Peer Review File

Stream Restoration that Allows for Self-Adjustment Can Increase Channel-Floodplain Connectivity

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[1]Round 1
[2]Round 2

Manuscript 1006: "Stream restoration that allows for self-adjustment can increase channel-floodplain connectivity"

Round 1 Review Comments and Author Responses

AE Comments

This is a useful study that examines stream adjustment a decade post-restoration using a two-dimensional hydraulic model to help explain geomorphologic process. Several reviewers have provided comments and editorial suggestions that could improve the manuscript. Two reviewers point to the lack of sediment data – and its critical role in explaining channel evolution. My recommendation is a major revision with careful consideration of reviewers' comments. Reviewer comments are reproduced below. There is a supplementary StreamStats report that Reviewer 2 worked up, to help improve the manuscript.

Major revisions have been made based on the suggestions of the reviewers, which are greatly appreciated and have improved this paper.

The style has been shifted to a case study. Given this change we felt that figure 5 ought to be included as site pictures in keeping with the provided template, other figure numbers are adjusted accordingly. In general, the conclusions were changed to better reflect the site-specific nature of the study and the single snapshot of the site we have in terms of hydraulics. Adjustments were made to figures 6 and 7 (previously 7 and 8) based on comments pointing out a plotting error. The site description was expanded to include more information on the hydrology of the site and sediment characteristics.

We now provide, in the description of our study area, more information on the sediment characteristics of the reach and also summarize information on morphodynamic change within the reach. The morphodynamic change of the reach has been analyzed and described in previous work. In rereading our manuscript, it might not have seemed clear that our comments about morphodynamic change were in reference to that previous work. It is beyond the scope of the present work to calculate a full sediment budget and leave that for future work.

Please note that all lines referenced in this document refer to the track changes version. Lines do not correspond to the Final version.

Reviewer 1: Celso Bolinaga, North Carolina State University

Christensen et al. carried out a numerical study to examine the performance of two active and one passive stream restoration practices with regards to channel-floodplain connectivity. The performance of these practices was examined in the StREAM Lab reach of Stroubles Creek in Blacksburg, VA via four metrics: normalized floodplain volume; fraction of flow in floodplains; flux into floodplain; and residence time in floodplain. The study represents a valuable contribution by providing process-based performance metrics of channel-floodplain connectivity in the context of restoration practices. Nonetheless, there are some aspects of the numerical effort and analysis of the results that require further clarification.

General Comments:

- I am not confident that the influence of time on the hydraulic function of the restored stream was examined in this study, as indicated in Lines 109-110. Because the channel in Treatment 1 is – and has been – actively adjusting, it is difficult to judge without further information if the results of this study reflect a specific instance in time (i.e., when the modeled geometry was measured) or a trend in which channel-floodplain connectivity has remained high as the channel evolves.

We have added more discussion on the lack of pre restoration data for this site and the possibility of this high connectivity changing with future adjustments on line 461-472.

- In general, the study is missing information on bed and bank material characteristics and the sediment transport dynamics along the modeled reach. This is particularly important as fine sediment loading is still a concern and channel adjustments are actively taking place.

Information on the bank and bed characteristics were added on line 184. For more information on the channel evolution of the system please refer to Hendrix et al. (2022).

- There is a need to be more specific regarding what low and moderate flows mean for this stream, particularly in the context of the onset of overbank flow.

More information has been added on regressions to bankfull flow and the recurrence intervals studied for this site on line 145 and line 299.

Specific Comments:

- Lines 100-106: This text seems more appropriate for the discussion section given that no details of the present study have been provided up to this point, but results are discussed and compared to other studies.

These lines address the goals of this study and how they differ from previous works, no results of the study are mentioned. We feel this is more appropriate for the introduction than the discussion. No change.

- Lines 167-171: Are there more specific data on the bed and bank material characteristics (e.g., grain size distributions; erodibility parameters)? If bank erosion was an important factor in the restoration project, and it is certainly a key process in driving self-adjustment, more information about these characteristics needs to be included.

