RE: egusphere-2023-1783 (author) - final response "Exotic tree plantations in the Chilean Coastal Range: Balancing effects of discrete disturbances, connectivity and a persistent drought on catchment erosion"

Dear Editorial Team of Earth Surface Dynamics,

Please find below our replies to the referee comments as requested. We thank you for considering our manuscript for publication in Earth Surface Dynamics. We particularly thank the Associate Editor Veerle Vanacker for handling our manuscript. We also express our thanks to Amanda Schmidt and Thomas Hoffmann for their time spent in providing constructive reviews and assessments. We highly appreciate all these efforts. In the following, we reply to the comments.

Response to Reviewers

Referee Comment 1
Amanda Schmidt

This paper looks at the effects of tree plantations on erosion in the coastal range in Chile by comparing suspended sediment concentration to in situ 10Be-derived erosion rates in the context of hillslope connectivity and detailed land use land cover change mapping. Although the authors do not have particularly conclusive results and the methods are not that new, I really like this paper. The data are well explained and the results are interesting. It's a bit puzzling to see so little difference between the two different metrics of erosion and the authors do a good job thinking about why that might be.

I do have a few very minor points that I would like to see the authors clarify.

We thank for the positive feedback. In the revised version of our manuscript we hopefully addressed all your concerns and suggestions where we found them applicable.

1) end of page 14 (around line 307), I got to wondering if storage in the system, like in floodplains or alluvial fans, could be part of the reason for the depressed sediment concentration. That is brought up later, but forecasting it earlier on would make things clearer.

We incorporate storage in this introductory paragraph of discussion. That paragraph now states:

`[...]however we regard the suspended-sediment decadal erosion rate as a very conservative estimate for recent catchment-scale erosion. First, we argue that this estimate does not account for possible effects of sediment storage and other transient processes that affect soils and streams, which is discussed below. Second, previous studies on rivers in the western Andes have indicated that catchment-scale erosion from gauging data may be underestimated due to under-sampling of extreme events (Vanacker et al., 2020; Carretier et al., 2018). It may be the case here, due to the numerous gaps on streamflow and suspended sediments dataset, and the absence of sub-daily or depth-integrated measurements of sediment concentrations. Third, suspended sediments do not record the effects of chemical weathering on denudation rates[...]`

2) The last word on line 307 ("This") is a pronoun that is unclear. I am not sure what "this" is.

We rephrase the related text (see second point above).

3) Is it possible that with the high rates of chemical weathering and likely deep regolith, the 10Be is an underestimate of long-term denudation rates? (see: Campbell, M. K., P. R. Bierman, A. H. Schmidt, R. S. Hernandez, A. Garcia-Moya, L. B. Corbett, A. J. Hidy, H. C. Aguila, A. G. Arruebarrena, G. Balco, D. Dethier, M. Caffee (2022). Cosmogenic nuclide and solute flux data from central Cuba emphasize the importance of both physical and chemical denudation in highly weathered landscapes. Geochronology.)

4) Along the same lines of questioning the 10Be data, is it possible that you have stripped so much soil that these are artificially elevated and don't actually reflect the long-term pre-people rates? (like Hewawasam et al found in Sri Lanka [Hewawasam, Tilak, et al. "Increase of human over natural erosion rates in tropical highlands constrained by cosmogenic nuclides." Geology 31.7 (2003): 597-600.] or Schmidt et al (ESurf) found in China [Schmidt, A. H., Neilson, T. B., Bierman, P. R., Rood, D. H., Ouimet, W. B., and Sosa Gonzalez, V., 2016, Influence of topography and human activity on erosion in Yunnan: Earth Surface Dynamics , v. 4, n. 4, p. 819-830. http://www.earth-surf-dynam.net/4/819/2016/])

I agree that 10Be results may be affected both by intense weathering and the rapid soil erosion. Thanks very much for the literature and the ideas to improve our discussion on the long-term results:

`[...] This long-term rate, however, may even be overestimated: after 200 years of intense soil erosion, deep saprolite layers are widely exposed at Earth Surface. Those hillslopes might be depleted in` ^{10}`Be, which may lead to overestimate the total denudation (Schmidt et al., 2016)[...]`

`[...] total denudation in the short term can be equal or even higher than the reported long-term denudation. Quantitative estimates of chemical weathering and soil production rates could illuminate the magnitude of this difference. Indeed, deep chemical weathering may also lead to underestimating long-term denudation rates with` $^{10}$`Be. That may occur if mass loss is high below several meters, under the depth at which most cosmogenic nuclides are produced, as has been seen in tropical landscapes dominated by dissolution (Campbell et al., 2022)[...]`

5) Is it possible that the hillslopes have been so disturbed that the sediment is totally stripped from them, leaving the hillslopes in a detachment limited system even while the valleys have stored sediment that is transport limited? Given the magnitude of erosion you are talking about, it seems like this could be possible. I could be entirely out to lunch though, not knowing the area, and I do think that intermediate hillslope storage is a reasonable explanation.

