Identification of erosion hotspots and scale-dependent runoff controls on sediment transport in an agricultural catchment

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We sincerely thank the reviewer for their thoughtful and constructive feedback on our manuscript. We have fully addressed all the comments and propose revisions accordingly, as detailed below. Referee comments are shown in black, author replies are in blue. We believe these updates have resolved all the issues and look forward to further feedback from the editor and reviewer.

Kind regards,

Christopher Thoma and co-authors.

The manuscript presents an experimental analysis of the factor contributing to runoff, erosion and sediment load, at the small (66 ha) headwater catchment scale. The results suggest that considering both catchment structural connectivity and crop type (erosive vs non-erosive) is needed to assess the effect of management pratices on sediment load and peak flow.

The assessment of sediment source and field-to-stream connectivity at the catchment scale is a current research question. The additional effects of agricultural conservation practices on water and sediment dynamics at the catchment scale is an additional interesting and relevant scientific question. The studied catchment presents a high-quality database of traditional hydrological gauging stations, including high-frequency rainfall, runoff, streamflow and tile drainage monitoring of water and sediment load.

However, the manuscript presents major issues that preclude publication.

Below, we outline how we address all the issues raised by the reviewer.

1.) First, calculations are hard or not possible to understand. Particularly, the assessment of sediment load values, a central point in this study, is unclear. Was the turbidity-sediment concentration rating curve of good quality?

In our study, suspended sediment concentration (SSC) was derived by calibrating high-frequency turbidity measurements (FNU) against SSC values obtained from laboratory analyses of ISCO water samples collected during hydrological events. This calibration was performed separately for each station using paired turbidity–SSC data spanning a wide range of hydrological conditions. The turbidity–SSC relationship showed a strong and consistent fit across all events and sites ($R^2 = 0.86$ at site E2 and $R^2 = 0.98$ at MW).

Following this calibration, turbidity values were converted into SSC values in g/L, and the complete turbidity time series was thus expressed in sediment concentration units. Sediment loads were then calculated at 5-minute intervals using the formula:

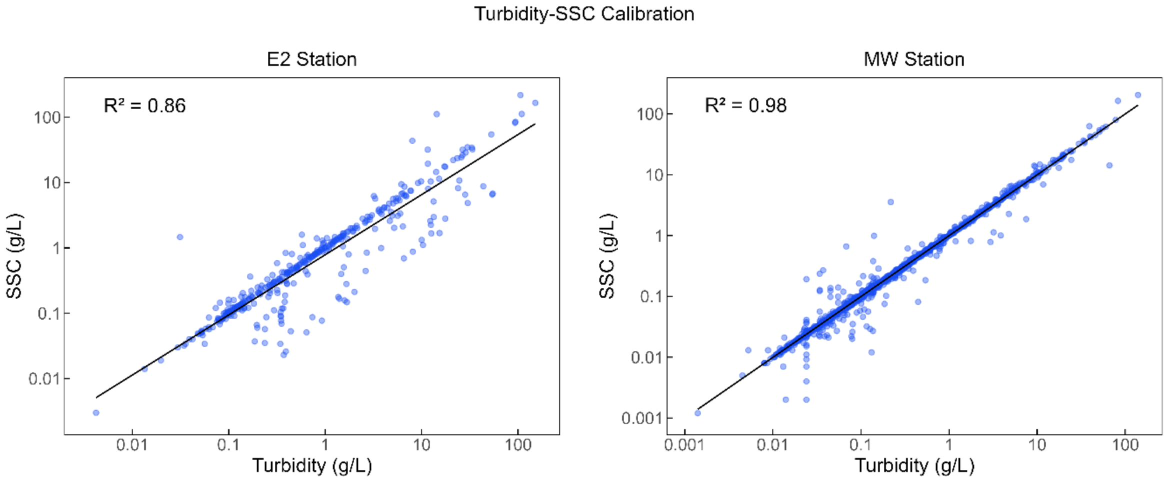
$$\text{Sediment Load}\left(\frac{\text{g}}{\text{s}}\right) = \text{SSC}\left(\frac{\text{g}}{\text{L}}\right) \cdot \text{Q}\left(\frac{\text{L}}{\text{s}}\right)$$

These instantaneous loads were integrated over the duration of each event to derive total event-based sediment loads:

$$\text{Event Sediment Load} = \sum(\text{SSC} \cdot \text{Q} \cdot \Delta\text{t})$$

where Δt is the 5-minute time step.

We will clarify this methodology in the revised Methods section and include the turbidity–SSC calibration plots below for both stations in the Methodology or Appendix. We will also discuss the quality of the rating curves.



2.) The authors alternatively used turbidity and sediment load values in the analysis, but what is the point in analysing turbidity if sediment load values were available? Evaluating the robustness of the results is therefore not possible.

We agree that the use of both turbidity and sediment load values in the analysis may have introduced redundancy. We will remove all turbidity analyses.

