. species richness) or for the small 0.4 x 0.4 m erosion plot. If variables like canopy cover, LAI relate to the plot it must be specified how to make sure that a canopy above the small plot was measured. If all these variables were determined for the entire plot (VIPs) it is necessary to discuss this in much more detail in the discussion. E.g. if an average LAIs are determined of the entire VIPs it is clear that the plot internal variability is more important for the individual measurement in the ROPs, than the differences in average LAI between the VIPs.

Line 317-318. SOM cannot be measured in a Vario EL. The elemental analyser determines the total C content, so SOM must be calculated using as ratio of TOC to SOM.

L 322-330. This paragraph is somewhat confusing. (i) Why did you use only four events if the entire year 2013 was measured? (ii) If only the four events were measured at all ROPs I suggest to omit the detailed information regarding the erosive events in 2013, as this is confusing.

Line 327. How did you define events?

Line 327-328. If the Wischmeier and Smith threshold was confirmed by Yin et al. seems to be not important for the presented study.

Line 339-340. Explain the random effects in more detail.

Line 355. Why are there 44 measurements, which are not valid? Explain how this was defined.

Line 359-360. How to extrapolate from four events to a yearly value? (see comment above).

Line 365-367. If these results were not significant, I would not use this argument here and in the following.

Line 374. I would expect also information regarding the monocultures.

Line 378. As indicated earlier I think the term species identity should be changed to species or individual species.

Line 379-381. Just a suggestion to make things clearer: 'Individual tree species in monocultures show significant differences in sediment delivery (Fig. 3) ranging from … to ….

Line 381 ff. What about runoff volume in case of individual species.

Line 393. Give details regarding the measurements of stone and biological curst cover in methods.

Line 395-396. I guess you mean 'Sediment delivery decreased with increasing SOM content'.

Line 411-426. I do not see the relation to the rest of the study. Hence, I suggest omitting this paragraph.

Line 432. Replace 'Whereas' through 'In contrast'

Line 438-439. Not clear from results.

Line 439-440. Somewhat confusing as the monocultures seem to have significant differences.

Line 441-442. I guess that the missing effect of species richness is also a result of the very small plots not representing a variable canopy (see comment regarding methods).

Line 452-454. This is true if there is no understory.

Line 529-530. Speculation.

Line 530-534. Not supported by the presented data. Speculative.

Line 564-566. As there are no significant effects, I suggest omitting this at least in the conclusion.

Line 592 ff. Omit the appendix and integrate information in text (see comment above).