This landscape is probably transiting to a detachment limited system. Trees dig into soils where the inorganic component is each time coarser. We added the section 4.4 in the discussion to incorporate this idea.

6) The first two sentences of the conclusions are printed twice.

Corrected

This is a really neat study and I look forward to seeing the final version published.

We appreciate your review and evaluation. Your suggestions are quite pertinent and we hope you find better this version of the manuscript.

Referee Comment 2
Thomas Hoffmann

The authors present an interesting study on the development of stream flow and suspended sediment transport in Chilean headwater systems that are conditioned by changes of multiple drivers (drought, wild wire, tree plantations). This study is relevant and valuable to be published in the journal ESurf. The aim of the study, to extract the effect of tree plantations and wild fires on sediment transport is very challenging. In the end, the discussion of the results is very general and I have the feeling that the authors missed the chance to analyse the data in more detail (see general and detailed comments) and learn more about the specific controls. I made some suggestions to more specific approaches, which might shed more light to the discussion. Overall, I suggest major revisions following my general and detailed comments before publication.

Kind regards, Thomas

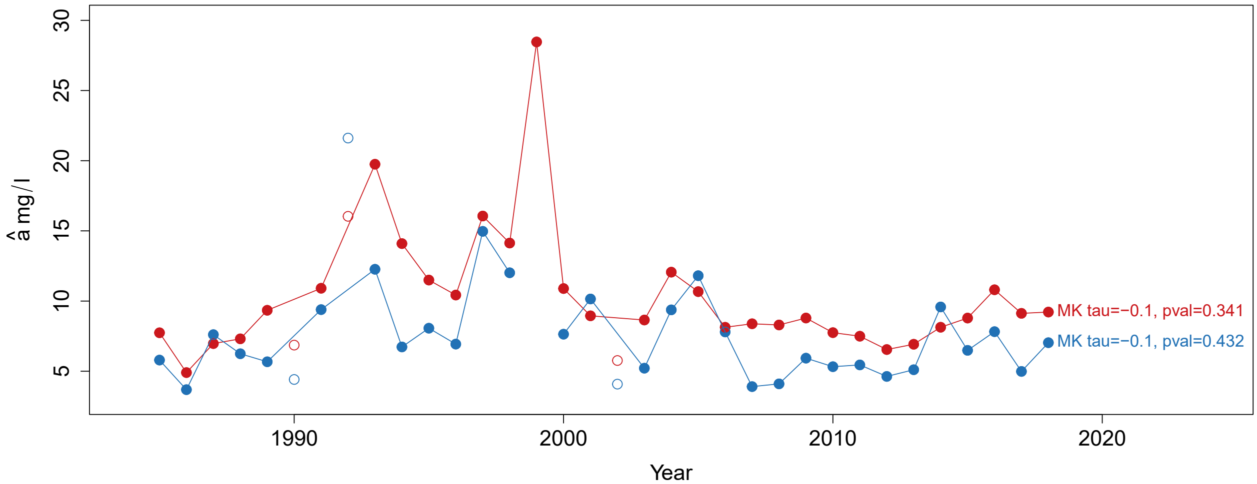
Thanks very much for your detailed review. We improve the discussion and respond here to your specific suggestions on data analysis.

General comments:

Unravelling causes/drivers of changing suspended sediment transport is a challenging task give the multiple interdepended drivers of suspended sediment loads. The authors are correct stating that an unambiguous attribution of cause and effect is difficult to assess. In this study the authors rely their statements mainly on the trend analysis. However, more statistical approaches are available to learn more about the driving factors.

The major issue is that SSD (load) is directly related to Q, since it is part of the calculation of the load. SSC (suspended sediment concentration) is strongly conditions by Q, but is not directly related to its estimation. Changes in sediment supply should therefore be discussed by changes in SSC. To see if changes in SSC are related to changes in Q or by changing sediment supply from hillslope sediment rating analysis could be performed, e.g. SSC = a(Q/Qm)^b) à changes in coefficient "a", which is the suspended sediment supply at Q=Qm should be related to changing supply conditions that are not related to changes in Q (see for instance Warrick 2015, WRR or Hoffmann et al. 2023, ESurf). I suggest to add a trend analysis of the rating parameters, to learn more about changing supplies.

