

Response to Referee 2

We are thankful for Referee 2's constructive and valuable feedback. We have assessed the Referee's concerns in the manuscript and detail our individual responses in the following. The Referee's comments are in a yellow background, and our responses are beneath.

The manuscript examines the issue of modeling culvert blockage during flood events. This process is significant in river reaches where culverts are numerous, often undersized, and susceptible to clogging by material transported by the flow. The study builds on the TELEMAC-2D hydrodynamic model, which already includes a routine for simulating culverts as one-dimensional elements. The authors extend this routine by introducing an additional head-loss term defined as a function of the blockage ratio (BR). This term is assumed to be activated dynamically when the ratio (TR) between the upstream water depth and the culvert's height exceeds prescribed threshold values. The influence of these two parameters on flood dynamics is investigated through a set of hydrological–hydrodynamic scenarios developed for a small German catchment in which several culverts are present.

In principle, the study is relevant and aligned with the topics addressed by the journal. However, in my view, several aspects require further clarification before the manuscript can be considered for publication in NHESS.

We thank the referee for providing valuable feedback and acknowledging the relevance of the study.

General comments

Choice of study basin. A general concern relates to the authors' selection of a reference catchment for which hydrological information is extremely limited. There are no hydrometric measurements available, either at the outlet or within the basin. Information on the channel network is also incomplete; several culverts were missing from the initial dataset, and the authors acknowledge the possible presence of additional undocumented structures (line 110). This lack of data has made model validation essentially impossible, meaning that all scenarios are assessed only relative to a reference simulation that may itself not reflect actual system behavior. The authors should provide a clear justification for adopting such a poorly documented case study.

Unfortunately, it is rather rare for such catchments to have excessive calibration and validation data. Even if these catchments have a gauging station at the outlet, this does tell little about the flood dynamics in upstream parts of the catchment. A robust calibration data set would therefore include not only multiple gauges across the catchment, but also detailed spatial inundation data, since urbanized areas have particularly complex flow paths which can often not be linked to a temporal gauge at individual locations.

Other studies have also acknowledged incomplete data sets of culverts – both in their parameters as well as their location (e.g. Truhlar et al. (2020) <https://doi.org/10.1061/JSWBAY.0000900> , Lessard et al. (2023), <https://doi.org/10.1080/07011784.2022.2160660> , both references now added to the Introduction). Therefore, our study aims to highlight that culvert data sets are often incomplete which is the source of errors and uncertainties in hydrodynamic flood modeling.

Finally, the motivation for this study comes from the fact that the study area is known to experience culvert blockages, both triggered by floods, but also in dry conditions (see Figure 4). Therefore, we argue that these points combined justify adopting this catchment for our study despite the lack of validation data. We also want to point out that the study by Fallowfield & Motta (2024, <https://doi.org/10.1111/jfr3.13021>), which similarly assesses culvert blockages in a modeling framework, does not even look into validation of their model.

However, to further acknowledge that our study does lack data for calibration and validation, we have added explicit statements in the introduction that the aim of this study is not to provide a fully calibrated flash flood model, but to assess the relative impacts of culvert blockages by comparing different blockage scenarios. This provides a framework for taking culvert blockages into account when assessing flash flood hazards.

Furthermore, we have added in the conclusion that future research should focus on establishing spatial monitoring systems for catchments to generate robust calibration data sets.

Result interpretation. In several sections, the discussion of results is excessively detailed yet does not offer adequate interpretation, particularly where outcomes appear counterintuitive or anomalous. The authors should make a more substantial effort to synthesize the findings and, importantly, to explain and justify the behaviors observed rather than simply describing them. This issue is especially relevant to my comment no. 11 below.

We address this comment in the specific comments, especially for comment 11.

Specific comments

In the following, I outline, point by point, the aspects that in my opinion require further clarification.

1. Line 98. The manuscript mentions here that “the study area has high gradients” yet no quantitative information is provided regarding the channel slopes or, more importantly, the slopes of the principal culverts (particularly the longer structures near the municipality of Reichenberg). This omission is nontrivial because the culvert modelling approach implemented in TELEMAC-2D - on which the entire study is based - excludes Type 1 flow conditions, i.e., situations in which the control section is at the inlet and the culvert slope exceeds the critical slope. The authors later refer (line 200) to Smolders et al. (2016), who state that Type 1 operation is very rare; however, this statement pertains to a completely different hydrodynamic setting, involving culverts between tidal basins with negligible slopes. The authors should therefore verify, at least for the longest and hydraulically most relevant culverts, whether their slopes can exceed the critical value for plausible discharge ranges, and hence whether Type 1 operation can indeed be disregarded a priori.

