Review Report for

Sediment transport and flood discharge in the Menominee River, Illinois, USA

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Round 1 Review

Comments by reviewer 1 (anonymous)

Completed: 2024-06-13 08:49 PM
Recommendation: Major revisions
Reviewer Comments

For author and editor

The manuscript is overall well-constructed, but I find it challenging to discern the motivation behind this study. I recommend major revisions according to the criterion of Geomorphica, which highlights that "the motivation for the work is poorly explained."

This appears to be a case study that reads more like a report, likely due to inadequate development of the knowledge gap and the study's advancements. Consequently, the excitement level for both authors and audience seems diminished.

I will list some places where this could be improved.

Abstract:

The manuscript begins abruptly with "this case study." It would be beneficial to first outline the problem, the associated challenges, proposed solutions, and then introduce the case study.

The abstract concludes rather abruptly. It should articulate the implications of the findings and specify which knowledge gaps have been addressed.

The abstract requires more detailed information, such as specifying the size of the "largest sediment" mentioned in Line 11, as interpretations of size may vary among readers.

In line 13, it is unclear what characteristics are “similar”.

Introduction

Line 42: Again, “this case study” appears too abrupt, which makes the significance of this study limited to a very local study.

Line 84 to 95: There is not a clear problem statement, leaving readers without a focused context for the content that follows.

Other comments

Line 168: “Cross Sections” – I don’t think this is a methodology. A more appropriate title might be "Measurements of Depth, Velocity, and Slope" or any equivalent titles for this section.

Line 183: It is unclear to me what “starting bank” refers to here. The labeling of L1 is confusing, as it appears to denote a vertical distance. Consider using a double arrow to clearly indicate the length of L1.

Line 184: where is D2 in Figure 3? Consider using a different variable, as D2 may get mixed with other grain size variables with similar subscripts.

Line 194: All variables from this point onward should be italicized for consistency.

Figure 4: what is the size of the scale bar in both photos?

Line 258-289: This block of text deserves a title as Section 4.1.

Line 217: It is beneficial to introduce why D95 and D84 are used and what physical meaning these two grain sizes have. Also, in Equations 4 and 5, I did not see them, but d.

Line 331: D50 sediments – The introduction of D50 sediments is abrupt. Provide more context before discussing this specific grain size.

Figure 9: so is there a relationship between clast size and distance displaced? It is currently not analyzed.

Figure 10: the meaning of the diagonal line should be mentioned. It is a 1:1 line for compare critical and basal shear stress.

Table 1: why specific dates are not provided for measurements taken in June and July?

Line 406: Justification is needed for citing other studies if new results are not presented. Consider integrating Supplementary Table 2 into the main text to enrich this results section.

Conclusions

The conclusion should revisit the knowledge gaps initially identified and detail how they have been addressed. This will help clarify the manuscript's contributions and implications.

Comments by reviewer 2 (anonymous)

Completed: 2024-06-24 04:02 PM
Recommendation: Major revisions
Reviewer Comments

For author and editor

Review of: “Sediment transport and flood discharge in the Menominee River, Illinois, USA”

Submitted to Geomorphica

6/24/2024

Summary:

The submitted manuscript – a “case study” – involves the description of data collected at four cross-sections along a 1000-to-2000-foot reach of the Menominee River in Illinois. The stated goal of the study is to understand sediment transport and channel evolution that may be impacted by land use and climate change. Channel transects, discharge (velocity) measurements, shear stress/stream power calculations, and grain size/painted rock measurements were used to assess the channel change and sediment transport potential of the river from six separate field visits. While there is nothing wrong with the data set presented, my feeling is that the data set is fairly basic and lacking in spatial extent to provide a broader geomorphic perspective of processes of sediment transport and channel change in the watershed, especially as it pertains to possible land use and climate change effects. The results are mostly a presentation of the field data, with some analysis of clast motion based on painted rock studies and calculations of shear stress/stream power. One interesting result was that the authors demonstrated that the reach was losing discharge upstream, and gaining discharge downstream, perhaps due to interaction with carbonate (karst) bedrock.

