

Response to reviewers regarding Manuscript ID egosphere-2024-2132 entitled:

"Hydro-geomorphological modelling of leaky wooden dam efficacy from reach to catchment scale with CAESAR-Lisflood 1.9j" to Geoscientific Model Development.

Original text from the Topic Editor and Reviews is [blue](#).

Responses are black, Unedited text is [grey](#), Edited text is **bold and black**.

Reviewer 1 responses to first paper revision and author comments given below in [red text](#).

Handling Topic Editor (Andy Wickert)

[Thank you for your comprehensive and thoughtful response to the referees and for your thorough updates to the manuscript over the course of this work. I find these to be fully satisfactory for recommendation to publish.](#)

Thank you for your time and effort in editing this manuscript, it is greatly appreciated, alongside the time taken by the reviewers.

[I have only one small note, which you might choose to consider, or use, or ignore. I notice that many of my UK colleagues will use the term "flux" with sediment to mean the same as "discharge" with water. However, mathematically, a flux is a rate of transport per unit cross-sectional area, and therefore, in the case of a volumetric flux, should have the same units as a velocity. I therefore always write "sediment discharge" unless I mean a flux, in order to support clarity.](#)

Thank you for this comment. We have chosen to make the suggested changes, and also those of the reviewers detailed below for completeness.

Reviewer 1

[L24 State recurrence interval or annual exceedance probability of storm event to give an idea of its magnitude. Also state for what catchment size.](#)

We have adjusted this sentence as follows:

The findings show that simulating sediment transport increased the volume of water stored in the test reach (**channel length 160 m**) by up to an order of magnitude whilst reducing discharge by up to 31% during a storm event (**6 h, 1 in 10-year event**).

[Still need detail on catchment size, this important context to present.](#)

The size of the catchment used to run the model is detailed in section 3.2 (line 249).

[L46-48 More nuance and specific reference to sources that back up these claims is needed. These benefits are often perceived and have not been quantified comprehensively or assessed.](#)

We have adapted this sentence as follows to include recent publications in support of this:

Reintroduction of wood to the river channel is a popular form of NFM, employed for multiple co-benefits such as habitat creation and ecological enhancements (e.g., Wohl, 2017; Ockelford et al., 2024) as well as flood peak reduction (e.g., van Leeuwen et al., 2024; Villamizar et al., 2024).

But there are still deficiencies of understanding and assessment of LDs specifically which may differ to other types of large wood addition (e.g. bank attached or medial structures used to improve habitat) or naturally occurring large wood structures. For example, the number of studies that quantify empirically the flood peak mitigation of these structures is limited. Also, the study of Villamizar is based on a model whereas the van Leeuwen study is empirically based, this distinction should be made.

Thank you for this comment. We agree however this is beyond the scope of our manuscript, and the aim of this sentence is to briefly introduce the popularity of NFM and its broader application.

L50-62 A more up to date and accurate reflection on the knowledge gaps and recent advancements on understanding is needed. For example, see the work of Follett and Hankin (2022) and Geertsema et al., (2020) on approaches to model the hydraulic effects of in channel large wood interventions. Also, the work of Lo et al. (2022) gives field based observations on the geomorphic effects of leaky barriers and the work of Norbury et al. (2021) and Van Leeuwen et al. (2024) measure the hydrological effects of structures using field data. Flume based studies on hydro-geomorphic responses are also potentially useful to synthesise and compare with (e.g. Schalko et al., 2019; Muhawenimana et al., 2021).