Information on the bank and bed characteristics were added on line 184. For more information on the channel evolution of the system please refer to Hendrix et al. (2022).

- Lines 230-231: Does sediment transport occur over the range of the tested flows?

Yes. This system has a complex cyclical sediment supply which is currently being researched by others in the Virginia Tech Biological Systems Engineering Department. For this study we chose to focus on the current hydraulics of the system and leave the sediment dynamics to the future study. No change.

- Lines 234-235: What is considered the low flow of this channel, which was also used to determine the channel-floodplain boundary? Is such a flow related to the observed break at 4 cms? It is indicated in Lines 241-243 that flows less than 5 cms did not inundate many of the pressure transducers in the floodplains.

The channel is defined on line 153 and was delineated based on vegetation, not a modeled flow. We adjusted the wording to make this clearer.

- Lines 289-292: Fig. 5 provides a sneak peek of how floodplain vegetation looks like in each treatment, and in Lines 298-303 the authors stated that the floodplains vegetation along each treatment were noticeably distinct. Therefore, Manning’s n values for the floodplains should be inherently distinct in each treatment. I am wondering if the selected calibration point, which is located at the downstream end of Treatment 2, does not allow for an adequate calibration of Manning’s n values for the floodplains of Treatments 1 and 3. Did you test the sensibility of the modeling predictions to changes in Manning’s n far away from the calibration point? How much more complex was the roughness characterization made by Prior (2021)? Did it result in significantly different Manning’s n for the floodplains? This is an important point that merits further discussion as the floodplain roughness directly affects the model-based metrics used to examine channel-floodplain connectivity.

In the model evaluation section, we test the performance of the model based on data from all three treatments (line ~297). This analysis shows no bias in error between the sites. While roughness values differ throughout the site, including between restoration treatments and within restoration treatments these differences did not produce major differences in accuracy between sites. Not enough data exists for the site to calibrate a Treatment dependent roughness, but our analysis and the analysis of Prior et al. show satisfactory performance with uniform roughness calibrated through the velocimeter. A sensitivity analysis extracting connectivity metrics with the current methodology for different roughness characterizations is preventatively time consuming. For more information on the analysis performed by Prior et al. (2022) please refer to their work. No change.

- Figure 7: Please add the low flow that was used to determine the channel-floodplain boundary. Same comment applies to Figure 8.

We added a note on line 180. The channel was delineated based on vegetation, not a modeled flow.

- Lines 371-371: Have turbidity or suspended sediment been measured along the modeled reach? How much has sediment transport loading/capacity changed as a result of the restoration project?

There is one turbidity sensor, which is not sufficient data to attribute reductions in sediment loading to the restoration or to one treatment in particular. We recommend that a full analysis of the sediment regime of the system be completed on line 474 to explore this but feel that it is beyond the scope of this study. No change.

- Lines 376-378: How much has the channel of Treatment 1 migrated and narrowed within the past 10 years? Is the expectation that the channel will continue to adjust? Or is the channel close to a state of dynamic equilibrium? Based on Lines 382-386, it appears that bank erosion is still driving channel adjustments along Treatment 1. This was further confirmed in Lines 429-431.

Hendrix et al. 2022 details this further. We have now summarized these changes in our study area description section lines 207-216 and discuss the possibility of future adjustment on lines 471 to 474.

- Lines 387-389: Because the channel in Treatment 1 is – and has been – actively adjusting, it is difficult to judge without further information if the results of this study reflect a specific instance in time (i.e., when the modeled geometry was measured) or a trend in which channel-floodplain connectivity has remained high as the channel evolves.

A note was added to address the possibility that further adjustment could reduce this connectivity as you suggest line 471.

- Lines 396-398: Same comment as before.

See response above, line 471.

Minor Comments:

- Line 110: Remove "restoration" after "restored stream".

Done.

- Lines 319-320: Add a statement to the caption of Figure 7 indicating this. It is very difficult to determine if the velocity values of Treatment 2 are even plotted.

Due to a comment from reviewer 2 this figure has been corrected and the points no longer overlap as they did before.

