General comment to the handling editor(s) and both reviewers.
We thank Stefanie Tofelde and Richard Ott once more for their reviews of the revised manuscript. The suggested changes are very helpful and we implemented the changes as described in this response to reviews. The specific/detailed reviewer comments are addressed. below. The reviewer's comments are in bold, our replies are in italics, and the changed text is in normal font.

Public justification (visible to the public if the article is accepted and published):
Dear authors,

Many thanks for submitting your revised manuscript. The two original reviewers have now submitted a further round of reviews, which range from minor to major revisions. Both reviewers appreciate the attempt of quantifying the impact of anthropogenic disturbance in the catchments, which I echo, and it is interesting to see that these metrics correlate as strongly or stronger with denudation rates than the impact of lithology or geomorphic metrics. I think this study will be of great interest for those attempting to quantify anthropogenic impacts on erosion and is a good first step along that road.

However, both reviewers feel that more explanation of the different anthropogenic metrics is needed along with further justification of how denudation rates can be corrected to account for anthropogenic impacts. Both reviewers still feel some methodological detail is lacking on the calculation of weathering and erosion rates from TSS and TDS. Reviewer 2 also points out that the introduction is focused on comparing denudation rates across scales. While this is discussed, a lot of the manuscript is focused on spatial differences in denudation rates which could be better emphasised in the introduction.

Overall, I do not feel these add up to major revisions, but all these comments should be carefully addressed. Please respond fully to the comments of both reviewers while preparing your revised manuscript.

Best wishes,
Fiona Clubb (Associate Editor)

Response to reviewer comments RC 1

The authors have done a good job at addressing my review and the manuscript is much improved. I am glad to see that a clear relationship between erosion and weathering with anthropogenic factors could be documented. Below I list minor comments that should be addressed before publication.

In the initial review, I asked what kind of mineral dissolution was assumed. I tried to follow what the authors did in the excel sheets, but it wasn't obvious to me (an equation in the manuscript would really help). I got the impression that all the cations and anions were summed to get the total dissolved solids, which were multiplied by discharge. But does that include HCO3-? Because if CaCO3 is dissolved, HCO3- is partially derived from the water and the CO2 in the atmosphere, which is why I asked about assumed mineral compositions in the first place. I might be wrong and confused here, but would be nice to clarify.

We thank the reviewer for their attention to detail on this and for helping us catch this. We (finally) realized that one important correction was missing (HCO3- from the atmosphere). We have now accounted for this and updated the text to make the corrections done clearer. Values corrected in the text, figure 3, 4, 5, 6, and 7 as well as Table 1, 2 and 3. The above modifications do not change the story/interpretation of the paper, but did make a significant enough change to warrant updating rates, figures and tables.

The authors recalculate their rates in an attempt to account for anthropogenic impact on erosion/weathering. However, the equations used for this calculation are only shown in the discussion. They should be either stated in the methods, or the explanation of the anthropogenic disturbance correction should be removed from the methods and only mentioned in the discussion. After only reading the methods, and therefore not knowing the equations, I spent a considerable amount of time looking at the data table, and trying to figure out what was done.

Our apologize for the unclarity and the time spent figuring out what we did. We found the source of confusion. The anthropogenic corrected rates are presented in Table S12, which we referenced in the methods section. However, the calculations shown in Table S12 are not referred to again, or used, until the discussion section 5.1.2 where we do explain how the correction is applied. There is no need for us to reference table S12 in the methods, so we have removed this. The methods section reads now as:" Anthropogenic impacts on each catchment were evaluated using three different main approaches. First, the connectivity status index (CSI) for river systems was evaluated for each catchment, where values of 100% represent undisturbed rivers (Grill et al., 2019). Second, the human footprint index (HFI) was extracted with the highest impact to be 50 and the lowest 0 (Mu et al., 2022). This HFI represents the relative anthropogenic influence in each terrestrial biome and is represented as a percentage. Third, the % of different landcover such as artificial surfaces and constructions as well as cultivated area, vineyard or tree covers were extracted from the landcover map of Europe 2017 (Malinowski et al., 2020). The third metric used for anthropogenic impact is the % area of artificial constructions and surfaces. ".