In the discussion section, the authors compare the results to some other regional rivers. They note that the channel of the Menominee tends to be smaller than other regional rivers, although the exact reason is unclear. There is then a substantial discussion of sediment transport and channel geometry. It would have been useful to see the cross sections presented relative to a common elevation (supplementary figure 4) so the reader could evaluate whether any significant channel change occurred during the various flood events. The authors present no quantitative analysis of channel change at their repeat cross sections. The painted rock analysis only partly matched the predictions of basal versus critical shear stress for the various flows (bankfull and flood flows), and calculations of the discharge necessary to move D84 and D95 clasts were exceedingly high compared to observed clast motion and flood discharges; although the recurrence interval of these floods is unclear. Thus, the reader is left without much insight into the overall sediment transport and channel change dynamics of the reach, other than to say it is variable across these four cross sections. The discussion

also includes a section on implications for changing land use and climate, but I see little in the data presented from this study that could tease out any of those effects, and this section reads more like a review on the likely outcomes as presented in the literature.

In summary, while the data set presented here certainly provides a nice baseline for this particular reach for longer-term monitoring, there is not much significance or novelty to the results as presented that would be of more wide applicability. If the authors could somehow tie their data set into a more robust assessment of the watershed – for example, using repeat aerial photos to document channel change across a larger range of reaches in different parts of the watershed; or developing a more robust hydraulic-sediment transport model (e.g. HEC-RAS; iRIC) using more detailed topographic surveys, and incorporating gaining/losing streamflow - then perhaps this case study would more aptly provide a model for developing a long-term stream monitoring site. Further, the results presented are not clearly linked to broader implications - even though climate change and land use are discussed, I do not see how the results provide insight into how those changes could be understood from the perspective of data collected in this study. Unfortunately, in my opinion, this work does not meet the criteria for publication in this journal in its current form, and would need major revisions and additional analysis to warrant publication.

Line and Figure Comments:

Line 165: It seems more like four cross sections at one location – why was this section chosen? Is this section representative of the larger watershed? Is it possible to measure at one location in a watershed and apply that to the watershed in general?

Lines 387-402: Were the floods of the Sinsinawa abnormally high? 50-year or 100-year floods? This information should be available if there is a long-term gaging station. Could some context be provided as to the recurrence interval of these floods to better understand how to consider these events relative to the probability of sediment transporting flows?

Line 410: According to Supplementary Table 2, site 3 did have its highest stream power during the September flood. Its unit stream power wasn't highest then, but neither was site 4.

Line 426: I don't know how much can be said about channel geometry within the watershed with only 4 cross sections along one reach.

Line 480: It would have been useful to quantify channel change, and present the cross-sections with a common elevation datum so the reader could view the changes.

Lines 503-504: Aren't your discharges based on measurements of flood height at the cross section using Manning's equation? Wouldn't this account for the additional discharge at gullies because high water marks were used?

Section 5.3: It is not clear exactly how the data presented in this study provides insight into these issues. Can you directly connect your data sets to these effects, other than the apparent minimal effect of the subdivision at your site?

Line 542: How do you know if was related to runoff from the subdivision, and not just changing surface water-groundwater dynamics during different flow levels? I would expect less loss during higher discharge when the groundwater levels are also likely higher. Is there any direct data from the neighborhood runoff?

Lines 564-566: I do not understand this statement. How does sediment availability affect this calculation? Also, did the authors consider how spatial variation in shear stress across the channel could affect sediment transport?

Figure 1. Ensure legend is readable at for size of the figure in the manuscript.

Figure 5 c/d: Should y-axis units be centimeters?

Figure 9: were there no painted clasts at site 3?

Table 2: Blue River width given as 22-18 – is this correct?

Recommendation by Associate Editor 2 (anonymous)

Completed: 2024-06-28 02:04 PM
Recommendation: Reject
Reviewer Comments

For author and editor

Dear authors,

I read with interest your manuscript entitled “Sediment transport and flood discharge in the Menominee River, Illinois, USA” in addition to the two anonymous reviewer reports. Based on my reading of your manuscript and the reviews, I would recommend for the manuscript be declined, but I would encourage a resubmission.

Writing and reviewing a case study type submission, compared to the more common research article can be challenge. And I think that some of this is reflected in the comments by both reviewers. I agree with both reviewers that additional context around the motivation and purpose for the present study and that this needs to be focused at a scale that is supported by the data presented is needed. For example, as R2 describes in more detail, with the datasets in hand it is difficult to see how an argument about climate change implications or land use change is warranted. R1 (and R2) also suggest that the novelty and inspiration for this study needs to be strengthened, in addition to drawing out the implication for these findings (at the appropriate scale). While additional datasets are always fantastic, they are not always feasible. Both reviewers provided suggestions for additional work, but that might be beyond the scope of this study. Borrowing from R2, perhaps the authors could consider how to frame the 2019 data collection as a baseline case study – ‘here’s what is happening now’, and provide some additional context with respect to the flow regime broadly and how that might impact event-scale and reach-scale changes (which have implications on ecosystem services).