Reviewer 2: Daniel Mecklenburg, Ohio State University

The aim of this manuscript is laudable and valuable for the field of ecological engineering. The techniques it explores, quantifying floodplain connectivity, is a needed tool for providing objective insights into the value of stream restoration. The methods appear sound, two-dimensional hydraulic modeling, and the use of four metrics to demonstrate quantification of restoration benefit.

However, the manuscript goes on to make claims that are beyond what is supported by the experimental design and results. The three treatment examples are insufficient for drawing conclusions about broad restoration approaches. Passive verses active verses stabilization-in-place are each diverse approaches with a range of application. Treatment 3, excavation of floodplain adjacent to the channel, is particularly problematic in the range of restoration outcomes it represents. Does it represent a standard design method? If so that needs to be explained. As it stands the results are arbitrary as the original excavation could have been constructed higher or lower, narrower or wider yielding entirely different results. I suggest the manuscript be presented as a case study and limit the claims and conclusions directly supported by the results.

Other regrading methods would certainly change the channel-floodplain dynamics. We have narrowed our conclusions (lines 527 and 541) to reflect that the observed differences are a function of one application of the regrading standards which were followed in this restoration (and are followed by many projects). We still feel that these comparisons are valuable because the method of setting the bank and inset floodplain geometry are common.

The manuscript would benefit from a more complete description of the study site including standard points of reference. StreamStats references several studies on regional bankfull dimensions as well as peak discharge recurrence interval (No doubt frequency is an important determinant of connectivity). Was the original channel entrenched?

We have added flow statistics on lines 145-148 as standard points of reference. Restoration engineers suspected channelization but do not have direct records and determined that the 2010 entrenchment ratio was satisfactory (note added on line 141).

Quantify the height of the "inset floodplain" in treatments 1 and 3 as well as the surrounding floodplain… or is it a terrace? Some of this information can be gleaned from figures 7 D and 8 but site descriptions need more hydrogeomorphic context.

We have added heights of the inset floodplain and main valley (line 187). We do not consider the main valley floor to be a terrace, it is inundated during the higher flows tested in this analysis (~5 cms or more).

Section 3.2 seems more appropriate in the site description section.

Moved to line 205.

The self-formed inset floodplain:

- Do we know anything about its formation?
- Did it form rapidly in an over-widened channel once the cattle were removed?
- Was it the result of lateral channel migration?
- Is that process still going on, with the inset floodplain continuing to gain lateral extent or vertical accretion?

More on the formation of the inset floodplain and its current geometry have been added to lines 187-196.

I suggest within the introduction the origin and definition of each of the metric used be presented.

We have added more discussion on the rationale and origin of these terms on 307-319.

An impressive model was developed to explore connectivity by calculating: 1) Normalized floodplain volume, 2) Fraction of flow in floodplain, 3) Flux into floodplain, 4) Residence time in floodplain. However, a rationale is needed for why these metrics were chosen – citing sources? What do they show? The introduction has some of this information in lines 45 to 54 but would benefit from a more complete background.

We have added more discussion on the rationale and origin of these terms on 307-319.

Results

The paragraph on lines 298 to 303 may be better placed in the discussion. Results really begin on line 315.

This has been moved to the site description lines 187-196.

To review the flow and channel characteristics results I referred to the values provided by StreamStats, attached. It suggested flow rates: Bankfull about 5 cms and 2 yr-RI about 11 cms. This seems to fit nicely with Figure 7 D which shows wetted width is relatively constant for all treatments from 3 to 5 cms then rapidly increasing as water spreads out over the inset floodplains. StreamStats also estimates bankfull width, depth and cross-sectional area. The width, about 8 m, is similar to the average wetted width below 5 cms. So far so good. The bankfull depth from StreamStats is about 0.5 m. That suggests a w/d ratio of 18 which is reasonable. Figure 7 A however appears to have, at 5 cms, a depth of only 0.05 for treatment 1 and zero for treatment 2. Depth values appear to be in error, an order of magnitude less than reasonably expected. Figure 7 B presents velocity that also appears erroneous. Given StreamStats bankfull discharge and cross-sectional area the bankfull velocity, V=Q/A, should be about 1.2 m/s significantly higher than the 0.4 presented for treatments 2 and 3. The velocity is further suspected by treatments 2 and 3 being virtually identical and only half of the velocity suggested for treatment 1. The explanation presented is inadequate to explain the discrepancy. Manning's equation suggests velocity changes as the square root of slope so big changes in slope cause little changes in velocity.