I was surprised to see that the authors attempted to correct for the anthropogenic impact on erosion/weathering rates. As the authors state themselves, there is no established way of doing this and the effects of agriculture could be non-linear. The authors state several times how uncertain these correction with relatively arbitrary equations are. The

authors use these corrections to argue that the Neckar tributaries erode faster than the Danube tributaries even after correction. Wouldn't it be more straightforward to show boxplots or state the values of average HFI, cultivated area, CSI etc for Neckar and Danube side. If the distributions of human influence are similar in both drainage basins, one can make the argument that the difference in erosion/weathering is due to natural factors.

The average distributions of human influence are the following for the Neckar and the Danube side:

CSI: Neckar 99.16%; Danube 99.54% (similar in both catchments)

HFI: Neckar 29.59%; Danube 25.19% (different between the catchments)

Artificial area: Neckar 9.7%, Danube 5.2% (different between the catchments)

So the argument cannot be made that the correction would affect both sides of the Swabian Alb in the same way and the differences in rates are due to natural factors. Therefore, we decided to apply a correction to each rate for the specific catchment. However, as the review notes – we very cautiously do this and every time we mention this correction we have clear text saying how to do this correctly is unknown and that we are only working with an estimate here.

Response to reviewer comments RC 2

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).

L291-292: Reference to Fig. 1 has been added to the text to clarify how the weathering rate is calculated. In addition, we refer to Table S2 (formerly Table S6A) to improve the clarity of the weathering rate calculation.

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)?

There was some unclarity in the method section as already stated by reviewer 1. We hope that the adjustments made to improve the clarity of the manuscript for the reader. We further reference back in the discussion section to the method section where CSI, HFI, and % area of artificial constructions are described and references provided.

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 CSImean/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?

We appreciate the reviewer's thoughtful engagement with our correction approach and agree that the 'direction' of correction for anthropogenic impact is not straightforward. As noted, human activities can have contrasting effects on sediment and solute yields depending on the specific processes at play. While damming and different forms of river regulation have indeed led to widespread reductions in total suspended sediment (TSS) in many large rivers, our study focuses on smaller, upland catchments where anthropogenic activities such as agriculture, urbanization, deforestation, and mining are more likely to ***enhance*** *rather than suppress erosion and weathering rates. In this context, we assume that human impact tends to elevate measured denudation, erosion, and weathering rates beyond what would occur under more natural, less disturbed conditions. Consequently, we applied a correction that* ***reduces*** *the observed (human-influenced) rates proportionally to the magnitude of anthropogenic disturbance.*

We fully acknowledge, however, that this assumption may not hold in all cases — particularly in highly regulated lowland rivers or where sediment trapping dominates. We have revised Section 5.1.2 to clarify the rationale behind our chosen correction direction and have added a stronger caveat that this approach may not capture all facets of human impact. We also highlight in the revised text that future studies should consider process-based corrections that can distinguish between erosive and sediment-trapping anthropogenic effects.

In summary, while the reviewer raises a valid point, we believe that for the specific geomorphic setting of our study — relatively small, mountainous catchments with evidence of anthropogenically elevated sediment and solute fluxes — our correction logic is appropriate. Nonetheless, we agree that this issue deserves more attention.

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.

L342-345: The reviewer is right that the Spearman rank correlation coefficient does not assume a certain relationship. However, it should be noted that both metrics (Pearson vs. Spearman) produce a similar result in terms of the direction of the correlation, or no correlation (e.g., positively, negatively, or no correlation). To address the reviewer's concern we have calculated the Spearman correlation for reference and show it in the table below, and also show the difference between the two approaches. It should be noted that a) the direction of the correlation calculated is the same in almost all cases, and b) the correlations are almost all weakly correlated (e.g., << 0.5 or -0.5) and support our original interpretation of weak to moderate correlations between denudation rates and the various metrics analysed. As this additional analysis does not change our interpretations or conclusions, we have left our analysis in the text to discuss the Pearson correlation relationship. We do this because it provides an evaluation of the potential linearity in the relationships analysed, which we find useful to report.