Additionally, both reviewers suggest some organizational revisions. I agree that this is needed and aligns with the primary suggestions of strengthening the context/motivation for this study, why it is novel, and how it may inform future work. Additionally, re-organization and removing unnecessary information would help ensure the submission is closer to the typical 6000 word length (at present I believe it is well over 6000 words, I don’t believe this additional text is warranted).

Best wishes

Anonymous Associate Editor

Authors' responses to review comments

January 9, 2025

Dear Editors and Reviewers,
Below are the original comments from reviews of our manuscript. We converted individual comments and suggestions into bullet points to work through the list and added our actions or arguments in the sub-bullets and italicized text.
Thank you for the helpful feedback and suggestions to make this manuscript stronger. Please let us know how we can continue to improve this work for potential publication in Geomorphica.

Anonymous Associate Editor:
• Additional context around the motivation and purpose for the present study and that this needs to be focused at a scale that is supported by the data presented is needed. For example, as R2 describes in more detail, with the datasets in hand it is diIicult to see how an argument about climate change implications or land use change is warranted.
o Section 5.3 was revised to cut out the background literature and track changes that could be observed.
• R1 (and R2) also suggest that the novelty and inspiration for this study needs to be strengthened, in addition to drawing out the implication for these findings (at the appropriate scale).
o In the introduction, we kept the implications for land use and climate change more general, as the research is needed for these purposes, but also present the local, immediate need based on recent events and impacts to the local community as the motivation for this work.
• While additional datasets are always fantastic, they are not always feasible. Both reviewers provided suggestions for additional work, but that might be beyond the scope of this study. Borrowing from R2, perhaps the authors could consider how to frame the 2019 data collection as a baseline case study – 'here's what is happening now', and provide some additional context with respect to the flow regime broadly and how that might impact event-scale and reach-scale changes (which have implications on ecosystem services).
o To address this concern, we emphasized that this is a baseline study in the introduction. We also added a figure analysis of GoogleEarth that was initially compiled by students to study changes to the channel position over ~30 years.
• Additionally, both reviewers suggest some organizational revisions. I agree that this is needed and aligns with the primary suggestions of strengthening the context/motivation for this study, why it is novel, and how it may inform future work. Additionally, re-organization and removing unnecessary information would help ensure the submission is closer to the typical 6000 word length (at present I believe it is well over 6000 words, I don't believe this additional text is warranted).

o We worked to more concisely explain the benefit of this work. We cut sections from the discussion to improve organization. We identified repeated statements to make sure the document was better organized, more clear, more concise, and brought the word count closer to the limit.

Reviewer 1:

o Explain the motivation: The manuscript is overall well-constructed, but I find it challenging to discern the motivation behind this study. I recommend major revisions according to the criterion of Geomorphica, which highlights that "the motivation for the work is poorly explained."

§ In the introduction, we explained that the goal is to collect baseline data and connected the need to understand river systems in this area because of flood events that occurred in the last 10-15 years.

• This appears to be a case study that reads more like a report, likely due to inadequate development of the knowledge gap and the study's advancements. Consequently, the excitement level for both authors and audience seems diminished. I will list some places where this could be improved.

• Abstract:

o The manuscript begins abruptly with "this case study." It would be beneficial to first outline the problem, the associated challenges, proposed solutions, and then introduce the case study.

o Added a sentence to state a more general problem.

o The abstract concludes rather abruptly. It should articulate the implications of the findings and specify which knowledge gaps have been addressed.

o Revised the abstract to add a closing sentence and more clearly state the outcomes of this project.

o The abstract requires more detailed information, such as specifying the size of the "largest sediment" mentioned in Line 11, as interpretations of size may vary among readers.

o Revised to use cobbles as a reference to the sediment size.

o In line 13, it is unclear what characteristics are “similar”.

o Removed this sentence to clarify and specify findings as recommended in a previous comment.