We previously addressed the rating parameters of those data, although without the normalization by the geometric mean (Sauzal station in Figure 3, Tolorza et al., 2019)). Then we knew about the large dispersion of SSC vs Q data in log-log space and we would anticipate a lack of trend for the intercept. Nonetheless, given your suggestion, we calculated the trend of the $\hat{a}$ intercept on $SSC = \hat{a}(Q/Qm)^b$, where Qm is the geometric mean. In the following plot we discard (1) hydrologic years with less than 185 daily data and (2) years where the linear model of log transformed data resulted on pvalue > 0.05. The intercepts plotted in red are calculated for all the records and the intercepts plotted in blue are restricted to Q>Qm:



They do not present trend, according to the Mann-Kendall results. For that reason, we do not include those results in the MS.

Additionally, the observed patterns is superimposed by a declining trend. To extract the effect of single events (wild fire, Earthquake etc.) the authors should focus on the residuals of single years (with and without events) with repsect to these long-term trends.

We kindly thank you for raising this important issue. In fact, we discussed this previously and before submitting our initial manuscript. There are several points we want to emphasize here.
First, the fundamental question is how to estimate the baseline, i.e. the undisturbed state of the hydrological system to compare the measured data against. Here, a data-driven study, as the one we are performing, is limited, because we have to either include the data under 'disturbed' state or need to interpolate for the period of time when the disturbance may have effects. Both options are not adequate here, as both may introduce a bias. For example a regression plausibly (and most likely, too) experiences leverage effects if data from an disturbed state are included. The other option, i.e. interpolating for the questionable period of time, is neither feasible as we cannot exclude any side effects during the questionable time period, such as variable rainfall patterns driving the hydrological response. Accounting for all these limitations, we would need to perform a physics-based modeling exercise to quantify baselines, e.g. following Mohr et al. (2021). While we agree that a baseline level is important and the missing thereof definitely a weakness, we reject the suggestion here. Our study is data-driven and adding additional physics-based modeling exercises is out of the scope of our manuscript.

Detailed comments:

Line 31: rephrase Mediterranean section (35-37.5o S) of the Chilean Coastal Range (CCR)”

Rephrased

Line 33: "slow denudation rates" à typically Mediterranean regions are characterized by high rates of soil erosion and gullying.

I agree that gullying and soil erosion under Mediterranean climate may be very high in terms of mass load, even more under anthropic intervention. However, in this paragraph we refer to the long-term and large-scale process of sculpting the gentle and largely convex hillslopes of (this specific portion of) the CCR. Here the slopes are much more gentle than the same Coastal Range in the Mediterranean latitudes where both the Coastal and the Andean range present their own peaks in denudation, (Carretier et al., 2018; Schaller et al., 2018)). It is expected that soil-mantled landscapes of gentle slopes denudate much more slowly than very steep landscapes dominated by landslides (e.g. Roering et al). This is in agreement with the observed detrital 10Be denudation rates on catchments draining the Andes Cordillera (landscapes dominated by landslides), which are orders of magnitude higher than denudation rates on the CCR (Carretier et al., 2018; Mohr et al., 2022).

Line 34: "secondary native forest" à should be explained

Done:
`[...]Currently, the remnants of secondary native forest (i.e. successional forests growing in areas where the forest cover has at some time in the past been removed) stand on soils as thick as 2 m[...]`

Line 52: "the CCR ranks amongst the highest..." rephrase to "CCR ranks amongst regions with highest forest loss and gains worldwide".

rephrased

Line 55ff: are the statements limited to the "storm and yearly scale"? Furthermore, no other scales are mentioned later on à I suggest to remove the reference to the time scale.

Ok, we remove this reference to the time scale

Line 87: "transport-limited conditions" à this strongly depends on the dominant grain size. Only streams in the humid south of the CCR show transport limited conditions à see and refer to Terweh et al (2019, Geomorphology)

Thanks very much for pointing this study. We now incorporate Terweh et al. (2021 Geomorphology) in the MS, although the related text was rephrased in the introduction.

Line 90: The two time-scales fall out off the box here: Why 10^4 years? à You should motivate the use of the two time-scale. Since 10^4 Yr is related to 10Be, we should explain here why you use 10Be!

Thanks very much for this suggestion. We move this explanation from the method section to the introduction.

Line 135: is there any chance to identify gaps due to ceased stream flow and gaps due to measurement errors/mistakes, defect measurement devices?

We couldn't unambiguously discriminate between ceased and unmeasured streamflow.

Line 149: the use of 10Be in the context of LULCC impacts should be motivated in the introduction

Done

Line 213: The calculation of RC needs more explanation. How did you calculate IC_rs and IC_s? What was the reference point of both calculations? Does IC_s is the connectivity of hillslopes with streams and IC_{rs} the connectivity to either streams or roads, meaning that you treat roads as streams? What is the effect of subtracting both and what are the major assumptions in this approach? Please give more details on your approach. If I understood it correctly, calculating IC_{rs} assumes that the connectivity of roads is 100%, meaning that very sediment that enters a road will immediately enter the stream. Am I correct and is this assumption valid? If yes, what about roads that are not connected to streams or that first go downhill and uphill afterwards? Could you show these effects of using IC_s and IC_{rs} based on a simplified graphic?