We agree that including a summarised review of knowledge gaps and recent advancements would improve the manuscript. As such we have updated this section to provide greater understanding on the influence of large wood and LDs on geomorphology, as well as numerical and flume studies:

Despite their rapid deployment in riverine management over recent years, a key knowledge gap is how LD efficacy evolves temporally, both in response to geomorphic evolution up- and downstream of the LD, but also in response to flood sequences (Addy and Wilkinson, 2019; Grabowski et al., 2019). **The influence of large wood on river systems is well understood (1): wood increases fluvial complexity whilst being resistant to erosion and providing storage space for water (2) (Gurnell et al., 2018; Wohl et al., 2019). Specifically, large wood can form pools (e.g., Abbe & Montgomery, 1996; Al-Zawaidah et al., 2021; Ravazzolo et al., 2022), increase sediment storage (e.g., Comiti et al., 2008; Wohl & Beckman, 2014), protect against or induce bank erosion (e.g., Abbe et al., 2018; Galia et al., 2024) and influence floodplain morphology (e.g., Sear et al., 2010; Wohl, 2013). Large wood is generally mobile (Wohl et al., 2023), unlike LDs that are often engineered and anchored in-situ and therefore can be functionally different with wide-ranging designs (Lashford et al., 2022; Lo et al., 2022; Quinn et al., 2022).**

Challenges in disentangling the relative impact of LDs from the influence of land use, antecedent conditions and other flood risk management interventions presently result in an unclear understanding of their influence over time. **Similar to natural wood, LDs influence the hydraulic regime through increasing roughness and thus have the potential to influence channel geomorphology. The few empirical field studies that have focussed on LDs have highlighted that LDs can reduce peak flows for the 1-year annual exceedance probability (AEP) by 10% on average (3), however the response can be highly variable (Norbury et al., 2021; van Leeuwen et al., 2024). The backwater rise induced by LDs is also variable and can**

be increased or decreased with the presence of porosity-reducing material (Muhawenimana et al., 2023). Furthermore, the ability of a LD to store water, or sediment, can be dependent on the distance between the riverbed and the bottom of the LD, with gaps >0.3 m unable to store sediment in the Yorkshire Dales, UK (Lo et al., 2022), while increased wood volume also amplifies scour (Schalko et al., 2019). Laboratory experiments have shown that representing LDs as non-porous structures increases drag and flow area (Muhawenimana et al., 2021), and therefore it is important to account for porosity of the structures in numerical simulations. Yet often porosity is not considered in numerical simulations due to representing these complex structures in reduced-complexity models.

Recent works have focused on integrating LDs into 1D and 2D models at different spatial scales (Hill et al., 2023), most commonly representing the interventions as localised roughness adjustments (Pinto et al., 2019; Geertseema et al., 2020), geometry adjustments (Pearson, 2020; Walsh et al., 2020), or a combination of the two (Dixon et al., 2016; Senior et al., 2022) (4). LDs have also been represented in hydraulic models, through stage-discharge relationships realising LDs (and other RAFs) as weirs or culverts (Thomas and Nisbet, 2012; Metcalfe et al., 2017; Keys et al., 2018; Hankin et al., 2019; Pinto et al., 2019; Hankin et al., 2020; Leakey et al., 2020; Pearson, 2020; Follett and Hankin, 2022). A comprehensive review of the large wood numerical modelling literature focused on artificially placed wood can be found in Addy and Wilkinson (2019).

Please see numbers inserted into text above:

1. Assume this is a reference to naturally formed and occurring large wood?

Yes.

2. Do you mean large wood has resistance to being transported? Better state resistance to displacement instead to be clearer? The statement that large wood provides storage space for water seems a bit strange as by adding wood to a stream the volume of in channel storage would be reduced. Do you mean the ability of large wood to reconnect floodplains and create out of bank storage?

We have updated this sentence as suggested for clarity:

The influence of large wood on river systems is well understood: wood increases fluvial complexity whilst being resistant to **transportation** and providing storage space for water **through increasing floodplain connectivity and creating out of bank storage** (Gurnell et al., 2018; Wohl et al., 2019)

3. Did both studies (Norbury and van Leeuwan) show the same reductions in peak flow for the same size of flow? Be clear.

Updated for clarity:

The few empirical field studies that have focussed on LDs have highlighted that LDs can reduce peak flows for the 1-year annual exceedance probability (AEP) by 10% on average, **however the**

response can be highly variable (van Leeuwen et al., 2024); Norbury et al. (2021) reported an average reduction in peak discharge of 27.3%.