• Introduction

o Line 42: Again, “this case study” appears too abrupt, which makes the significance of this study limited to a very local study.

o The introduction was significantly restructured, and the transition to introduce the study was revised to make it less abrupt.

o Line 84 to 95: There is not a clear problem statement, leaving readers without a focused context for the content that follows.

o We provided more specific context to the regional problems to explain the purpose of this work.

• Other comments

o Line 168: “Cross Sections” – I don’t think this is a methodology. A more appropriate title might be "Measurements of Depth, Velocity, and Slope" or

any equivalent titles for this section.
o Revised to Depth, Velocity, and Slope Measurements
o Line 183: It is unclear to me what “starting bank” refers to here. The labeling of L1 is confusing, as it appears to denote a vertical distance. Consider using a double arrow to clearly indicate the length of L1.
o Revised the text to clarify length, modified figure to remove arrows and add new arrows to indicate length and depth.
o Line 184: where is D2 in Figure 3? Consider using a diIerent variable, as D2 may get mixed with other grain size variables with similar subscripts.
o Revised the figure and text to define depth as H.
o Line 194: All variables from this point onward should be italicized for consistency.
o Italicized variables.
o Figure 4: what is the size of the scale bar in both photos?
o Added a note to the figure caption. The scale bar is 6 inches.
o Line 258-289: This block of text deserves a title as Section 4.1.
o Moved the 4.1 heading to the top of the results section. Changed the heading title to incorporate descriptive observations of the channels and the single paragraph summarizing velocity and discharge into this section.
o Line 217: It is beneficial to introduce why D95 and D84 are used and what physical meaning these two grain sizes have. Also, in Equations 4 and 5, I did not see them, but d.
o Adjusted this with added text in line 218 and 226-227.
o Explained how d represents the grain size and that we compared both the 84th and 95th percentile values in our calculations to compare results.
o Also added an explanation in an earlier paragraph in the methods to define these variables and communicate how they will be used later in the paper.
o Line 331: D50 sediments – The introduction of D50 sediments is abrupt. Provide more context before discussing this specific grain size.
o We added a reference to the median sediment size earlier in this paragraph. We also added notes about grain size calculations in the methods to introduce these terms earlier.
o Figure 9: so is there a relationship between clast size and distance displaced? It is currently not analyzed.
o We looked at adding trendlines to the data, but the graph tells the story of movement throughout the field season. Combining the movement of clasts across the dates that we sampled did not seem to make the most sense, since the movement would represent variable conditions. Trying to calculate a correlation did not seem valid with the limited data.
o Figure 10: the meaning of the diagonal line should be mentioned. It is a 1:1 line for compare critical and basal shear stress.

o Added a sentence to the figure caption to explain meaning of the diagonal line.
o Table 1: why specific dates are not provided for measurements taken in June and July?
o Included June and July dates in Table 1
o Line 406: Justification is needed for citing other studies if new results are not presented. Consider integrating Supplementary Table 2 into the main text to enrich this results section.
o References here were moved to the Methods because they were referencing that we used the same calculations to quantify these values as the other studies.
o Added Supplementary Table 2 as Table 1 in the main text of the document where it was first referenced.
• Conclusions
o The conclusion should revisit the knowledge gaps initially identified and detail how they have been addressed. This will help clarify the manuscript's contributions and implications.
o We revised the conclusions to specifically address each of our goals and put the project into the broader context of need for collecting these types of data.

Reviewer 2:
Summary:
The stated goal of the study is to understand sediment transport and channel evolution that may be impacted by land use and climate change. The results are mostly a presentation of the field data, with some analysis of clast motion based on painted rock studies and calculations of shear stress/stream power. One interesting result was that the authors demonstrated that the reach was losing discharge upstream, and gaining discharge downstream, perhaps due to interaction with carbonate (karst) bedrock.
o Why is the Menominee River smaller than other regional rivers? In the discussion section, the authors compare the results to some other regional rivers. They note that the channel of the Menominee tends to be smaller than other regional rivers, although the exact reason is unclear. There is then a substantial discussion of sediment transport and channel geometry.
§ In an area with multiple bedrock lithologies and karst influence these systems don’t behave the same way. Even though some of these watersheds neighbor each other, there are other lithologic controls. We tried to communicate these di[erences are related to karst processes, too. Another potential factor on size of this watershed and river could be related to Quaternary reorganization of the Mississippi River, but this is just speculation and questions for future work. To incorporate the complex glacial history around this region would require too much context to fit into a case study.
§ Part of the incorporation of these other regional rivers were in response to previous submission for review where a reviewer did not