Metric	Pearson all data			Spearman all data			Difference Spearman-Pearso		
	$W_{corr.}$	$E_{corr.}$	$D_{corr.}$	$W_{corr.}$	$E_{corr.}$	$D_{corr.}$	$W_{corr.}$	$E_{corr.}$	$D_{corr.}$
	n=43	n=43	n=43	n=43	n=43	n=43	n=43	n=43	n=43
Catchment area	0.03	0.05	0.07	-0.01	0.41	0.11	-0.04	0.36	0.04
Mean elevation	-0.17	-0.15	-0.27	-0.16	-0.32	-0.30	0.02	-0.17	-0.03
Max. relief	0.07	0.44	0.41	0.27	0.71	0.46	0.20	0.28	0.05
Local relief (1000m)	-0.09	0.22	0.09	0.09	0.26	0.11	0.18	0.03	0.02
Trunk mean_k_{sn}	-0.22	0.20	0.01	-0.13	0.30	0.03	0.08	0.11	0.03
Slope	-0.07	0.21	0.09	0.13	0.32	0.17	0.20	0.11	0.08
Mean annual precipitation	-0.40	-0.24	-0.40	-0.41	-0.05	-0.38	-0.01	0.19	0.02
Mean annual temperature	0.22	0.17	0.31	0.31	0.30	0.42	0.09	0.13	0.12
NDVI (Vegetation cover)	-0.04	-0.02	-0.03	0.02	0.27	0.10	0.06	0.29	0.13
Soil depth	0.20	-0.07	0.11	0.10	0.02	0.13	-0.10	0.09	0.01
Connectivity Status Index	-0.20	-0.55	-0.52	-0.38	-0.41	-0.50	-0.18	0.14	0.02
Human Footprint Index	0.49	0.26	0.52	0.52	0.25	0.53	0.02	-0.01	0.01
Artificial constructions	0.52	0.52	0.66	0.55	0.38	0.54	0.03	-0.14	-0.12
Cultivated area/vineyards	0.22	-0.07	0.11	0.18	-0.10	0.16	-0.04	-0.03	0.06
LowerTriassic	-0.43	-0.11	-0.32	-0.40	-0.03	-0.35	0.04	0.07	-0.04
MiddleTriassic	0.13	-0.10	0.01	0.26	0.16	0.22	0.13	0.25	0.21
UpperTriassic	0.48	0.13	0.35	0.72	0.53	0.67	0.24	0.41	0.32
LowerJurassic	0.62	0.44	0.62	0.65	0.71	0.75	0.02	0.27	0.13
MiddleJurassic	0.41	0.33	0.43	0.52	0.45	0.55	0.11	0.12	0.12
UpperJurassic	-0.31	-0.14	-0.28	-0.12	-0.12	-0.11	0.19	0.02	0.17
Tertiary	-0.27	-0.18	-0.26	-0.32	-0.14	-0.31	-0.05	0.04	-0.04
Quaternary	0.10	0.09	0.15	0.33	0.22	0.38	0.23	0.13	0.23

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.

We struggle to understand why the reviewer interprets this from the introduction. The other reviewer hasn't highlighted this, and none of the co-authors see the manuscript as an attempt to bridge between timescales. The words "bridge" or "bridging" are never used in the manuscript. The current text in the introduction simply highlights that conventional approaches used are sensitive to different time scales (i.e., truth in advertising). To accommodate the reviewer's confusion, we have added a sentence in the introduction stating that there is currently no way to bridge timescales between these different metrics, but we mention it there is a timescale sensitivity in the methods used. We've also added a sentence in the introduction (last paragraph) why horizontal retreat rates are calculated.

Technical corrections

• Sometimes abbreviations like W or E are not in italics (e.g., lines 172, 279, and more).
Further abbreviations not in italics were found and corrected for.

• 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.
L1884-185: The sentence reads now as: "… the Neckar River draining Northward into the Rhine River in the Northwest and the Danube River draining to the Southeast."

• Table 2: What is meant by direction here, the aspect in degrees?
Yes, the aspect is meant. Direction has been replaced in Table 2 and Table S14.

• Lines 411-412: Remove italics.
L411-412: Italics have been removed. Thank you.

• 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?
Yes, correct. Pearsons correlation coefficient has been added to the table caption:

• Line 473: A 'be' is missing here.
L473: The missing "be" has been added.

• Line 483: The closing parenthesis after 'Footprint Index' is missing.
L483: Missing *part of parenthesis has been added.*

• Line 688: Remove single letters in the sentence.
L688: Singel letters have been removed.