We thank the referee for this very important comment!

The Reichenberger Bach starts at an elevation of around 290 m and has an elevation of 192 m at the outlet of our study area. With a channel length of approximately 7.7 km, it has a slope of about 1.3 %.

The Guttenberger Bach starts at an elevation of around 356 m and has an elevation of 225 m at its confluence with the Reichenberger Bach. With a channel length of approximately 5.3 km, the Guttenberger Bach has a slope of about 2.5 %.

Based on these parameters, we consider that our study area is steep. We have added this information to the manuscript.

The issues involving flow type 1 in the culvert implementation in TELEMAC-2D must indeed be addressed in more detail. The referee is very correct to point out that Smolders et al. (2016) is based on a different hydrodynamic setting. We have therefore conducted a thorough analysis on the possible appearance of flow type 1 in our study area. In general, flow type 1 requires two conditions: first, the slope of the culvert is steeper than the theoretical slope for critical conditions. Second, the water depth at the outlet is small (e.g. $<2/3$ of the water depth at the inlet).

As the first condition was indeed met by some culverts for lower discharges, the water depths at the inlets and outlets were investigated for the different scenarios of the study. Since we investigate flood scenarios, water depths are large at the outlet and only in the less extreme scenarios (HN5 and HN10), we possibly have flow type 1 during the rising limb of the flood wave at some culverts. These culverts, however, are not located directly at the Reichenberger Bach, but at the end of minor creeks with very low discharge that confluence to the Reichenberger Bach.

Furthermore, the scope of this study was not to investigate and benchmark culvert representation in 2D-flood modeling, which is still ongoing research in itself (e.g. Oberauer et al. (2026), <https://doi.org/10.1016/j.jhydrol.2025.134728>). Culvert modeling will therefore still induce uncertainty into the modeling framework. In our opinion, this uncertainty is more concerning when the aim of a model is to accurately represent flood events and during possible model calibration. Since our aim, however, was to compare the model results of inundation and discharge under different blockage conditions, these uncertainties are of minor importance, since we mostly draw comparative observations and conclusions.

We have added this information to the methodology section ("Culvert Modeling in TELEMAC-2D") and furthermore added to the "Limitations" section.

2. Figure 1 should be enhanced by clearly delineating the channel network, labelling the streams referenced in the text, and indicating the flow direction.

Fig. 1 indeed lacked some necessary information. We have added the two referenced streams which should also indicate the flow direction. We thank the referee for these suggestions.

3. (Section 2.2 – Precipitation scenarios). The manuscript does not specify whether rain gauges are available, how many exist, or where they are located. Are they situated within the catchment boundaries or outside? This information should be provided, and the locations should be shown in Figure 1 if applicable.

The referee is correct that we should include more information about the rain data in the manuscript, besides just referencing the source. The rain data are from radar-based rasterized data sets with a spatial resolution of 1 km x 1 km and a temporal resolution of 1 h. We have added these information in the manuscript.

4. Lines 121–125. Given the relatively small size of the basin (33 km²) and its likely short hydrological response times, it is not particularly informative to report, for example, that 60 mm of rainfall occurred over a 13-hour period on 9 July. What matters is the temporal distribution of this event, especially the peak intensities. Additional detail on the hyetograph should therefore be provided. Moreover, for Events 1, 3, and 4, the authors report total precipitation amounts (e.g., 30 mm in 3 hours) and also state that values “locally reached 37 mm in 3 hours.” The meaning of this statement is unclear. Does spatially distributed rainfall information exist for these events? If so, what is the data source - point rain gauges, radar-derived products, or another dataset? Please clarify.

These rainfall events were obtained from a rasterized data set with a 1 km x 1 km spatial resolution and 1 h temporal resolution. We have added this necessary information to the manuscript.

It is true that floods are defined by numerous parameters in the rainfall scenario, not just the total amount of rainfall, but especially the peak intensity plays a crucial role. We have therefore added the information that the peak intensity in the July 9th event was lower than 8 mm/h.

In general, we argue in this section that the catchment area is often affected by flash floods, which can be seen by two factors. First, the characteristics of the rainfall events indicate that over the last 5 years, there were four events that exceeded at least HN3 on a catchment average, but also reached HN30 locally. Second, these small flash floods caused multiple reactions by the fire department, which demonstrates that it is well worth investigating the catchment.