think there was enough comparison and reporting on other rivers and previous work done in this region.
o It would have been useful to see the cross sections presented relative to a common elevation (supplementary figure 4) so the reader could evaluate whether any significant channel change occurred during the various flood events. The authors present no quantitative analysis of channel change at their repeat cross sections.
o These comments are related to a specific comment below. On our initial field visit, we did leave markers in the field to identify the exact location of our cross-section surveys to return for subsequent observations. Unfortunately, those markers were disturbed prior to our second visit to the site. We completed surveys each time to complete calculations of discharge and in a previous submission to a di[erent journal, a reviewer recommended that we include the cross sections and discuss the channel geometry. We include them here in response to that recommendation in the supplementary data because we have the data to share, but we recognize that we cannot directly compare the changes due to the disturbance to our markers.
o The painted rock analysis only partly matched the predictions of basal versus critical shear stress for the various flows (bankfull and flood flows), and calculations of the discharge necessary to move D84 and D95 clasts were exceedingly high compared to observed clast motion and flood discharges; although the recurrence interval of these floods is unclear. Thus, the reader is left without much insight into the overall sediment transport and channel change dynamics of the reach, other than to say it is variable across these four cross sections.
o In the Discussion referenced here, we removed the repeated information that was already presented in the results and focused on a discussion of what was and was not predicted and how this indicates that there are improvements that can be made to these methods for predicting sediment transport in the future. We observed that sediments moved at lower discharges than expected when comparing our results in Figure 10 where we compared the critical and basal shear stresses. We did not expect sediment to move, but we did see the sediment move.
o The discussion also includes a section on implications for changing land use and climate, but I see little in the data presented from this study that could tease out any of those eIects, and this section reads more like a review on the likely outcomes as presented in the literature.
o This section was revised as noted elsewhere and a figure was added to include a more specific assessment on where change has occurred.
o In summary, while the data set presented here certainly provides a nice baseline for this particular reach for longer-term monitoring, there is not much significance or novelty to the results as presented that would be of more wide applicability.
o We focused more on the introduction that this work serves as a baseline for future work. We also note in the introduction that sediment transport and changing morphology of river channels is a secondary outcome of climate change, and to fully understand the changes the occur because of climate change requires making all data available, even if results do not support the

hypothesis of climate influence as noted in East et al., 2022.
o If the authors could somehow tie their data set into a more robust assessment of the watershed – for example, using repeat aerial photos to document channel change across a larger range of reaches in diIerent parts of the watershed; or developing a more robust hydraulic-sediment transport model (e.g. HEC-RAS; iRIC) using more detailed topographic surveys, and incorporating gaining/losing streamflow - then perhaps this case study would more aptly provide a model for developing a long-term stream monitoring site. Further, the results presented are not clearly linked to broader implications - even though climate change and land use are discussed, I do not see how the results provide insight into how those changes could be understood from the perspective of data collected in this study.
o At this time, we do not have the capacity to incorporate more suveys or models, however, as preliminary work, we did look at channel migration over time in GoogleEarth over about 30 years. These analyses were updated to include the changes that occurred in the year after the data were collected for this study and incorporated into section 5.3 to help document some change.
o We included a graph and discussion in section 5.3 to show increases in recurrence interval estimates for the Sinsinawa River since 1967 to help project how this is related to broader implications as well as connect to the need to collect baseline data as discussed in the introduction.
Line and Figure Comments:
o Line 165: It seems more like four cross sections at one location – why was this section chosen? Is this section representative of the larger watershed? Is it possible to measure at one location in a watershed and apply that to the watershed in general?
o We revised the text at this location to explain that we had permission to collect data at this site. This river flows entirely through private property.
o Lines 387-402: Were the floods of the Sinsinawa abnormally high? 50-year or 100-year floods? This information should be available if there is a long-term gaging station. Could some context be provided as to the recurrence interval of these floods to better understand how to consider these events relative to the probability of sediment transporting flows?
o We added a statement in the introduction from a hazards mitigation report for Jo Daviess County indicating how frequently flash floods occur in this region and put into context of their evaluation that these events may happen twice each year and heavy precipitation events have increased in historic data.
o We added a note to Figure 12 caption to indicate that the two peak flows in September and October 2019 had 5-year recurrence intervals.
o We added a discussion of recurrence interval change over 59 years in section 5.3.
o Line 410: According to Supplementary Table 2, site 3 did have its highest stream power during the September flood. Its unit stream power wasn't highest then, but neither was site 4.
o We reviewed and revised this section to accurately summarize the data in the