The average floodplain depth was mistakenly plotted as average channel depth, thank you for your in-depth evaluation and catching this mistake. This has been replaced by the correct channel depths which are much more in line with the regional regressions from StreamStats.

The velocity discrepancy has also been addressed. The velocity plot similarly pulled information from some floodplain cells reducing the average and causing this discrepancy. We fixed this issue by using the correct classification for extracting values for this plot. The values are now much closer to the regional curves and each other. Note that this calculation caused us to revisit and recalculate the values for figure 7b (previously 8b) which was the only plot after this step in the workflow. To be sure that mistake was not made for the previous plots that data analysis was repeated showing no differences for the other figures.

The next section, calculated metrics for connectivity, is the essential content of the manuscript but until the issues with the hydraulics are sorted out, we better hold off on delving into those results. Generally, the discussion and conclusion would also benefit from narrowing the scope to a quantifying connectivity case study and scale back the conclusions to those directly supported by this important work.

Several changes have been made between lines 527 and 541 specifying that the results represent the outcome of one application of a common technique. We recognize that results may vary depending on site conditions and restoration goals and constraints but still feel there are lessons which are broadly applicable to restorations of this style.

Reviewer 3: Chelsea Mitchell, Daniel Ullom, Anand Jayakaran, Washington State University
General comments:

- A clearer description of the timeline of restoration activities is needed in the methods. Please be explicit about what years are pre-and post-restoration and what measurements and models are associated with each.

More information has been added on the restoration of the site on lines 155-163. The language throughout has been adjusted from "A stream restoration" to "The stream restoration" to remedy the confusion.

- Methods are quite long (5+ pages). Consider whether any of this belongs in the supplemental information.

We feel that all provided information in the methods section is essential for repeatability of this analysis and cannot move information into supplemental information. No change.

Though extensive work was done to characterize the state of the treatments in 2021, the authors don't provide any data or images of the treatments prior to restoration, or from early stages following restoration. This makes me question whether channel-floodplain connectivity observations can really be attributed to the treatments. If possible, show pre- and post- restoration comparisons. Are there any images from before restoration in 2010, or even shortly after restoration?

Additional images of the restoration are available in Thompson et al. (2010) and Hendrix et al. (2022). Unfortunately, there is not enough data from pre-restoration conditions to create a full hydraulic model of those conditions. We have noted that the pre restoration conditions may be a driver of the current observed differences on line 460.

Line comments:

25-26: what year were these active restoration activities completed?

Added here.

103-105: It would be helpful to know what is considered shot term versus long term in this context.

Added here.

109: change "restored stream restoration" to "restored stream".

Done.

129: Figure 1: The color scheme for Virginia is hard to see on the map. Could you alter the color or contrast so that it looks nicer and pops out more immediately?

We have adjusted the color of the Virginia inlay and added a thick black outline.

135-137: As stated here, it sounds like the 2010 restoration here is a different restoration project than the one in this study. Please clarify the study timeline.

Wording adjusted. Just the one restoration project.

140: how were cattle removed? fencing?

Yes, note added line 165.

177: should be Prior et al. (2021)

Changed.

205: I think it's spelled, "mosaicked."

Changed.

215: in line 154 you state: "the inset floodplain was considered part of the floodplain when we refer to the channel versus the floodplain in this study", but here it seems you are describing inset floodplains as being part of the channel. Please clarify and be consistent.

Inset floodplain is considered floodplain consistently. Wording adjusted on line 180 to clarify.

247: info on which transducers were operating for which events should be included somewhere. Maybe supplemental info?

No change. This is more detail than we think is necessary.

250: change "ran" to "run."

Done.

263: Start new Methods section at "Floodplain volume (m^3)…" called "Metrics of floodplain connectivity" or something similar.