5. (2.3 Mesh generation and parameters). At the beginning of this section, there is no information on the topographic datasets used. Only later (lines 153–154) is there a brief indication of the provider, but no details are given regarding DEM resolution or the acquisition technique (e.g., LiDAR or another method). Considering that the channels in the study area are relatively small, the quality and resolution of the DEM - along with any supplementary field surveys - are critical for accurately representing the terrain. Similarly, the main geometric characteristics of the culverts (width, height, length, slope, etc.) are not reported. Please provide this information in a specific Table.

We agree that more details about the applied digital elevation model are necessary and we thank the referee for pointing this out. We added that the digital elevation model is LiDAR-based and has a 1m x 1m resolution to the beginning of this section.

We have added a table and a figure in the appendix, which provides information on the locations and geometric characteristics of the culverts.

6. (Section 2.4 – Model validation). Given the very limited amount of observational data, I do not consider it appropriate to refer to this step as “model validation”. The authors explicitly acknowledge that the only available information consists of a few photographs which, due to the lack of temporal metadata, do not necessarily correspond to the peak inundation extent (line 168) and which cover only selected portions of the study area. Under these conditions, true validation is not feasible. I recommend renaming this section to “Simulation of real events” and avoiding the term “validation event”.

The referee is correct to point out that this should not be called a model validation and we thank the referee for that remark. Based on the suggestion of another referee, we have changed the section title to “Model Plausibility” and use this term throughout the manuscript.

7. Lines 172–175. The manuscript compares the rainfall events of 9 and 15 July 2021, asserting that the former was more severe because it produced 60 mm over 13 hours, whereas the latter produced “only” 25 mm in two hours (although locally up to 43 mm in two hours, as stated in line 124). As the authors will surely know, the severity of a rainfall event cannot be assessed solely using its total depth; the temporal distribution and the mean intensity over timescales comparable to the basin’s response time are far more relevant. For a basin of this size, the response time is likely on the order of a few hours, not 13. I therefore ask the authors to revise this paragraph and clearly state the criteria by which they consider the 9 July event to be more severe.

The referee is obviously correct that the severity of a rainfall event is not solely defined by the amount of precipitation. The purpose of these lines was merely to state that the event on 9th of July was still impacted by saturated soil from the event on 15th of July. However, our formulations were misleading and we thank the referee for outlining this. We have adapted the manuscript so that this section more clearly focuses on the event on 15th of July, and only mention the event on 9th of July to explain the decision of increasing the runoff in the model.

8. Line 178. The manuscript states that “a spatially distributed precipitation” was used, but it is not explained how this field was derived. Was it interpolated from point rain gauges, obtained from radar-based products, or generated through another method? Please clarify the data source and processing steps used to obtain this distributed precipitation input.

The spatially distributed precipitation was derived from 5-minute radar-based RADKLIM-YW precipitation data set, which has a spatial resolution of 1km x 1km. This information is added to the manuscript and the data source is referenced.

9. (Section 3.1). In my view, this entire section -including Table 3 and Figure 5 - introduces unnecessary confusion and does not substantially contribute to the manuscript. The authors themselves acknowledge that the inventory of culverts in the basin is incomplete, and there is no hydrometric information available to calibrate or validate the various configurations. Given these limitations, the analysis presented in this section does not appear to be supported by verifiable evidence. Considering the manuscript’s overall length, this paragraph could be omitted without loss of content.

Section 3.1 discusses the issue of incomplete data sets of culverts, which is a common problem, but only rarely addressed (e.g. Truhlar et al. (2020) <https://doi.org/10.1061/JSWBAY.0000900>). Therefore, we argue that this section is relevant and adds another layer to the study. Nevertheless, we agree that the section does contribute less to the overall key outcomes of the study. For this reason, we have reduced it to only one brief paragraph and moved the more in-depth analysis into the Appendix.

10. Lines 322–327 and Figure A2. The delayed response observed at section 2 for the HN5 and HN10 events requires explanation. This delay cannot be attributed to culvert blockage, as the hydrographs show negligible differences across scenarios. Additionally, it is unclear why, for the HN30 event, the peak flow in the br_08_tr_1.2 scenario is slightly higher than in the reference case. The authors should provide a clear explanation for these behaviors. This issue is closely related to the concerns raised in my subsequent point 11 and should be addressed in that context.

