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Review and rebuttal of the paper

Internal Erosion in a Full-Scale Experimental Embankment Dam: Insights from the Dismantling of the Älvkarleby Test Dam

Johan Lagerlund, Peter Viklander, Jasmina Toromanovic

Editor handling the paper: André Koelewijn (Guest editor for Thematic Series on dike erosion)

Below the comments by the reviewers or editor(s) are listed in black. Rebuttal by author(s) is given in red.

Round 3

Editor decision: accept submission

Both reviewers accept submission

Round 2

Editor decision: resubmit for review

Reviewer A, resubmit for review:

The authors adequately revised the manuscript in accordance with the reviewer's comments. However, there is a point where clarification is required. Thus, this reviewer suggests revising the following point before accepting this manuscript.

The authors amended the first sentence (Lines 401-402) in the revised manuscript in relation to Comment 10. They say, "the maximum global hydraulic gradient was only about 2", but no explanation is given for how the authors obtained this value. In the reply to the comment, they say that the maximum global hydraulic gradient of approximately 2 came from the fact that the 3.27 m head difference over the 1.6 m core thickness. This should be explicitly mentioned in the manuscript so that readers understand how this hydraulic gradient is estimated.

Of course! Very valid point. Only having this information in the conclusion is a no no. We have addressed this issue now on line 101 – 104. Thank you for highlighting this.

Reviewer B, Recommendation: Revisions Required:

Please plot the locations of the piezometers in figure 5. It is difficult now to see how the depth measured in 209 and in 2024 relate to the settlement of the dike itself.

Thank you for your comment. We considered this as well; however, when we added the depths to Figure 5, increasing the y-axis scale made the crest surface appear very flat. It became challenging to see the actual variation in settlements when the y-axis ranges from 0 to 350 cm. By keeping it between 310 and 350 cm, it's much clearer how much the core crest has settled. We've indicated where the piezometers are located within the section. Apologies for not fully addressing your suggestion, but we believe it's best to keep Figure 5 unchanged. We also rearranged Figure 6 and Table 1 so they're closer together, which we hope will improve understanding. See lin 188, line 192 and line 198-200.

In par. 5.4 the authors describe a two-stage internal erosion mechanism at the abutments. The fact that there is a sharp contact between the earthen dam and the concrete abutments is not fully explained. Specifically, what is the effect of preferential flow at this contact? This should be addressed.

We appreciate the reviewer's observation. We have added a short clarifying paragraph in Section 5.4 (immediately after the discussion of Figure 17) to explain that if the soil–concrete abutment contact is not perfectly sealed, preferential flow can develop along that interface, causing local contact erosion of the fine filter at the boundary. In the added text, we emphasize that this abutment-specific effect is treated as a boundary condition contributing to the overall two-stage mechanism, rather than its sole driver. We also clarify that the two-stage internal erosion mechanism is proposed for the dam system as a whole and is supported by multiple lines of evidence across the dam's interior (e.g., fines redistribution patterns and eroded core findings), not solely by the abutment observations. This clarification does not introduce new data, but ensures the role of the soil–structure interface in our interpretation is clearly articulated. Changes made at lines 390-397.

Round 1

<u>Editor decision: resubmit for review</u>

<u>Reviewer A: modifications required, resubmit for review.</u>

1. General comments
This manuscript reports observations of the test embankment dam during its dismantling after hydraulic loadings. The manuscript identifies traces of internal erosion and provides possible explanations for their causes. This manuscript may be of value to readers, since it acts as a summary of the report by Lagerlund and Riaz (2026). One problem is that the report referred to has not yet been published. Thus, this reviewer could not check the report and could not confirm much of the information. This reviewer is not familiar with the journal's policy, but for many ordinary journals, citing unpublished papers/reports is not permitted. If the editor accepts such a situation, this reviewer does not deny the publication of this manuscript. Some specific comments are given below. This reviewer recommends that the authors revise the manuscript accordingly.