We incorporate more details in the description of methods and assumptions.

Line 240ff: Not clear what exact criteria you used to define alpha=0.7. I would be helpful to use ranges of alpha to show confidence intervals of the analysis. The presented results of the main topic (changing suspended sediment loads) is very limited. More detailed description of the results should be given.

Changes in alpha do not produce a relevant change in Mann Kendall results for baseflow.

Line 255ff: This is in line with the comment in Fig. 5: It seems that you calculate the percentiles of annual estimates (as suggested by the units per year). Percentiles are derived from probability distributions, but as far as I understand you only have a single estimate of the annual load of each year. If you use the daily measurements as annual distributions you should highlight this using the correct units (e.g. t/day). Please clarify.

We include those units now in the plot.

Line 257: "Only the lower percentiles of SSD revert the decreasing trend at the end of the time series"?????? I don't see that!!!!!

p 0.25 (and maybe p50) after 2017 is higher than the period before the wildfires. This may explain the high p-value of this specific trend. I recognize this observation may be not clear, then I remove it from the text.

Line 260: How do you see if data are homoscedastic? Homoscedastic data show a normal distribution of residuals with respect to predicted values (e.g. using a linear regression). Why is homoscedasticity important in this context?

We applied the Fligner-Killeen test to evaluate if the variance of pre- and post-fire hydrometric data is or not constant. This is relevant to interpret the result of Wilcoxon-Mann-Whitney test, which is also explained in the method section.

Line 263: post-wild fire SSC/SSD is very low (due to low discharges) and changes of SSC are mainly related to changing discharges. Again, a rating analysis of pre and post-fire SSC Q relationships would be very helpful.

Post-fire $\hat{a}$ is within pre-fire variability, as can be observed in the plot above.

Line 276-277: you should also indicate whether there are places that show reduced connectivity, as the legend in Fig.8c indicates that there are areas with negative RC.

Now it is described the occurrence of negative RC.

Line 280: The threshold is very arbitrary and likely depends on the input data and their resolution. Therefore, it is questionable if the threshold from another study can be used here. If the definition of a "threshold" is necessary, why don't you use the distribution of IC to identify breakpoints.

This threshold is not arbitrary. It was found on a specific landscape by looking for quantitative estimations of connected and disconnected hillslopes. Reporting values over and below this threshold allows future comparisons between those different landscapes.

Line 328: In Figure 6 no mean stream flow is visible. The question is whether means stream flow is a good proxy of the impact of wildfire. Streamflow is strongly related to rainfall. Thus, stream flow should be related to rainfall to see the effects of the wild fire.

In Figure 6 comparisons of medians or means for different hydrologic years depend on the test used (Wilcoxon-Mann-Whitney or Welch t-test, respectively)

Line 335: Whether sediment transport is transport-limited or not depends on the grain size. This should be discussed.

We incorporate more discussion related to topsoil grain size and limitations in transport through time.

Line 338f: "a lack of minimum rainfall intensity required to trigger runoff and soil erosion on hillslopes (Mohr et al., 2013) and/or an increase in the residence time of sediments stored within the valleys is plausible."

Not sure what you want to say?

We rephrased this text

Line 341-354: The discussion is very general and only weakly related to the results of the study. You should discuss the result from the IC here. The IC does not explain any changes of the transport capacity, but only regarding the connectivity between hillslopes and channels! How are the increased connectivity values related to the decreasing SSC?

We incorporate more discussion in the MS

Line 355ff: Again, limited links are drawn to the results of the study.

Discrete disturbances was widely described in the MS. Also, in the introduction the link with this discussion is explicit.

Figure 4: Stream flow and base flow show very similar pattern and similar tau values. Given the difficulties of defining the correct alpha value for stream flow separation and the similarity of the trends, I suggest to use total discharge as a more robust estimate of the discharge.

The decline in water storage is relevant for the discussion. We preserved this result in the MS

Figure 5: Please give more details on the percentiles. Percentiles of what? It seems that you calculate the percentiles of annual estimates. Percentiles are derived from probability distributions, but as far as I understand you only have a single estimate of the annual load of each year. I assume that you used the daily data here. This should somehow represent in the use of the unit. Please clarify.

We changed the units

Figure 6: Unfortunately there are major data gaps in SSC/SSD after the fires.

The fires are in the dry season. Those gaps probably are periods of zero streamflow.

Figure 7: I suggest to represent burned areas as bar and not as "*". The "*" are difficult to see.

Done

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