We thank the referee for this very precise observation and we have reassessed this cross-section. Cross-section 2 is at the Guttenberger Bach and affected by several effects. Downstream is the inlet to a culvert, which can cause backwater effects, especially after triggered blockage. Upstream is a long culvert, which is also overtopped in the simulation and therefore causes a flow path through a complex urban regime. This flow path merges with the Guttenberger Bach briefly upstream of cross-section 2. Directly after the triggered blockage of the upstream culvert, less discharge flows through the culvert, but more into the urban flow path, which can cause very short term differences in the hydrographs at cross-section 2. E.g., in the br_08_tr_1.2 HN30 event, the culvert upstream triggers a blockage at around 6.25 h, so directly before the flood peak and the deviation between the reference hydrograph.

As cross-section 2 is evidently not suitably located and an in-depth analysis would not contribute to the key messages of our study, we have decided to remove cross-section 2 from the manuscript.

11. Lines 354–392. This entire section requires a more thorough explanation, as both the comments provided and Figure 7 are unclear to me. Focusing on the HN5 case (5-year return period, therefore a relatively frequent event), Figure 7 (first panel, top left) presents the computed discharge hydrographs for the different culvert blockage scenarios. In the reference case (no blockage) and in the br_08_tr_1.5 scenario (delayed blockage), the flow at section 3 remains below 0.04 m³/s throughout the simulation period (over 9 hours). In contrast, in the other scenarios, starting around the fourth or fifth hour, the discharge increases sharply, reaching a peak above 0.4 m³/s around 6:20, before gradually decreasing, yet remaining higher than in the reference and delayed-blockage cases. It is unclear why the discharge in the presence of blocked culverts is up to ten times (!) higher than in the unclogged scenarios. Given that rainfall excess input is identical across all simulations, one would expect culvert blockage to reduce discharge through section 3 due to hydraulic constriction, with the excess volume appearing as surface flooding. Indeed, Figure 8 confirms that maximum water depths in flooded areas are greater under blocked culvert conditions than in the reference scenario. However, this observation contradicts the trend in Figure 7, where the discharge and conveyed volumes are higher in the blocked cases. The same unexpected pattern, although less pronounced, occurs for higher return periods: in every instance, the volume passing through section 3 is consistently higher when culverts are blocked than when they are unobstructed. The authors need to provide a comprehensive explanation for this behavior. Without such clarification, one might reasonably suspect that the model does not preserve the mass continuity, which would call all results into question and undermine the validity of the study. I also recommend that, at least for the HN5 case, the authors calculate the ratio between the volume passing through section 3 and the rainfall excess from the contributing sub-catchments. This would help identify the fate of the rainfall excess volume and clarify whether surface retention or other processes are being properly accounted for in the model.

Based on comment 10, we have decided to remove cross-section 2 from the manuscript. Therefore, cross-section 3 is now cross-section 2.

The decision from us to first briefly describe the different cross-sections and then do an in-depth analysis of cross-section 2 was rather poor, as it might confuse the reader and require a deeper knowledge about the different locations in the catchment. Furthermore, we wrote cross-section 4 instead of cross-section 3 (now 2) twice (which the referee also pointed out). We apologise for these circumstances and thank the referee for pointing this out.

We have restructured the section based on the comments of referee #1. Now, only cross-section 2 is discussed in detail and the other cross-sections are moved to the Appendix.

Cross-section 2 lies within the municipality of Reichenberg. Within the municipality, the streams (Reichenberger Bach and Guttenberger Bach) are fully conveyed beneath the surface. This means that there is a culvert inlet for the Reichenberger Bach at the upstream border of the municipality and the outlet is located at the downstream border of the municipality. Any discharge present at cross-section 3 is therefore from overloading of these culverts. Therefore, the results in Figure 7 align with the effects of blocked culverts. Since a blocked culvert has a reduced discharge capacity, less water is conveyed through the culvert. The water depth at the inlet increases, overtops the inlet structure and flows through the municipality, reaching cross section 3. For earlier blockages (TR=0, TR=0.8), water also enters the municipality earlier (seen clearly for HN5). TR=1.5 is not triggered for the HN5 event, therefore, only minimal water flows through the municipality.

In addition to restructuring this section (3.3) in the manuscript, we have also rephrased some sentences in the description of the study area in section 2.1, which should improve clarity about the locations of the Reichenberger Bach above and beneath the surface.

12. Lines 370–372. The authors report a specific observation in these lines. How is this behavior explained? Please refer to the issues raised in point 11 regarding the unexpected discharge trends under culvert blockage scenarios.

