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

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We sincerely thank you for your thoughtful and constructive assessment of our manuscript. We greatly appreciate your positive comments regarding the relevance of our study, the quality of the writing, and the clarity of our figures.

We have carefully addressed all the major and minor comments and propose revisions accordingly, as detailed below. Referee comments are shown in black, author replies are in blue. We believe these updates have strengthened the manuscript and clarified the points raised.

Kind regards,

Christopher Thoma and co-authors.

The text is sound and can be accepted for publication at HESS. It tackles an interesting and relevant topic (erosion hotspots and their relation to runoff and sediment transport) and uses primary data for the analysis. the text is well written and the figures are meaningful. Despite its merits, the manuscript needs some improvement before publication.

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

Major review.

1) Lines 117-119. You mention "sediment load change across spatial scales" as one of your aims. However, the experimental catchment and the stream reach are small (66 ha and 620 m, respectively). Do you consider that this experimental setup allows you to investigate different "spatial scales"?

Thank you for this valuable comment. We agree that the overall spatial extent of the HOAL catchment (66 ha) is small compared to large river basin studies. Our intention with the term “spatial scales” was not to imply regional or basin-scale variability, but rather to analyse sediment transport dynamics across different hydrological source areas within the catchment — specifically, (i) the hillslope-scale at E2 (45 ha), and (ii) the catchment-scale at MW (66 ha). We will clarify this by explicitly referring to "hillslope-scale versus catchment-scale" instead of the more general term “spatial scales” and revise our research question accordingly. This distinction is still meaningful as flow pathways differ substantially between these monitored locations. E2 only consists of overland flow at the hillslope-scale, whereas the catchment outlet MW aggregates multiple flow pathways such as perennial and intermittent spring discharges, perennial and intermittent tile-drainage systems, and wetlands as described in lines 148-175 of the manuscript.

2) 3. Methods. A table with the data availability (e.g., number of samples, monitoring period, method...) would be desirable. Besides, neither a table, nor a figure with the events' characteristics is presented, which hampers the readership possibility of analyzing the results.

Thank you very much for this helpful suggestion. In the revised manuscript, we will add a table summarizing data availability (monitoring period, number of samples, and measurement methods for discharge and turbidity). In addition, we will include a separate overview table of all analyzed events, including start and end time, event duration, rainfall amount, Qmax, total flow volume, and total sediment load.

3) Line 234. How did you measure the total kinetic energy?

Thank you very much for pointing this out. We will clarify this in the revised manuscript. The total kinetic energy E of rainfall was calculated following McGregor et al. (1995) as shown in equation (1):

$$\mathrm{E} = 0.29 \cdot [1 - 0.72^{(-0.082 \cdot I_{30})}] \quad (1)$$

where I_{30} is the maximum 30-minute rainfall intensity during the event (mm h^{-1}), and E is the kinetic energy of that rainfall event expressed in MJ ha^{-1} mm^{-1}. The resulting EI_{30} estimates were comparable to previously reported results for the HOAL (e.g., Szeles et al., 2025).

4) Lines 271-272. Please justify the selection of the trigger values (5 Ls-1, 2 Ls-1, 100 mgL-1) and comment how these values may intefere on the results.

Thank you for pointing this out. The trigger values (5 L s^{-1}, 2 L s^{-1}, 100 mg L^{-1}) were originally adopted following Eder *et al.* (2010) to minimize the inclusion of low flow events with negligible sediment transport. We would like to clarify that only events that triggered overland flow at E2 were ultimately included in the analysis (n = 55). Events with values close to these thresholds generally do not generate overland flow and therefore do not contribute to the dataset used for comparisons between E2 and MW. Consequently, while the precise thresholds influence the total number of events identified at the catchment outlet MW (n = 255), they do not affect the subset of events representing overland flow at E2, which were used for our analysis. These threshold values thus do not interfere with our results. We agree that listing these trigger values in the manuscript is not essential and will remove them to avoid confusion.

5) Figures 8 and 9. Why should erosive land cover (especially at this spatial scale) correlate with EI30? Please consider revising the figures.

Thank you for this insightful comment. We agree that the correlation between EI_{30} and the percentage of erosive land cover involves two conceptually independent variables. In the revised manuscript, we will remove this correlation to avoid implying a causal relationship.

6) 6. Conclusion. We expect to read some conclusive statements about hydrological and sedimentological processes (e.g., dilution, sediment sources, and macropores, among others).

Thank you very much for this constructive suggestion. We agree that the current conclusion can better emphasize the underlying hydrological and sedimentological processes. In the revised manuscript, we will extend the conclusion to explicitly discuss key mechanisms, including (i) dilution at the catchment outlet, where subsurface inflows from springs, wetlands, and tile drains increase flow volume but contribute little sediment, resulting in lower SSC compared to the hillslope outlet E2, and (ii) episodic sediment transfer through preferential flow pathways such as macropores and vole burrows above the tile drainages, which we documented in the field (Figure 11), facilitating rapid surface-to-subsurface connectivity and allowing fine sediment to bypass retention. The updated conclusion will therefore provide a clearer process-based synthesis beyond the summary of observed results.

Minor review.

7) Lines 56-57. I understand that the numbers (20%, 80%) refer to a specific example, they cannot be used in generalized terms. Please consider rephrasing the sentence.

We agree that the original phrasing may imply a generalization. We will rephrase the sentence to clarify.

8) Figure 2. Please identify the height of the isolines.

We will label the heights of the isolines in Figure 2 directly on the contour lines.

9) Lines 287-290. Please check for the correct reference to Figures 4a, 4b, 4c, and 4d.

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.

10) Figures 4b and 4d. Flow volumes vary two orders of magnitude for similar peak flow. How valid is the use of peak flo as a proxy in these cases?

Yes, this is an important point and was also raised by Reviewer 1. It has now been fully addressed in the revised manuscript.

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.

11) Table 2. Please provide the size of each area. This is particularly relevant to compare the results at E2 and MW.

In the revised manuscript, we will add the size of each area in Table 2. Additionally, we will add to the Study Area description of the revised manuscript that the catchment size for the E2 station is 45 ha and was derived from a high-resolution 1 m LiDAR DEM using flow accumulation and watershed extraction in ArcGIS Pro. This allowed precise delineation of hydrologically nested contributing areas from the hillslope-scale E2 (45 ha) to the catchment-scale at MW (66 ha).

12) The term EI30 is (correctly) defined as 'erosivity', but throughout the text and figure captions, we read that EI30 is "intensity". Please revise it.

We thank the reviewer for pointing this out. We will carefully revise the manuscript and all figure captions to ensure that EI_{30} is consistently referred to as 'erosivity' instead of 'intensity.'

13) Lines 657-658. Where were the data from Frau 1 and Frau 2 presented? What do you mean by "previously assumed"?

In the revised manuscript, we will present the data for the tile-drainages Frau2 and Frau1 and replace the term “previously assumed” with a more accurate description. Specifically, we intended to convey that the contribution of the tile-drainages is likely higher than the measured values suggest, because some sediment is deposited in the metal H-flume before reaching the turbidity sensor (Figure 4d in the manuscript).

References

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