We acknowledge the oversight regarding the reference. When drafting the paper, the report was pending publication, and by submission, it was published; we neglected to update the reference accordingly. The full report number has now been included. (line 480)

2. Specific comments
(1) The authors say that "the reservoir was impounded in early 2020 and held at a near-constant level for two years" at Line 71 on Page 2. The actual water level of the reservoir during this period should be clearly mentioned and indicated in Fig. 2.

We thank the reviewer for pointing out the omission. We have now explicitly stated the reservoir water level maintained during the first two years of operation and annotated Figure 2 accordingly. In the revised manuscript (Section 2.1), we clarify that the reservoir was held at approximately 3.27 m above the dam's base (~20cm below the core crest). Figure 2 has been updated with a horizontal line indicating this near-constant water level. This change makes the operating hydraulic head clear to the reader, as recommended. Furthermore. We have updated the dimension for the fine filter, coarse filter and core soil crest height. And old cross section had mistakenly been inserted. Now, everything should be correct. Added new text marked in red at line 71 – 72.

(2) Some explanations on the materials used are given in Lines 122-129 on Page 4. However, the information about the density is missing. They should be given in the text (or in a table).

We appreciate this suggestion. We have added the missing density information for the embankment materials in the manuscript at line 128-129 (Section 2.2: Construction procedure and quality control). We also mention at line 104 that the core was DESIGNED with a certain moraine. As we see later in figure 14, the design moraine and the actual used moraine differed

(3) The height of the core at the time of construction should be plotted in Fig. 5.
Good suggestion. Makes it more clear. We have updated Figure 5 to include the original core crest elevation for comparison. In the revised figure, a horizontal dashed line at 347 cm marks the core's height at construction (2019). This allows readers to see the magnitude of settlement by comparing the initial and final elevations. We have also mentioned this original height in the text.

(4) Locations of the piezometers listed in Table 1 should be shown in Fig. 4.

We inserted an explanation in the text at line 187 – 188 and also a complementary figure (new figure 6 at line 193 to make the positions and orientations more clear. We think this makes it much more easy to follow.

(5) Figure 9 is not referred to in the text. Perhaps it should be cited in Lines 210-211 on Page 8.

Nice spotted! A reference to Figure 10 (a new Figure 6 was added so the old Figure 9 has now become Figure 10). Reference made at line 218.

(6) The statement at Line 243 on Page 11 cannot be confirmed since the densities at the time of the construction are not provided. They should be given in the text (or in a table) and shown in Fig. 12.

When we read this again, we indeed agree with your comment. We have now rearranged section 4.5 to address this concerns. I hope we have made it clear now. Changes marked as usual in red (line 249 - 264

(7) It is mentioned that the water content at the time of constriction was 8-10.5% in the caption for Fig. 13. It is recommended to plot this range in Fig. 13.

This have now been included in the Figure 14 (“old” Figure 13) at line 254.

(8) The authors have a discussion in Section 5 without properly referring to the figures shown in the previous sections. For easy understanding of the statements in this section, the authors should refer to the figures in the previous sections in the text.

You are absolutely right. We have added references to figures marked in red in section 5.1 and 5.2, see red markings at line 270, 306, 316, 317 and 324.

(9) The statements in Subsection 5.1 cannot be confirmed, as no pore water pressure change is shown. Proper plots or references should be provided.

We thank the reviewer for pointing out that the pore-pressure related statements in Subsection 5.1 require explicit supporting evidence. In the revised manuscript, we have addressed this by adding direct references to published pore-pressure measurements from the Älvkarleby test dam. Specifically, the delayed pore-pressure build-up during initial impoundment is now explicitly referenced to Toromanović et al. (2022b) (added at line 276), and the significant and persistent pore-pressure drop at the downstream core base following reservoir drawdown is referenced to Toromanović et al. (2025) (added at line 278). These studies present the measured pore-pressure time series and detailed interpretations that underpin the statements made in Subsection 5.1. No new figures were added, as the cited publications provide the necessary documentation of the pore-pressure behaviour discussed. We believe these additions ensure that the interpretation of internal erosion mechanisms is now fully traceable to documented measurements.