included table.
o We note that this table is now in the main body as recommended by Reviewer 1.
o Line 426: I don't know how much can be said about channel geometry within the watershed with only 4 cross sections along one reach.
o I revised the reference to channel geometry in 5.1 to support the regional context but also tried to minimize the discussion of this characteristic.
o In a previous submission, a reviewer had very much asked for more connections to other regional studies and the inclusion of channel geometry was to help relate to the studies that the reviewer wanted me to reference.
o Line 480: It would have been useful to quantify channel change and present the cross-sections with a common elevation datum so the reader could view the changes.
o We had markers in the field to return to our sites, but they were disturbed before the floods occurred. We added a sentence to the methods to communicate this disturbance.
o Lines 503-504: Aren't your discharges based on measurements of flood height at the cross section using Manning's equation? Wouldn't this account for the additional discharge at gullies because high water marks were used?
o This statement was removed through revisions of section 2. Yes, the highwater marks were used. This referred more to the complexity of flow from directly upstream or from a perpendicular channel feeding into the larger channel.
o Section 5.3: It is not clear exactly how the data presented in this study provides insight into these issues. Can you directly connect your data sets to these eIects, other than the apparent minimal eIect of the subdivision at your site?
o We moved literature references to support the problem statement in the introduction and focus this section on the outcomes of this study.
o We also added a figure capturing channel changes in GoogleEarth.
o Line 542: How do you know if was related to runoI from the subdivision, and not just changing surface water-groundwater dynamics during diIerent flow levels? I would expect less loss during higher discharge when the groundwater levels are also likely higher. Is there any direct data from the neighborhood runoI?
o We included a statement of observations of the gully; however, we note that we did not directly observe the flow during the flood. This is expected since we acknowledged early that we were not observing the flood directly due to time to travel to the field site. The overland flow in a developed area would be expected to reach the gully and stream channel sooner than the groundwater.
o Lines 564-566: I do not understand this statement. How does sediment availability aIect this calculation? Also, did the authors consider how spatial variation in shear stress across the channel could aIect sediment transport?
o In this section, we clarified this sentence to relate it to the controls on sediment available for transport in this system. A river could move very large sediment, but if large sediment just isn't available because of the bedrock

limitations, then it's not a useful method for estimating discharge.
o Figure 1. Ensure legend is readable at for size of the figure in the manuscript.
o Increased the font size of the legends and revised legends to make space for the larger text.
o Figure 5 c/d: Should y-axis units be centimeters?
o The graph originally generated from the National Weather Service presented the precipitation numbers in inches. I converted the values to cm on the figure. I also edited the format of the x axis labels on figures a and b to match the format on figures c and d.
o Figure 9: were there no painted clasts at site 3?
o No, we did not paint the clasts at site 3.
o Table 2: Blue River width given as 22-18 – is this correct?
o Yes, this width is correct. Revised the table to show smaller to larger sizes to match the format for the other rivers.

Round 2 Review

Comments by reviewer 1 (anonymous)

Completed: 2025-01-27 03:30 AM
Recommendation: Accept
Reviewer Comments

For author and editor

I applaud the authors for well addressing all the comments from me and the other reviewer. I have no further comments and recommend publication in its current form.

Comments by reviewer 2 (anonymous)

Completed: 2025-02-21 06:22 PM
Recommendation: Minor revisions
Reviewer Comments

For author and editor

Review of revision to: “Sediment transport and flood discharge in the Menominee River, Illinois, USA”

Submitted to Geomorphica

2/20/2025

Summary:

This manuscript represents the revision of a previous manuscript. As per the response to reviewers, the manuscript had previously been reviewed at another journal, and thus the manuscript has undergone significant revision and editing. To briefly recap, the authors present a case study – or to some extent baseline study – of a 1000-to-2000-foot reach of the Menominee River in Illinois using channel transects, discharge (velocity) measurements, shear stress/stream power calculations, and grain size/painted rock measurements to assess the channel change and sediment transport potential of the river from six separate field visits. The authors clearly made extensive revisions to the paper following the suggestions of the reviewers, but do not provide any additional analysis. At this time, I believe the manuscript still needs further revision and clarification of some sections before it is suitable for publication. The paper could also be shortened further if its main purpose is to provide a baseline study, and I provide some suggestions below.

