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Comment on egusphere-2022-616

Anonymous Referee #3

Referee comment on "Reconstructing five decades of sediment export from two glacierized high-alpine catchments in Tyrol, Austria, using nonparametric regression" by Lena Katharina Schmidt et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-616-RC3, 2022

Review of the manuscript (egusphere-2022-616): Reconstructing five decades of sediment export from two glaciated high-alpine catchments in Tyrol, Austria, using nonparametric regression by Lena Katharina Schmidt, Till Francke, Peter Martin Grosse, Christoph Mayer, Axel Bronstert

Summary: In this manuscript, the authors apply quantile regression forest (QRF) to simulate suspended sediment concentration (SSC) at the outlet of two nested glacierized catchments in Upper Ötztal in the Tyrolean Alps, in Austria. As predictors, they use discharge, precipitation and temperature. The QRF model(s) are used to generate long-term (1967-2020, 1974-2020) time series of mean daily SSC and specific annual suspended sediment yield (sSSY), which are later analyzed for trend analysis and point change detection. To identify causality for such trends and abrupt changes, the authors apply the same statistical analysis to the observations of precipitation, temperature, discharge, and mass balance of the two largest glaciers within the study area.

General comments: I think that the aim of the manuscript of understanding the potential of machine learning techniques to model SSC in alpine catchments, including climate variables as predictors, is valuable. However, in my opinion several aspects require substantial revision.

Major revisions: The methodology is not sufficiently explained. The authors should clarify better mainly: (1) the quantile regression forests approach and the selection of the antecedent conditions for the predictors, (2) the procedure followed to fill-in the missing data (how did you compute the correction factors?) as well as (3) to disaggregate the data.

Likewise, the availability of data and their resolution is quite confusing and requires

clarification.

The authors frame some parts of the manuscript in a way that is conceptually questionable and potentially misleading. First, when the authors talk about 'reconstruction of sSSY', they should clarify well that the analysis of sSSY is based solely on simulations of SSC derived with a QRF model. Not only the QRF model cannot reproduce values outside of the range of values of the training dataset, but also the processes of sediment production and transport might have changed over time. Second, given the nature of the model, it is expected that trends and changes in the predictors lead to trends and changes in SSC. Therefore, I suggest that the authors discuss the trend analysis of the predictors before or together with the trend analysis of sSSY .

I think that it would be interesting to quantify the trends and shifts in SSC, to analyse how much the change in sSSY is related to a change in discharge, in SSC or in both. This would allow understanding if the increase in sediment load is due to an increase in transport capacity, in sediment supply or a combination of the two.

In both Validation A and B, models fail to capture the largest SSC values. As discussed by the authors, this is likely related to the inherent limitation of QRF in extrapolating beyond the range of values of the training data. It would be important to quantify the impacts of this limitation on the total suspended sediment yield. I suggest that authors compute the fraction of total suspended sediment yield transported during these 'extreme' days.

It is not clear to me, which is the added value of using P and T? This could be quantified by running the QRF models excluding either precipitation or temperature and evaluating their performances. Likewise, I think that it would be interesting to run the QRF model without discharge. This would contribute to understand the relevance of the predictors and to estimate the potential of using such models in ungauged catchments.

Specific comments:

Ln. 166-171: Which is the resolution of the discharge data? Please, specify.

Ln. 174-176, Ln. 181-183: How did you compute the 'conversion factors'? Over which time period?

Ln. 179: which resolution?

Ln. 234-244: Please, in addition to the reference to Zimmermann et al., 2012 provide clarification for the antecedent predictors.

Ln. 208-2010: How did you disaggregate the data? How did you use the 10-min data? In the gap-filling part?

Ln. 211-214: I find this paragraph confusing. Please, clarify.

Ln. 259-265: Please, move this chapter to chapter 3.1

Ln. 272-274: Does it make sense to first use a model to estimate the SSC data, and later use the modelled SSC to estimate a model? I think that it would be more correct to exclude from the QRF the time steps in which SSC is not available.

Ln. 277-278: Did you train the QRF models on all available data? Please, clarify.

Ln. 281-282: Is this model different from the daily model of Validation A?

Ln. 291: Please, clarify q-weighted.

Ln. 300: Please, clarify equation 2.

Ln. 349-350: I understood that at gauge Vernagt predictors were available at hourly resolution (see ln. 259-260). Do you mean that the predictant, SSC, is daily? Please, clarify better in chapter 3.1. Data availability and resolution is very confusing.

Ln. 369-370: I agree that NSE and BE are quite good, in the context of suspended sediment transport. However, I wonder how much the largest values, which are substantially underestimated by the model, contribute to the total suspended sediment yield. Quantifying this would help assessing the model performance.

Ln. 385-401: Please, move this chapter to the chapter about data (3.1).

Ln. 471: CP is not defined previously.

Ln. 476-477: please, describe more in details the mass balance record.