Done.

286: is this the piece-wise model you mentioned in the methods? If so, state this explicitly, including the two functions used in the curve.

We added a statement clarifying that this is the piecewise fit in the description line 348. We did not add the equation for this fit because we feel it is not essential to understanding the model calibration and would add clutter to an already busy figure.

291: is this the piece-wise model you mentioned in the methods? If so, state this explicitly, including the two functions used in the curve.

Repeat comment, see our response directly above.

295: Figure 4 is not described or referenced in the text. More detail needed and more explanation as to what Figure 4 is meant to convey.

Reference added on line 288.

299-301: Do these represent changes from pre-restoration? The floodplains of each treatment are in not "notably distinct." Potentially add some annotations to the photographs to highlight the floodplains. Also, it is clear that the three treatments were taken at different times of the year – or at least treatment 3 was based on the lack of greenery. Could you take new photos from the same time of year so differences in vegetation are also apparent?

Pictures have been moved to the site pictures now that the paper is a case study. The Treatment 3 picture was replaced with one from the same time as the other supplied pictures.

313: Figure 6 – map should have a year in the caption for the satellite imagery.

We added this in the figure caption on line 377.

315: variable in space or time?

This text has been removed addressing another comment.

317-319: are the baseflow channel widths related to the treatments here, or pre-restoration differences?

Values are from 2021, we added a note to clarify this on line 396.

323: Wetted widths overtaking Treatments 2 and 3 suggest you should plot average wetted width vs flow rate, like Figure 7A. I think that would tell a better story of Treatments 2 & 3 overtaking Treatment 1 at higher flows.

This is plot 7D it is noted in line 390.

326-327: (fig 7 caption): you only describe the y-axes of these plots. please mention somewhere that x-axes for A, B, and D are total flow (cms). Also, for (C) it is unclear which of the items listed here describe "1", "2", or "3" in the figure. Need to add detail and meaningful labels to x axis. Also in the caption, specify that you are showing "modeled flows" and not "observed or measured flow rates."

Addressed in line 328.

330: please list the metrics you are referring to for clarity.

They are listed now line 402.

331: Define metrics of floodplain connectivity in the methods section – normalized volume, overbank exchange, etc.

We have added more discussion on the rationale and origin of these terms on 307-319.

339: "Overbank exchange" - this is the first use of this term. please define.

Changed to Channel-floodplain exchange for consistency.

384-386: unclear to me what you mean here by "constrained". Please elaborate. Phrasing is very awkward. Consider rewriting to make ideas explicit.

Changed to measured.

387-389: Phrasing is awkward. Consider rewriting to make ideas explicit.

Wording adjusted.

392-395: Phrasing is awkward. What does "opening floodplain sections" signify? Consider rewriting to make ideas explicit.

Wording adjusted.

Round 2 Review Comments and Author Responses

AE Comments

The case-study format appears to work well. One reviewer is satisfied with the efforts made, reviewer 2 would like to see some minor edits, and reviewer 3 requested a more extensive rewrite. Reviewer 3 points to the need for a better organization of the case study to conform with JEED guidelines for Research Case Studies – see https://jeed.pubpub.org/pub/submission-overview/release/4. They also ask for clear objectives to be outlined and a better tie-in to this work's design or management implications in keeping with the journal's scope. Therefore, I recommend a significant revision if the authors want this work to be reviewed again for publication in JEED.

We thank the reviewers for their continued support in improving this work. In response to reviewer 3's comments we have added some details as appropriate related to channel change as described in Hendrix (2022) and direct readers to the specific portions of that work which more thoroughly details this evolution. However, we feel that describing "precisely how the sites have evolved geomorphically over this time frame" is beyond the scope of this study. Our focus here is to quantify resulting channel-floodplain connectivity post restoration. We have one manuscript in process that will fully present the "morphological response" over the timeframe of the restoration from Hendrix (2022) as well as one presenting channel change between 2017-2022 based on drone-based laser scanning data. Both of these manuscripts are planned for submission to JEED in the near future (one within a month and one as part of the AEES 2023 conference special collection). We will not be forced by one reviewer to go beyond the scope of this current manuscript which would certainly impact these other two research projects/manuscripts. Finally, we have also better highlighted the key implications of this study for future restoration projects through a bulleted list in the discussion section.

