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2022WR032039 (Editor - Jim Hall): Decision Letter
1 message

wrr@agu.org <wrr@agu.org>
Reply-To: wrr@agu.org
To: fernando.aristizabal@noaa.gov

Fri, May 27, 2022 at 6:58 AM

Dear Dr. Aristizabal:

Thank you for submitting "Reducing Horton-Strahler Stream Order Can Enhance Flood Inundation Mapping Skill with Applications for the U.S. National Water Model" [Paper #2022WR032039] to Water Resources Research. I have received 3 reviews of your manuscript, which are included below and/or attached. As you can see, the reviews indicate that major revisions are needed before we can consider proceeding with your paper. I am therefore returning the paper to you so that you can make the necessary changes.

Please submit a revised manuscript that addresses the reviews and any editorial comments by July 26, 2022.

In your revision, please follow our Checklist and use our Templates for the main file and any supplements. Please provide the following:

1. A response to reviewer file that lists each major comment and describes how the manuscript has/has not been modified in response to those comments.

2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes).

3. The final revised manuscript with changes incorporated and separate final figure files (figure parts should be combined into a single file), which will be used for publication if the manuscript is accepted. If final figures are already uploaded, they can be easily copied over to the next revision version.

4. If any, supporting information text, figures, captions, and small tables in single PDF file using AGU's template. Large data tables and multimedia should be uploaded separately.

AGU requires that all data needed to understand, evaluate, and build upon the reported research must be available at the time of peer review and publication. Additionally, authors should make available software that has a significant impact on the research. Data, software, and other research objects (e.g. notebooks) should be made available in repositories that support preservation and reuse. An explicit Availability Statement in the Open Research section of the paper is required describing where readers can find and access the data (and software). Authors should include intext citations to data (and software) in the Open Research section and the full citation in the References section. Guidance on what the Availability Statement and Citation should include along with templates and examples can be found at Data & Software for Authors. .

AGU requires the corresponding author, and encourages all authors, to register for an ORCID.

Please check and verify authorship, and that all authors are included, have approved the revisions, and agreed to be listed in the order given. Authorship is final with publication. Responsibilities of the corresponding author are given here.

When you are ready to submit your revision, please login to your account (https://wrr-submit.agu.org/cgi-bin/main.plex) and click "Revise 2022WR032039."

I look forward to receiving your revised manuscript. If you have any questions, or need additional time to complete your revisions, please contact us at wrr@agu.org.

Yours sincerely,

Jim Hall
Editor
Water Resources Research

----------IMPORTANT INFORMATION----------
Additional information on text preparation, formatting, acceptable file formats, supporting information, graphics preparation, and AGU style, is here.

Sharing your work is an important part of the research process, and AGU leverages and shares published research to promote the broader importance of Earth and space science. Learn how you can promote your paper, including how your paper can be considered for additional publicity or for the issue cover if it is accepted.

Associate Editor Evaluations:
Recommendation (Required): Return to author for major revisions
Accurate Key Points: Yes

Associate Editor (Remarks to Author):

Dear authors,

We have received three very detailed reviews for your manuscript, which suggest that major revisions are needed before your manuscript can be reconsidered for publication in WRR.
All reviewers agree that the manuscript's structure has to be improved and that the method description needs clarification. Reviewers 1 and 2 highlight the need to better work out the novel contribution of this study, which is hidden behind the report-like structure of this manuscript. I agree with the reviewers that the manuscript needs a clearer problem statement, more focus on the major contribution, results and discussion. Some clarification regarding methodology seems to be needed in Section 2.5. In addition, Reviewer 3 points out that substantial editing is needed before re-submission. I am looking forward to reading a substantially revised and edited version of this manuscript.

Best regards

Reviewer #1 Evaluations:
Recommendation (Required): Return to author for major revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Yes
Referencing: Yes
Quality: The organization of the manuscript and presentation of the data and results need some improvement.
Data: Yes
Accurate Key Points: Yes

Reviewer #1 (Formal Review for Authors (shown to authors)):

This work improved the accuracy of the height above nearest drainage (HAND) based flood inundation mapping. This study first compared the original HAND-based flood inundation mapping approach with HEC-RAS 1D results and showed that HAND suffers from a limitation caused by independent neighboring catchments that cannot cross catchment boundaries. Then, the study proposed a series of terrain analysis steps to resolve this issue. The most crucial new method compared to other steps that had already been used in previous studies was to reduce the Horton-Strahler stream order of a HAND processing unit down to one. Overall, I support this work and find it a valuable research area. The paper was well written, and the work is original in its effort to resolve some issues associated with the HAND-based flood mapping method. However, I sometimes found it more like a technical report than a research paper. I appreciate the effort authors took to explain every step of the methodology in detail; however, I hoped to see more results and discussion than learning every detail mentioned in the methodology section. Also, the main part of the methodology section (section 2.5) requires modification as it does not clearly present the proposed method (please see my major comments below). I have some major, minor, and editorial points that I hope authors find helpful in revising this manuscript.