We refer to the reply to comment 11.

13. Lines 376–377. Similarly, this observation requires clarification. How can the results be interpreted in light of the anomalies noted in point 11?

We refer to the reply to comment 11.

14. Lines 414 and 417. The sentences “Increased inundation does not necessarily increase the damage induced by the flood” and “As higher water depths are also linked to greater damage” appear contradictory. The authors should clarify the intended meaning and ensure consistency in their discussion of damage relationships.

The referee is correct that the sentences appear contradictory in its current state. We have rephrased the section pointing out that inundation is more hazardous in settled areas and greater inundation also carries larger risks.

15. Lines 427–428. The reported observation requires explanation. How do the authors account for this behavior in the model results?

These lines compare the inundation extents of streets and combined residential and industrial areas. Since streets are often located slightly lower than houses, or directly next to the streams, we think that it makes sense that the inundation in streets already increases for lower return periods. We have added this explanation to the manuscript.

16. Line 430. The meaning of “temporal trend” is unclear. Do the authors refer to changes with respect to return period? This phrase should be clarified or corrected.

The word temporal trend is indeed unclear. We have replaced it with “trend along the return periods”.

17. Lines 434–436. The authors acknowledge that numerical instabilities in the model may explain anomalous behavior. This is concerning because such instabilities could potentially affect other scenarios but remain undetected. The authors should comment on the robustness of the simulations and any measures taken to verify stability.

The reviewer is correct that numerical instabilities could potentially affect other scenarios. We have re-checked this scenario and noticed an error in the simulation parameters. We fixed this error and reran the simulation. This scenario now aligns with the overall trend of the BR=0.2 scenarios.

18. Lines 477–478. The term “redistribution effects” is not clearly defined. Please provide a clear explanation.

The term “redistribution effect” is indeed unclear and we thank the reviewer for that comment.

The reason for the slight increase in inundation when only omitting blockage at culvert 2 has different facets. This slight increase in the inundation can be found at two different locations. First, there are some pixels downstream of the culvert outlet, which have a water depth higher than 30 cm only when culvert 2 is not blocked. This ever so slight increase can come from slightly different timings in the flood dynamics behind the outlet of culvert 2. Furthermore, a slightly larger inundation is noticed within the main square of Reichenberg. This can come from an untimely overlay from the two hydrographs coming from the Reichenberger Bach (cross-section 4) and the Sichelsgrund (cross-section 3). From the figures in the Appendix, C4 and C3, it is seen that for the HN10 event, the two peaks from these cross-sections almost perfectly align at around 6.5 h. Since the two critical culverts lie in direct vicinity of these cross-sections, slight changes in the culvert behavior can also slightly change the inundation within Reichenberg.

However, we think that this analysis is too detailed for the paper and might only confuse the reader. Therefore, we have rephrased the sentence that the increase in inundation increase is in fact almost negligible.

19. Lines 520–522. The statement that “this study strongly cautions against deliberately inducing blockage to reduce discharge as a basis for mitigation planning” underscores the concerns raised in point 11. The model results in Figure 7 show an increase in flow rate under blockage

scenarios, which appears inconsistent with this recommendation. The authors should reconcile these observations and clarify the implications for flood management.

We thank the reviewer for this observation. In theory, blocked culverts can reduce discharge downstream and therefore be taken as mitigation measures. However, our study (and other studies, e.g. Al-Whoane et al. 2025) show that this effect is very limited. Combined with the hazard of culvert failure, dam erosion and releasing of a catastrophic debris-laden flash flood wave, culvert blockage should be prevented. We agree, though, that our statement in these lines lacked context and explanation; therefore, we rephrased our statement for clarity.

Technical corrections (typing errors, etc.)

- Line 193: Replace “diameter” with “height”.
- Line 195: Replace “before” with “upstream”.
- Line 317: Remove the repeated phrase.
- Lines 356 and caption of Figure 7: Correct “cross-section 4” to “cross-section 3” (if my interpretation is correct).
- Line 357: Remove the duplicate phrase “a slight delay”.
- Line 368: Clarify whether it refers to cross-section 4 or cross-section 3.
- Line 443: Change “In general, the inundation...” to “In general, the relative increase of inundation...”
- Line 468: Change “the resulting inundation extents...” to “the relative differences of inundation extents...”
- Line 525: Replace “has demonstrated...” with a less assertive verb, such as “has shown...”

We also thank the referee for pointing out these technical errors. We have corrected them in the manuscript.