3.) The methodology used for analysis is unclear. From my understanding, the authors chose to focus on peak values for flow and sediment/turbidity, which is surprising. To analyse the catchment dynamics, why not study the event-scale water volume and sediment load?

Yes, this is a very valid point. While, originally, our focus was on the peak values because of the larger number of sediment measurements around the peak, we have now changed the analysis to focus on the event-scale water volume and event-scale sediment load. In the revised paper we will back calculate the sediment concentrations for all time steps within the event based on turbidity-sediment concentration relationships and, where turbidity measurements are missing, based on discharge sediment concentration relationships from similar events or other time steps of the same event. This will allow us to estimate the complete event-based sediment loads and runoff volumes for all event.

4.) How did the authors account for hysteresis effects? How was the noise on turbidity values processed? Both may have significant implications for the robustness of the results, particularly considering the significant scattering presented in the log-log plots (Figure 4).

Hysteresis effects: The temporal dynamics of SSC, including any hysteresis effects, between discharge (Q) and SSC during hydrological events, are captured in our data because of the continuous turbidity measurement and numerous sediment samples available. The procedure will be described in more detail in the Methods section and we will also discuss the hysteresis effects in the revised paper.

Processing noise on turbidity values: Turbidity was measured using ViSolid700 IQ sensors installed at both locations (E2 and MW) in the HOAL catchment. These sensors operate on the nephelometric principle according to EN ISO 7027, using infrared light scattered at 90°, which means that colour discoloration typically does not cause any interferences. Ultrasonic cleaning continuously ensures that the two glass windows – located at the bottom of the sensor – remain clean. This reduces long-term drift and noise contributions caused by contamination. The accuracy of the sensor is < 1% process variation coefficient. Repeatability is < 0.015%, i.e., there is very little measurement noise per measured value. In addition, the resolution automatically adjusts to the measuring range. At low turbidity values, ambient light or scattered light from walls can influence the measured value. For this reason, we must maintain a minimum distance of at least 10 cm from the water bottom or walls in order to minimize interference signals caused by reflections. For E2, this distance is approximately 17 cm, and for MW it is slightly more. The turbidity sensor is connected to a DIQ/S 182 WTW digital measuring and control device, which transmits the turbidity values directly to the logger. During data processing, the sensor data were aligned with laboratory reference values. These laboratory values are obtained either during events via automatic samplers or from weekly manual grab samples. Quantitative estimates of the measurement uncertainty will be included in the Methods section of the paper.

5.) The land use classification, which serves as a basis for analysis, is questionable. Defining winter wheat and winter barley as 'non-erosive' crops would require a strong justification, particularly in a study addressing the runoff event scale. What about the intra-annual variations of crops growing?

The classification of crops into erosive and non-erosive categories was based on the Austrian Agricultural Environmental Programme (ÖPUL) and the national Soil Erosion Evaluation Report (BML, 2021). These documents report that significantly lower average soil erosion rates were observed in regions with a higher proportion of non-erosive crops compared to erosive crops, particularly in our study region of Lower Austria. It is true that this binary classification may not fully capture the intra-annual variability in erosion risk at the event scale. The approach reflects a trade-off between classification granularity and the clarity and comparability of the analysis. To address this, we will clarify this limitation in the discussion section and emphasize that erosion risk may vary within crop types depending on phenological stage at the time of the event. We will also provide more detailed evidence on the erosion rates for the classification chosen.

6.) What about agricultural practices, e.g. storm event occurring on ploughed fields vs crusted fields? It is questionable to propose general results such as those proposed in this manuscript without combining the analysis of both soil surface and rainfall dynamics.

We did analyse the influence of agricultural practices, but we did not find any statistically significant effects on either runoff or sediment load. For this reason, the results were not included in the original manuscript. We will include the results of this analysis in the Appendix with a brief discussion in the main text.

7.) Moreover, it is unclear how the authors labelled the different areas (A, B, C, GW9) as 'erosive' or 'non-erosive' (e.g. in Table 2 and 4), considering that these areas included a mix of erosive and non-erosive crops (e.g. Area A in Figure 2). It is therefore not possible to assess if the main results are supported by the data.

Areas A and B consist of more than one field and thus occasionally contain a mix of erosive and non-erosive crops. The areas GW9 and C—which are central to the study's conclusions—comprise a single field with homogeneous cultivation classification during each event.

For analytical clarity and to enable statistically robust comparisons, we categorized areas A and B based on the dominant crop type at a given event: If >50% of the area was covered by non-erosive, the area was labelled as "non-erosive"; otherwise, if >50% of the area was covered by erosive crops it was considered as "erosive". This classification method was applied consistently across all events in the study period (2012–2022). We will clarify this classification approach in the Methods section. We will also acknowledge this limitation in the Discussion section of the revised manuscript, noting that this simplification may underestimate heterogeneity within areas, but was considered appropriate to facilitate spatially explicit, cross-scale analysis of erosion dynamics.

8.) Last, the main message of the manuscript, as indicated in the abstract and conclusion, i.e. the need to consider both cultivation practices and catchment connectivity, lacks novelty, particularly considering that only part of the catchment structural connectivity was considered in the analysis.