Comments:

Section 3.1. Why not include width in the title here? Consider “channel geometry and velocity measurements”

Equation 1/Figure 3: Consider whether this equation and figure could be removed to reduce the total number of figures. To me this describes the mid-section method and a reference to standard USGS methods would suffice.

Section 3.3. It is not clear why the authors are using the largest clast method here when there are high-water marks or high-water observations for which Manning’s equation can be applied and is more accurate (as shown by the data herein). What is the purpose of presenting the largest clast

method in this study, especially given that the authors already state here in the methods that it overpredicts velocity? Explain why this is included or consider removing these calculations because the discussion surrounding these different discharge calculations distracts the reader from the key conclusions.

4.1. This is the first the reader has heard of the “active” channel. This nomenclature can have different meanings. Consider whether this wording is needed or should be introduced and defined earlier.

Section 4.3. See comment above. I am just not sure this adds much value to the story being presented. It would be more useful if the author’s plotted their own largest clast data versus discharge or estimated velocity from Manning’s equation and provided their own version of the largest clast equation for the streams in their region.

Section 4.4. I don’t think there needs to be a separate stream power section. Include this in the discussion of shear stress and clast size? I worry that this reads as a list of hydraulic parameters without much context on their distinct utility to address a hypothesis or problem statement.

Section 5.1. It seems to me the Manning’s predictions are reasonable compared to the Sinsinawa River; and as your discussion suggests was likely related to precipitation differences in the watersheds. This discussion could be streamlined.

426-433: Should some of this go in the channel geometry section?

Section 5.2. This discussion could be shortened and simplified as there is redundancy with the results section.

463: delete “observations”

482-485: This doesn’t make sense to me: “If sediment sizes produced by bedrock weathering and erosion are smaller than the stream can potentially transport, calculations of the river’s overall efficiency based on bedload could underestimate actual values”.

What is the river’s efficiency? Sediment transport capacity as defined by a shear stress or stream power value? Why would smaller sizes be less transportable…”smaller than the stream can transport”? Are you trying to say the river can transport larger particles than suggested by the shear stress calculation? Or are you saying that finer sediments are transported through the system, even if there is coarser bed material…as in suspended load versus bed load.

493-503: I don’t know that this is needed. I think the Manning’s equation is pretty well proven to work well when calibrated at a site with a simple channel.

495-496: Are these your n-values reported in parentheses, or the standard values? I don't believe the calibrated n values were presented in the results.

497-498: “even using the standard values from Chow (1959)” – if the Chow values of n are less than your values, how could you not get an even higher discharge. Consider deleting this section. If you include a discussion about large grains, also consider discussing the role of the relative grain size and bed configuration.

505: “additional factors” – please elaborate.

Figs 13/14: These figures are useful to provide some more context.

563-564: I thought the two rivers were about as expected, except for the largest grain velocity discharge calculation that was an overestimate.

Recommendation by Associate Editor 2 (anonymous)

Completed: 2025-03-07 10:14 PM
Recommendation: Accept
Reviewer Comments

For author and editor

Dear Authors,

I really enjoyed reviewing and read with interest your revised manuscript entitled “Sediment transport and flood discharge in the Menominee River, Illinois, USA” in addition to the two anonymous reviewer reports. Based on my reading of your manuscript and the reviews, I would recommend for the manuscript be accepted, but invite you to consider the minor comments Reviewer 2 included.

I applaud the revisions undertaken. Reviewer 2 has a few suggestions, you might consider as a means to clarify and strengthen your argument.

I really appreciate the work and effort that went into the revisions and can’t wait to see this paper published. Lastly, I appreciate your patience, my schedule delayed the decision on your paper and I apologise for this inconvenience, it is in no way a reflection of your work or the aims of the journal.