(10) The authors say that “the dismantling of the Älvkarleby test dam showed that internal erosion happened in many places, even though the hydraulic load was low” in Lines 389-390 on Page 16. However, the hydraulic load given is not shown at all; no one can accept this statement.

We appreciate the reviewer's suggestion to quantify the hydraulic load. In the revised manuscript, we have replaced the vague phrase "low hydraulic load" with a clear numerical value. Specifically, we now state that the dam operated under a maximum global hydraulic gradient of approximately 2 (based on the 3.27 m head over the 1.6 m core thickness). This explicit quantification addresses the comment by making the conclusion more precise. See changes at line 401 and 403.

Reviewer B, recommendation: revisions required, accept Submission

The paper describes the results of a detailed investigation of internal erosion phenomena found after dismantling of the Alvkarleby Test Dam in Sweden. The paper is well written and properly structured and follows a clear line of argumentation, starting with a description of the relevant aspects of the Test Dam, the observations and concluding with a discussion on hypotheses of the mechanisms leading to the observed internal erosion phenomena.

The topic of this paper is relevant. Internal erosion is a complex phenomenon which is still lacking a fundamental understanding. Observational data are of great importance in furthering the knowledge of the mechanisms of internal erosion. The interesting aspect of this study is the fact that observations are being made under controlled circumstances. The Alvkarleby Test Dam has been carefully constructed and dismantled which offers lots of information. When building the dam, a number of elements have been inserted, such as a concrete and wooden block and a gravel pillar and gravels sheets, to test the effect of inhomogeneities on the functioning of the dam.

Based on the findings of the study, the authors come to the conclusion that internal erosion started not only at known defects around the inserted elements but particularly along the interface between the core layers. This is explained by the fact that excessive moisture during construction led to insufficient compaction and zones of low density at interfaces and by the fact that wrong filter sizes have been used for the core and fine filter, thus leading to internal transport of fines. These insights are valuable for understanding the importance of proper construction and application of design rules.

There is a number of aspects which need to be elucidated:

1) Figure 5 shows the core soil height at time of dismantling. This is an important piece of information but authors did not add the locations of observed internal erosion to this figure, neither explained the relation between local variations in core soil height and internal erosion or the absence of this relation. This needs to be added.

We appreciate the reviewer's important observation. In response, we have added a new figure in Section 5.1 (now Figure 15 of the Discussion) that reproduces the core height profile from the original Figure 5 with clear markers at the exact locations along the dam where each internal erosion feature was observed. This approach allowed us to keep Figure 5 in the Results section unchanged, preserving the original crest elevation data (the erosion results), while presenting the comparative analysis between crest settlement and erosion features in the Discussion section for clarity. We added figure and explanatory text (lines 275–282 in the revised manuscript). These additions directly address the reviewer's comment and emphasize the spatial correlation (or lack thereof) between the observed crest settlements and internal erosion features in the dam."

2) Internal erosion is found near the abutment. Constrained flow might take place here and result in internal erosion. If this is the case, then it is an artefact related to the specific construction and any observation cannot be generalised to practical insights on internal erosion in functioning dams. Authors need to adress this aspect.

We thank the reviewer for raising the important question of whether the internal erosion observed at the abutment may be an artefact of constrained flow associated with the experimental setup. We have revised the end of Section 5.2 to address this explicitly. As now clarified in the manuscript, both abutments were constructed identically, including geometry and contact treatment, yet internal erosion was observed only at the right abutment, while the left abutment remained intact. This asymmetric outcome highlights the inherent local variability in embankment dams, where perfect homogeneity cannot be achieved even under controlled construction conditions. The erosion at the right abutment was confined to the downstream portion of the core and is interpreted as a localized response to a specific vulnerability at the core–abutment interface, rather than a universal artefact of constrained flow at rigid boundaries. We also clarify that the dismantling provides a snapshot after 4.5 years of operation and does not imply that the erosion process had ceased or stabilized. While acknowledging the influence of boundary conditions in the test dam, we therefore consider the observation to provide a relevant cautionary insight for real dams, where core–abutment contacts are likewise recognised as potential weak points if not perfectly sealed (Fell and Fry, 2007; Lowe, 1997). We believe these revisions appropriately address the reviewer's concern and clarify the scope and generalisability of the findings. Additions have been made at lines 334 -345.