4. Are these hydraulic or hydrological models?

They are a mixture of hydraulic and hydrological models.

L159-160 'Upwinding' and 'upwind' are strange terms to use. Consider rewording?

The upwind scheme is the correct term for calculating the downstream flow field based on cells upstream, however for clarity we have updated this to:

CL employs a first order **upwind scheme (Coulthard et al., 2013)**.

It would be better if this term was clearly defined here, or other terms were used to make this clearly understandable (or does this journal assume reader knowledge of the term?).

We have chosen not to amend this sentence and assume reader knowledge.

L210-214 More details on the prototype reach is needed in the paper rather than citing the thesis. The channel slope used seems quite low for a headwater stream where leaky barriers are typically used. Would it be worth testing the model over a range of slopes to see the effect? Why were different DEM resolutions tested? Certainly important but little context or purpose on this is given.

We have added more details to this section as outlined below:

The model domain was 160 m long and 100 m wide **and represents a second-order stream**. The DEM had the same average slope as a prototype site (0.01 m m⁻¹; Wolstenholme, 2023) **where LDs were installed in 2019** and was created by linear interpolation between the high and low survey points in the reach captured with a Topcon OS-103 Total Station (TS).

We explored the impact of grid resolution to understand whether any bias was introduced in the model outputs, and to ensure that potential caveats as a result of resolution were highlighted in this development and technical paper. We agree that it would be interested to explore the impact of slopes on the effect, however this is beyond the current scope of the manuscript.

This helps but any details to give on the topographical survey in terms of survey strategy and point density? This can affect the quality of the topography used. What catchment area is the reach at? What is the channel width? Could testing different slopes be referred to as an aspect for further study at the end of the paper?

The site topography data was simply used to extract a gradient between the high and low survey points as detailed in the text, therefore we do not believe that inclusion of the survey strategy nor point density is relevant to this submission.

Regarding testing of different slopes, yes this could be an aspect of further study and that is alluded to in the text (end of section 5.2 regarding calibration).

L343-344 The finding that increasing LD gap size resulted in less sediment being lost seems counter-intuitive.

Less sediment was lost out of the model domain. There was less erosion upstream and downstream of the leaky dams when gap size was increased (e.g., Figure 9) as the river had less energy to erode the bed when compared to a smaller gap size.

Ok if that is the case but perhaps need to be clear that is for a condition of no sediment supply coming from upstream into the reach at the upstream boundary (if that was the case). I.e. are these geomorphic findings for a clear water scour condition? Adding a log jam in that blocks more of the channel for the latter condition would be expected to result in reduced sediment yield downstream.

Yes, the findings are from clear water. We agree and this would be useful to incorporate in future research.

L455-459 Perhaps a more important point is how valid is this model. Whilst there may be some value in using it in an heuristic manner for scenario testing, validation using empirical observations of changes in discharge and morphology would strengthen the value of this model and give practitioners more confidence in using it. This is a knowledge gap that should be stated. The word 'behaviouralist' seems a strange one to use.

We agree that to add more value the discussion would benefit from being more closely tied in with existing leaky dam literature. Therefore, to address this point we have included the following text at the end of section 5.2.:

Care must be taken when using the LD module for CL as the right results, such as elevation change and Q reduction, may be overestimated when using coarser grid resolutions. **The scenarios simulated herein align well with the relative influence of LD from both field and laboratory observations including the formation of downstream pools (Lo et al., 2021; Lo et al., 2022; Muhawenimana et al., 2023), and the potential for sediment storage upstream (Comiti et al., 2008). Future work should focus on calibrating and developing this tool as a flexible and rapidly deployable option for LD simulations in CL, that should currently be used heuristically to mitigate the need for calibration. The LD module for CL can therefore be best used to understand the relative impact of LDs in larger, complex catchment to identify their individual impact on FRM.**

This is better but the words **..that should currently be used heuristically to mitigate the need for calibration'** are unclear and should probably be removed. The fact is the model is uncalibrated and unvalidated using real world data on morphological change adjacent to LDs. The potential value of the model to make predictions at the catchment scale is true but again some testing first at the reach scale is needed to underpin this first. This includes both calibration and validation.