Sincerely,

Anonymous Associate Editor

Authors' responses to review comments

April 12, 2025

Dear Editors and Reviewers,

Thank you for your time and consideration of our manuscript, Sediment transport and flood discharge in the Menominee River, Illinois, USA. We very much appreciate the feedback and recommendations for minor revisions.
Below we copied the comments and recommendations from reviewers into a bullet list of points to address. We added our actions or arguments in the sub-bullets with italicized text.
Please let us know if you recommend any additional changes to the manuscript. Thank you again for your consideration.
Reviewer Comments:
• Section 3.1. Why not include width in the title here? Consider "channel geometry and velocity measurements"
o Accepted these changes.
• Equation 1/Figure 3: Consider whether this equation and figure could be removed to reduce the total number of figures. To me this describes the mid-section method and a reference to standard USGS methods would suffice.
o Revised to remove Figure 3 and Equation 1. Updated figure labels. Updated equation labels. Added reference to USGS methods by Turnipseed and Sauer, 2010.
• Section 3.3. It is not clear why the authors are using the largest clast method here when there are high-water marks or high-water observations for which Manning's equation can be applied and is more accurate (as shown by the data herein). What is the purpose of presenting the largest clast method in this study, especially given that the authors already state here in the methods that it overpredicts velocity? Explain why this is included or consider removing these calculations because the discussion surrounding these different discharge calculations distracts the reader from the key conclusions.
o Removed this section.
• 4.1. This is the first the reader has heard of the "active" channel. This nomenclature can have different meanings. Consider whether this wording is needed or should be introduced and defined earlier.
o Changed the terms from active flow to baseflow.
• Section 4.3. See comment above. I am just not sure this adds much value to the story being presented. It would be more useful if the author's plotted their own largest clast data versus discharge or estimated velocity from Manning's equation and provided their own version of the largest clast equation for the streams in their region.
o We removed references in this section related to the discharge to move large sediments.

• Section 4.4. I don't think there needs to be a separate stream power section. Include this in the discussion of shear stress and clast size? I worry that this reads as a list of hydraulic parameters without much context on their distinct utility to address a hypothesis or problem statement.
o Incorporated this information into section 4.2.
• Section 5.1. It seems to me the Manning's predictions are reasonable compared to the Sinsinawa River; and as your discussion suggests was likely related to precipitation differences in the watersheds. This discussion could be streamlined.
o We revised the first paragraph to remove references to the results to reduce repetition from previous sections.
• 426-433: Should some of this go in the channel geometry section?
o I chose not to include this section in the channel geometry section because this is a comparison of width and how it relates to discharge and rock characteristics in a regional context in Section 5.1. Section 5.2 focuses on the more local variations along the Menominee River. By taking out the repetition of results from Section 5.2 as suggested below, I think that helps to emphasize the regional versus local differences. I also will remove channel geometry from the heading in Section 5.2 to help remove that focus.
• Section 5.2. This discussion could be shortened and simplified as there is redundancy with the results section.
o Cut much of the first paragraph where the results on sediment transport and channel observations were repeated.
• 463: delete "observations"
o deleted
• 482-485: This doesn't make sense to me: "If sediment sizes produced by bedrock weathering and erosion are smaller than the stream can potentially transport, calculations of the river's overall efficiency based on bedload could underestimate actual values".
• What is the river's efficiency? Sediment transport capacity as defined by a shear stress or stream power value? Why would smaller sizes be less transportable…"smaller than the stream can transport"? Are you trying to say the river can transport larger particles than suggested by the shear stress calculation? Or are you saying that finer sediments are transported through the system, even if there is coarser bed material…as in suspended load versus bed load.
o For the two points above, we revised the sentences in the associated paragraph to make the wording more concise, clear, and specific. We also deleted the first sentence of the last paragraph to remove possible repetition from the adjustments made to this paragraph.
• 493-503: I don't know that this is needed. I think the Manning's equation is pretty well proven to work well when calibrated at a site with a simple channel.
• 495-496: Are these your n-values reported in parentheses, or the standard values? I don't believe the calibrated n values were presented in the results.
• 497-498: "even using the standard values from Chow (1959)" – if the Chow values of n are less than your values, how could you not get an even higher discharge.

Consider deleting this section. If you include a discussion about large grains, also consider discussing the role of the relative grain size and bed configuration.
o We removed this entire paragraph which related to the concerns in the three bullets above. This paragraph discussed the calculations from section 3.3 that the reviewer also recommended removing. The n values calculated in this study are included in the supplementary data table.
• 505: “additional factors” – please elaborate.
o The additional factors were referencing the suggestion of the RFID tracking that was already mentioned in the previous section. As noted above, we also removed this first sentence of the paragraph to avoid repetition from an earlier paragraph.
• Figs 13/14: These figures are useful to provide some more context.
• 563-564: I thought the two rivers were about as expected, except for the largest grain velocity discharge calculation that was an overestimate.
o We revised this sentence to note the differences that were likely related to spatial precipitation patterns.