As highlighted by Vanmaercke et al. (2017), there remains a critical lack of long-term, high-frequency monitoring studies that can evaluate the effectiveness of erosion control measures across spatial scales. While conservation practices have been widely assessed at the hillslope scale, their effectiveness at the catchment scale remains poorly constrained—particularly regarding how overland flow and sediment redistribution shape in-stream sediment loads (Doody et al., 2017, Van Oost et al., 2007; Verstraeten et al., 2002). This gap limits our ability to assess conservation outcomes at early transport stages, as long-term studies linking surface runoff contributions to catchment-scale sediment connectivity remain scarce despite advances in erosion monitoring. Our study directly addresses this gap by using a high-resolution, multi-station dataset from the HOAL catchment, including rainfall, streamflow, tile drainage, and sediment monitoring at event scale over an entire decade. We explicitly account for area-specific characteristics such as slope, proximity to the stream network, catchment connectivity and catchment area specific cultivation and tillage practices. This allows us to move beyond previous work by empirically quantifying both sediment source dynamics and field-to-stream connectivity at the catchment scale—addressing a key research need for multi-scale sediment transport assessments identified by Vanmaercke et al. (2017). We have now sharpened the conclusions to better bring out the novelty of the research.

9.) As a conclusion, given the lack of novelty, the unclear analysis procedure, and the inability to assess whether the results support the claims presented, I would not recommend publication in HESS.

We have carefully addressed all general and specific comments and have clearly outlined how we will revise the manuscript to improve clarity, strengthen the analysis, and better highlight the novel contributions of our study.

Specific comments

10.) The title is misleading: 'identifying erosion hotspots' would require dedicated studies using e.g. sediment tracing and/or distributed modelling.

We agree that the term "identifying erosion hotspots" may imply the use of sediment fingerprinting or distributed modelling approaches, which were not applied in this study. To avoid any misunderstanding regarding the methods used, we will revise the title of our study, also considering the views of the other reviewers.

11.) Figure 2 is hardly readable, please consider bringing monitoring stations to the foreground and / or to increase dots size. It may help readers to use intuitive names for the monitoring stations over the manuscript, i.e. what is the difference between 'Sys' and 'Frau'? Would it be relevant to change these names for TD (Tile Drain) and other monitoring stations for e.g. S (Streamflow), R (Runoff)… ?

We will increase the dot size of Figure 2, enhance the contrast, and bring the monitoring stations to the foreground to improve readability.

Regarding the station naming, we understand the reviewer's concern. However, we would retain the original station abbreviations (e.g., "Sys4", "Frau2") because they are consistent with long-term monitoring at the HOAL and are widely used in previous publications from this catchment. To improve clarity for readers unfamiliar with these abbreviations, we will add a short explanation of each station's dominant flow pathway (e.g., tile-drainage, streamflow) to the figure caption and re-emphasize this in the Methods section.

12.) It is misleading to provides Pearson's r on a scatterplot including regression lines. It is suggested to add correlation coefficients to the correlation matrices, and to indicate determination coefficients in the figures.

Since we now include total event runoff volume and total event sediment load directly in the revised manuscript, this plot will no longer be part of the revised manuscript.

13.) Table 3: It is surprising that the peak flow is not significantly affected by tile drainage. Literature underlines the importance of tile drainage in modulating peak flow.

We completely agree with reviewer that tile-drainages play a key role in modulating peak flow. In fact, we were highlighting the modulation of peak flow by tile-drainages in lines 94-96 in our Introduction section, and in lines 596—602 in our Discussion section. We also discussed the dilution of peak turbidity at the catchment-scale due to tile drainage contributions compared to the hillslope-scale.

14.) l.493-497: It is not clear how a correlation coefficient can be used to deduce that 'rainfall erosivity exerts a dominant control over hydrological and sediment transport mechanisms'. It is also surprising that the correlation between EI30 and the sediment dynamics is better at the catchment scale than at the plot scale, while increasing scale should results in a higher complexity.

We agree that correlation alone does not prove causality or "dominant control" in a mechanistic sense. We will revise the sentence in line 493–497 to better reflect the descriptive, rather than causal, nature of the observed relationship.

15.) It is also surprising that the correlation between EI30 and the sediment dynamics is better at the catchment scale than at the plot scale, while increasing scale should results in a higher complexity.

We thank the reviewer for this insightful observation. Indeed, it may seem counterintuitive at first that the EI_{30}–sediment dynamics relationship improves with increasing scale, given the expectation of added complexity. However, as discussed in Sivapalan (2003), increasing spatial scale can lead to emergent simplicity. That is, aggregation of hillslope processes can filter out localized variability and highlight dominant controls (such as rainfall erosivity or connectivity patterns), which results in stronger, more stable correlations at the catchment scale–as also observed in our analysis. We will add this perspective and cite Sivapalan (2003) in the revised manuscript to clarify this point.

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