Thank you for this comment. We have chosen to retain the text as models such as this (and exploring their benefits towards understanding landscape processes) have great value in these types of behavioural studies, where we can evaluate the general response of an environment.

L477-480 Yes this is a fair statement but there simply isn't usually the data to calibrate or validate sediment transport in the real world unfortunately and processes are notoriously

difficult to predict for different hydrological events. In contrast, observations of channel hydraulics and hydrology are more available or accessible to enable model assessment.

We are glad that the reviewer agrees with this statement, and that the lack of data to calibrate/validate sediment transport highlights the need for the heuristic approach adopted and subsequent analysis.

Ok but model outputs in terms of erosion and deposition responses could be used for calibration and validation instead of sediment transport. Perhaps mention this earlier (see point above)?

We agree with this statement, and this is scope for future work.

L491 Could the model be used to assess other types of NFM structure or in channel wood? Another aspect deserving further study in future?

Absolutely. We have added the following to the previous paragraph to highlight this:

Additionally other types of structures with porosity can be evaluated with the model, if it can be defined through its height, gap (or lack of) and roughness, for example natural wood, bridges and bunds, and presents opportunities for further study.

Good though some of these structures are not necessarily porous (bridges, bunds).

We agree, however bridges can be porous as they allow water to pass underneath them, with pillars (or foundations) blocking the flow of water in specific places. Indeed bunds could also be represented with a very high roughness to emulate water infiltrating horizontally through the bund, or, to reflect an outlet point.

Associated files with the paper. Appears that the model has been uploaded for both weblinks on Zenodo. Could not see the datasets.

All data used in the study and the model are available on Zenodo, in the .7z file. Ok although the file format wasn't readable (CAESAR model needed?).

Ok although the file format wasn't readable (CAESAR model needed?).

7z is an archive file type that is unzipped using the 7zip program. All files within the folder are human readable.

Reviewer 3

The Authors satisfactorily answered my previous observations.

Overall, the paper presents an interesting approach to include sediment in evaluating the efficacy of leaky wooden dams. Now it emerges also from the text that the proposed approach needs to be calibrated before its wide applicability.

Some remaining observations are listed below:

Line 46: Is "reduce slow flows" correct? Isn't it "reduce flow velocity"?

We have updated this.

Line 114: why “the aims of this paper were...” and not “are”? Furthermore, why not “The aim of this paper is to explore...”? (the aim is “to explore”; then, different aspects are explored, but the aim is one).

Updated to “The aim of this paper is explore...”.

Line 309: The sentence is correct. However, while in the text the Authors refer to the “grid resolution,” in Figure 4 they show the DEM resolution on the horizontal axis. This is a bit counterintuitive, as the DEM resolution increases with decreasing grid resolution. Please consider displaying the grid resolution instead.

We have not made any changes as we have used “DEM resolution” and “grid resolution” interchangeably, referring to the resolution of the pixel size of the DEM.

Line 327: Please rephrase. ΔQ can be more variable, but the sentence “...contains substantially more variability” sounds weird. Additionally, ΔQ was not clearly defined.

ΔQ has now been defined when it first appears in the text. We have also updated the sentence to:

In contrast, when sediment transport was enabled ΔQ **was more variable** after the storm.

Figure 5: note that the secondary axis (Q) is different in the 4 sub-figures.

We have updated Figure 5D that had the extra axis labels.

Line 346: is it “ ΔQ_s ” or “ $\Sigma \Delta Q_s$ ”?

For clarity we have altered this to:

Cumulative sediment discharge ($\Sigma \Delta Q_s$).

Lines 361-362: is “average channel width elevation change” correct? Not only “average channel elevation change”?

Yes, we have updated the text accordingly.

Figure 6: the legend is missing.

The legend is visible on the submitted PDF.

Line 429: Shouldn't it be “4.3” instead of 4.43”?

4.4.3 is correct, this refers to the results from the adjustment of the leaky dam parameters (section 4.4).