MAJOR COMMENTS:
- My first comment is regarding comparing results with the HEC-RAS model results. In this comparison, you evaluated the flood inundation extent and not depth. My question is, how the proposed method affects the estimated flood stage (depth)? Figure 9a shows that the proposed method resulted in a smaller estimated stage value. This should then result in getting a smaller HAND-based inundation extent. But you showed a larger inundated extent in Figure 10. How do you separate these two effects? This needs to be at least discussed in your discussion section. My second question is that it seems the proposed method reduces the false alarm rate. This means the underestimation of the original HAND-based flood mapping compared to the FEMA maps is resolved. However, we know there are uncertainties associated with the FEMA flood extent. How would you say this reduction of false negatives is really in a right direction when you did not compare results with ground truth data?
- My second major comment is that the methodology section tends to be very long, particularly sections 2.1-2.4. The main contribution of this paper is section 2.5, but it took me as a reader a long time to get to this point and figure out what the paper presents. Also, section 2.5 is somehow confusing and could be revised and simplified. For example, in L531, you mentioned you present two

successive methods. Is the first one sub-setting MS? The NWM main-stem part (2.5.1) seems (at least to me) like an introduction, not something that you did. Please clarify and revise this part such that a reader can follow the story that connects 2.5.1 and 2.5.2 with the main paragraph in Section 2.5. Again, this is the most important part of you work.
- The limitation that the authors attempted to resolve is introduced as a known limitation in the abstract. Is that really a known limitation? If yes, did you cite it properly in your literature review? I believe this was not introduced well as a limitation. You only pointed out to this in your results (Figure 10). This is somehow explained in L203-210. But not very well explained. This is an important part and the gap your research is trying to resolve so be generous and explain more. Could you also clarify whether this limitation always results in more overestimation or underestimation?
- Links to all datasets introduced in section 2.2 should be included in your manuscript either in the suggested table or within the "Open Research" section at the end of your manuscript.
- Line 331-332: what were the values for buffer distance, smooth drop, and sharp drop you used? Were they fixed numbers or differ? How did you choose these values?
- I believe a diagram showing all the steps and methods you took to prepare your data and method could be helpful. Your material and data section has about 14 sub-sections collectively. It is sometimes hard to follow along.
- LINE 462-464. Could you explain more about why this is required? Is it to improve flat areas? Can't we see any results associated with this error improvement? Any other studies that observed this that you could cite?
- Fig 4. You need to explain this figure in the content more thoroughly. What are the numbers? How does one dominate the other? You mention that the level path method starts from an outlet, so it is probably better to show an outlet in Figure 4. Please clarify what you mean by "arbolate sum"? I cannot understand what this summation is.
- In equations 5 and 6, When you apply the mosaic method, what is the reason to choose the max? Is that because you always see the original HAND approach, as shown in Figure 3, underestimating the flood extent? This may not be true everywhere, or it might be. Please provide your thoughts on this and add it to the manuscript.