Reviewer 1: Celso Bolinaga, North Carolina State University

After reading the revised manuscript and responding to the reviewers' document, I commend the authors' effort in addressing the feedback. I do not have any further comments.

Thank you.

Reviewer 2: Daniel Mecklenburg, Ohio State University

The authors have done a laudable job addressing questions and concerns. I'd like to offer for your consideration these additional minor comments.

Thank you.

204 The formation of the inset channel has vertical accretion of 0.4 m. Should this be a lateral floodplain width dimension would support point of narrowing the channel and reduced channel area, line 205? Especially since on line 206 the inset floodplain is said to be 0.45 m above the bed.

The statement we had at line 204 is correct noting that the cut banks of Treatment 1 have eroded ~1 m (laterally) combined with the formation of an inset floodplain on the opposite bank through vertical accretion of 0.4 m on average. The latter sentence on line 206 denotes the average total height of the self formed inset floodplain above the lowest point of the channel. Our wording has been adjusted to communicate this more clearly.

162, 204, and 445 What was the inset floodplain height of Treatment 3 above the bed. It may be standard practice but your entire paper hinges on this value. As you say "Grading the floodplain lower in Treatment 3 to a similar height as the self-formed inset floodplain seen in Treatment 1 could create the high exchange rate observed in Treatment 1"

Note added on line 170. The inset floodplain was graded at 0.4 m above the thalweg based on existing inset floodplains that had naturally formed before the restoration (Thompson et al., 2010; Resop et al., 2014).

318 It is not clear if the average floodplain volume is a weighted average. It seems that the channel depth and channel velocity averages are not.

The average floodplain volume, as stated at line 336, was was calculated by multiplying the average depth of all cells contained in the floodplain by the cell area. This is not a weighted average. However, we later normalize this volume (normalized floodplain volume), as stated at line 348 by dividing floodplain volume of each treatment by the volume in the channel in that treatment. The channel depth and channel velocity averages are not weighted. In this same paragraph, we mention other metrics that use the depth and velocity to compute specific discharge, which is later used to normalize or compute other metrics. No change.

323 The new unit discharge vectors along the bank have components perpendicular and parallel to the bank. The perpendicular component should not be called a "unit vector."

To clarify the language, at line 343 we now state that we are, "calculating the component of this vector which is perpendicular to the bank."

317 -318Might it be clearer to have the sentence defining unit discharge vectors appear after the one defining Floodplain volume?

This sentence was moved to right before the channel floodplain exchange definition for clarity (one sentence later than suggested).

384 …4% higher than Treatment 1 I assume.

Yes. Now clarified in the text.

385 While the metric is a fraction we switch to percentages. Now adding an additional term, "portions". This took me a minute to figure out, especially when switching from percentage on floodplain to percentage in channel. Its all good but using terms consistently would speed up my grasping of what is being said.

Changed portions to fractions for consistency.

Reviewer 3: Anand Jayakaran, Washington State University
General comments:

The case study approach makes for a better story on the geomorphic evolution of the three treatment sites over the 11 years since restoration. However, the JEED case study guidelines require "detailed, in-depth examinations of a particular case within a real-world context, typically focusing on before and after implementation of an ecological engineering design or process." What is missing from this work is precisely how the sites have evolved geomorphically over this time frame. Figure 2 shows conceptual cross-sections of the three treatment sites, which most would assume is reasonably representative of the site at the restoration time. However, the sense of what the site looks like now after 11 years is not offered explicitly. The reference to extensive inset floodplain complexes in Treatment 1 was somewhat surprising in how it was presented. One can only infer their existence from the results of the hydrodynamical modeling – but it isn't clear from any cross-sections that these exist. So can one infer that these formed over time or where they were extant at the time of restoration?

