

In their revised manuscript entitled 'Spatiotemporal denudation rates of the Swabian Alb escarpment (Southwest Germany) dominated by anthropogenic impact, lithology, and base-level lowering', Schaller et al. analyse spatial and temporal patterns of weathering, erosion and denudation rates for the Swabian Alb. The authors have substantially modified the earlier version of the manuscript to take into account the reviewers' comments. Although the current version is a great improvement, I still have some comments, which are listed below.

Methods

An important comment from both reviewers related to the incomplete description of the methods, which made it difficult to follow the individual approaches presented in the study and hindered the reproduction of the analyses. In the revised version, the authors go into sufficient detail and readability has been improved. I have 4 comments here.

Firstly, it is still unclear how weathering and erosion rates are ultimately quantified using TSS and TDS. Section 3.2 gives a general explanation of which parameters are used in the calculation, but the equations themselves are not given. I assume that the equations shown in Figure 1 on the left are used. I suggest either referring to Figure 1 here or listing the equations in Section 3.2 and possibly removing them from Figure 1 (see also comment below).

Secondly, I appreciate the fact that the authors have now introduced three new proxies to quantify the anthropogenic influence on the catchment areas (lines 264-277). However, it is not clear from the description how the values were derived and what these parameters actually mean. Were the values calculated by the authors or taken from existing data sets? What does the connectivity status index (CSI) describe, lateral or downstream connectivity or both? And if the human footprint index (HFI) is given in % and the highest values found in the study region are ~50 %, why are 50 % and not 100 % used in the calculation of anthropogenic impacts (Section 5.1.2)?

Thirdly, I appreciate that the authors have tried to take into account the anthropogenic influence on erosion, weathering and denudation rates. However, I am a little concerned about the approach used. Although the authors clearly point out that the approach should be taken with caution as there is no standard procedure, I think the approach presented corrects in the wrong direction. Lines 465-465 state: '[...] many TSS values have declined by up to 50% in large German rivers (2 000 to 160 000 km²) in the last ~20 years (Hoffmann et al., 2023). Such a decrease in TSS is usually observed in the northern hemisphere due to dams (Dethier et al., 2022).' This statement implies that human influence has led to a reduction in TSS and thus weathering rates. Natural/unmodified rates would therefore be higher. However, the weighting of the anthropogenically corrected rates proposed here (Section 5.1.2) leads to a reduction in the natural rates compared to the measured human-influenced rates. For example, the greater the human influence, the lower the ratio of $CSI_{mean}/100$ and the lower the weighted natural rates. In view of the above statement, shouldn't the correction go in the other direction and instead increase the natural rates with increased human influence?

Fourth, I am still concerned about the approach to examining the relationship between erosion, weathering or denudation rates with topographic, climatic or biotic average catchment parameters (lines 342-345). Although I understand why the authors favour linear regressions over polynomial regressions, several previous studies have shown non-linear relationships between these rates and catchment average parameters. Therefore, instead of calculating the Pearson correlation coefficient, which assumes linear relationships, I suggest calculating the Spearman rank correlation coefficient instead. This measures the strength and direction of two variables

(monotonically increasing or decreasing), but does not assume linearity. I think this approach makes far fewer assumptions about the underlying relationships, but still provides a metric similar to the one presented in the study.

Structure

The readability of the manuscript has been significantly improved compared to the previous version. And I understand that there are different writing and organising styles for a paper. However, I still find the introduction quite complicated, especially the two paragraphs on how denudation, erosion and weathering rates are quantified on different time scales (lines 62-95, Fig. 1). As mentioned earlier, after reading this introduction, I would expect a study that focuses specifically on bridging the gap between denudation rates at different time scales. Instead, much of the results and discussion focus on explaining the spatial variability of denudation rates and identifying parameters that control these rates. This part of the study is not justified in the introduction. It is also not clear from the introduction why the horizontal retreat rates are calculated. Therefore, I would again suggest that the structure of the introduction be better aligned with the content of the rest of the manuscript.

Technical corrections

- Sometimes abbreviations like W or E are not in italics (e.g., lines 172, 279, and more).
- Lines 184-185: "...drainage systems of the Neckar River draining Northward the Rhine River in the Northwest and the Danube River draining to the Southeast." The sentence needs to be grammatically corrected.
- Table 2: What is meant by direction here, the aspect in degrees?
- Lines 411-412: Remove italics.
- Table 3 caption: "Correlations between corrected rates and mean..." What kind of correlations are reported here, what parameters are given in the table? I assume it is the Pearson correlation coefficient?
- Line 473: A 'be' is missing here.
- Line 483: The closing parenthesis after 'Footprint Index' is missing.
- Line 688: Remove single letters in the sentence.