3) in par. 5.1 "Stettlements and Vertical Deformation" pore pressure monitoring has been mentioned. Can authors explain why these data have not been included in the analysis or article. Clearly these data might give more insight in onset and development of internal erosion. Did the observed internal erosion phenomena indeed corroborate the hypothesis that a drop in pore water pressure, as mentioned, was evidence of a developing preferential seepage path?

We thank the reviewer for raising the important question regarding the pore pressure monitoring data and their potential relevance to the interpretation of internal erosion processes. Pore pressures were continuously monitored throughout the full operational period of the test dam as part of the broader research programme. However, the present paper is deliberately focused on forensic observations obtained during dismantling. In this context, the pore pressure transducers are used analytically only as internal reference points to quantify vertical settlements by comparing their elevations at installation and dismantling.

A detailed analysis of pore pressure time series, including interpretations during impoundment and drawdown–refill cycles, would substantially broaden the scope of the paper and shift its focus away from the dismantling-based forensic analysis. Such analyses have therefore been presented elsewhere. To avoid any ambiguity for the reader, we have added a brief clarification in Section 4.1, at the point where the piezometer instrumentation is introduced, stating that while pore pressures were monitored continuously, their time-series analysis lies outside the scope of this study, and referring interested readers to Toromanović (2024) for a comprehensive treatment.

Regarding the reviewer’s question on corroboration, we note that the forensic observations reported here are consistent with earlier monitoring-based interpretations. In particular, the internal erosion features identified during dismantling coincide spatially with the core zone where a persistent drop in pore pressure was previously observed during drawdown–refill cycles (Toromanović et al., 2025). While the present paper does not rely on pore pressure data for its conclusions, this consistency supports the earlier interpretation that such pore pressure anomalies can indicate the development of preferential seepage paths.

Additions made at line 198 – 200 plus a new reference, Toromanovic (2024) (line 556-557)

4) The paper would gain relevance by putting these findings in perspective of the current practice of dam construction. If this test dam, carefully built, showed internal erosion as a result of improper compaction and grading of the filter material, would that lead to a conclusion that guidelines need to be reviewed or dams might be more susceptible to internal erosion than previously thought.

We appreciate this insightful comment and have acted accordingly. We have added a new subsection 5.6 (“Implications for Current Dam Construction Practice”) to the Discussion Line 431 – 444). In this section, we explicitly place our findings in the broader context of dam construction standards and practice. We emphasize that our results do not suggest an inherent deficiency in current design guidelines (such as filter and compaction criteria), but rather highlight how small construction-induced heterogeneities or lapses in quality control can create conditions for internal erosion even in dams built to modern standards. Thus, the findings underscore the importance of strict execution, quality assurance, and verification of as-built conditions to ensure that design guidelines are fully achieved in practice. In short, our results serve as a caution that dams may indeed be susceptible to internal erosion if subtle construction imperfections go undetected, reaffirming the need for vigilant construction and monitoring, rather than indicating that existing guidelines require fundamental revision.

To reinforce these points, we have also added a brief statement in the Conclusions (line 462 – 464) to highlight that the study’s practical takeaway is the critical need for rigorous construction quality control to mitigate internal erosion risk in embankment dams built to current standards. These additions collectively address the reviewer’s request by contextualizing our findings within current practice and providing balanced commentary on their implications for dam safety guidelines and construction quality.