We feel that adding geomorphic evolution details is beyond the scope of this research/manuscript and would force us to encroach on other ongoing manuscrips. We feel that adding a reference to Hendrix (2022) and some details from that study is sufficent. We have added a brief summary Hendrix (2022) as part of our last revision in the case study area section. In addition, we added project images at the beginning that better show the existing geomorphological state within the three treatments.

Given all the effort to characterize the site using lidar and survey data, I would like to see a better description of the current geomorphology of the site contrasted against the typical cross sections shown in Figure 2. Create a new Figure 3 using detailed topographic data to show characteristic channel cross-sections in the three treatment reaches.

Again, this is beyond the scope of this manuscript. While we do have extensive data and research related to channel channel over time, the details of those results are being presented in two different manuscripts. We will not endanger those studies/manuscripts.

The next step would be to use the hydrodynamic model to explain why Figure 2 (restoration) now looks like Figure 3 (restoration + 11 years). Cite relevant information from Hendrix (2022). By the way, Hendrix (2022) is not on the list of references, so I could not determine what information they outline. But it is clear from the response-to-reviewers that some sediment data were collected and can be used to help explain channel evolution at the sites.

The citiation has been added back on line 623, apologies for the confusion. Our automated reference software must have removed it when the citation was edited outside of Word. The hydrodynamic model is currently built for the system in 2022. As above explaining the geomorphic change over time is beyond the scope of this manuscript as are any estimates of changes in sediment dynamics. As mentioned above, much of this information is being used in preparation of two addition manuscripts. The publication resulting from Hendrix (2022) will delve into morphodynamic and the evolution of the channel between 2011 and 2022 . We have some of Hendrix (2022) findings in the case study area section. Such a model is outside the scope of our work and that our analysis of the current system hydraulics provide enough insight into the system dynamics without this additional analysis.

The methods outlined to develop, calibrate, and validate the model are excellent and appear similar to those outlined in Czuba et al. (2019). However, what is not clear is how the results from the model give the reader insight into how best to restore a site, given the geomorphic context of this stream. The entire discussion is devoted to why specific hydrodynamic modeling results arrived. However, what does this all mean for the evolution of a stream restoration project when three restoration options are tried? I know this information is buried, but it needs to be stated explicitly. The closest to offering meaningful design implications is given in one sentence, the last sentence in the manuscript: "With these considerations in mind, practitioners should move forward with the knowledge that for this system providing adequate space and time for adjustment, the stream made significant adjustments and a decade later exhibited similar or higher channel-floodplain connectivity compared to both common regrading techniques."

We offer the pros and cons of regrading and stabilization as determined by this study in lines 413-423 and 439-466. This work explicitly states that avoiding stabilization where possible and adding heterogeneity into floodplain designs may promote floodplain connectivity. We have further highlighted these implications with a bulleted list of actionable considerations for future restoration designers (line 487-497) and have reiterated them in the conclusion (line 520-528). We have also included bullets related to research areas being explored as part of our overall research effort that are, however, beyond the scope of this study/manuscript.

Specific comments:

Still need a statement of objectives at the end of the introduction - what do you intend to show in this case study, and what about the science do you intend to further?

We did not explicitly call this a "statement of objectives" but instead the "purpose of this case study" starting at lines 124 to 133. We have changed "purpose" to "objective." As mentioned in the last sentence of this paragraph, the science we intend to further relates to stream and floodplain restoration design.

Lines 322-325: Confusing. The words "unit" and "vectors" are overused. Please use terms that Czuba et al. (2019) use and explain concisely but in more technical terms. Maybe show it in a diagram or formula format. When you say bank here, are you referring to what Czuba et al. (2019) state as "bank lines mark the river channel-floodplain boundary"?

Unit discharge has been changed to "specific discharge" for consistency with Czuba et al. (2019). Our wording was also adjusted for clarity. The use of formulas extended the explanation significantly and we have been asked to shorten the methods section. With this considered, we have opted to use in text descriptions alone and point to Czuba et al (2019) for a more in depth description of the calculation.

Figure 3: Change the "Sensor fit" symbol to something more distinct from "velocity sensor data."

Symbol changed to stand out more.