MINOR COMMENTS:
- In the third key point, what do you mean by higher skill inundation? Please be more specific. Do you mean more accurate inundation depth or inundation extent? Same for L39. Mapping skill itself is not clear. Does that refer to flood depth or flood extent?
- The last line of your plain language summary: Philosophically speaking, if you compared the HAND to a more realistic model and found improvements, what is the point you offer using HAND instead of a more realistic model? If HAND is not a realistic model, the logic is that we should not use an unrealistic model. Please revise this. I agree HAND is a simplified model, but that does not make it unrealistic. It is realistic because it is based on some terrain physics. You may want to revise this sentence and use "physically-based" rather than "realistic".
- L121. There is a gap between the geofabric concept you introduced and the Muskingam-Cunge routing method. You might want to start this sentence like this: The NWM provides stream forecasts at these geofabric segments using the Muskingham-Cunge method.
- Page 29: It could be better to change the title of subsection 3.1 to "Flood mapping performance".
- L671. Please add "(Table 2)" at the end of the sentence. This guides the reader to know what you are referring to immediately.
- Line 315-318: Please clarify what NHD is? Does that refer to the NHDPlusHR or NHDPlus medium resolution as used in the NWM v2.1?
- Line 344-345: This statement requires citation(s).
- I would suggest removing lines 534-535.
- I believe section 2.3.1 is not a DEM hydro conditioning step. It is more like creating an input (as seed points) that you need when delineating the stream network. The hydro conditioning of the DEM section should start with levee enforcement (section 2.3.2).
- Line 570. What is the logic behind using a 7 km buffer? Did you first come with this number or after some test? What is your recommendation or suggestion?
- Maybe reporting the percentage of improvements in Table 2 would help better understanding the contribution of your work.
- Figure 8 contains a lot of information, and readers may require a substantial explanation to understand what these graphs and numbers all shown in a figure mean. You introduced the figure in the first paragraph of the result section (L672-677). I suggest you take one example from this figure and walk the reader through the numbers. For example, what does it mean when the KDE of CSI for the GMS model has the most left-skewed graph compared to the other models? Is it considered good or bad? Is it something you had expected? Another question that arises here is why using a higher Manning's N shifts the KDE graphs up and whether this means results get better or worse. You discuss this figure on Line 686-690, but I think it could be better to bring this part further up where you first mention Figure 8.

FIGURES:
- Fig 1. The NWM FR streams line is hardly visible in the legend. Please revise.
- Fig 3. Please define MS in the figure caption (Is it mainstem?). Also, TN, FN, FP, and TP should be described here. You introduced these terms later in the evaluation section. A reader has no idea what these mean unless they first read your evaluation section (section 2.7). Please add flow direction to this map using arrows. It can help a reader quickly understand the flow numbers on the figure as you talk about in the content.
- Line 575. This should be Figure 5d not 5c.
- Fig 5. H ow many different color codes (level paths) are there? Is fig 5a one HUC 8? Please mention the HUC8 identifier.
- Fig 6. It seems that the blue areas are inundated areas for the 0.2% recurrence flow. How about the inundated areas for the 0.1% recurrence flow? I am asking this because the caption mentions 0.2% and 0.1%. Please clarify and revise the figure and caption accordingly.
- Fig 7. It could be good to add some labels on the figure indicating where this location is within your study area. What is the mainstem's name? Would it be possible to add NWM v2.1 catchments to the map? I understand this might make the figure busy, but I would like to know how many cross sections falls within a NWM catchment? Is this the FR or MR NWM v2.1 stream or the one you created (GSM)?
- Fig 10. Which recurrence event was used to create this inundation map? Please add this to the caption and mention it in the content where you explain results in Figure 10.

EDITORIAL COMMENTS:
- L66. Remove the period before the parenthesis, followed by the citations.
- L101. NWM is already defined. Just use the abbreviation here.
- Title of section 1.5: It could be better to use the Operational Water Prediction (OWP) Flood Inundation Maps (FIM)
- L160. HPC is already defined.
- L167. MR is already defined.
- L178. USGS is not yet defined.
- Line 347: NLD has already been introduced on page 11.
- Line 524: MS should be first defined here not on line 537.
- Throughout the manuscript, please be consistent with the terms you use. Use either Manning's n or Manning's N everywhere.

Reviewer #2 Evaluations:
Recommendation (Required): Return to author for major revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Yes
Referencing: Yes
Quality: The organization of the manuscript and presentation of the data and results need some improvement.
Data: Yes
Accurate Key Points: Yes

Reviewer #2 (Formal Review for Authors (shown to authors)):

This study presents recent development made by federal agencies and their contractors on streamlining HAND-based flood inundation mapping. By introducing the idea of level paths, they have also tested the hypothesis that unary stream order networks enhance flood mapping performance skills with HAND. The study presents some solid work, and the results support their statements. It's important to expose this workflow, which has been adopted by government agencies in daily operation, to the academic community for broader discussion. I'm a little concerned about the relevance of this draft. To publish on Water Resources Research, you need first to highlight the problem you want to solve in water science. Therefore, presenting some research that is a byproduct of tool development won't interest potential readers of this journal. The authors may consider reorganizing the draft in a more science-driven way instead of technology. Based on the reasons above, a major revision is recommended for the editors' consideration.

Detailed comments for the paper are provided below:
Line 82-84 Introducing these two NWM networks in section 1.2 makes more sense.
Line 112 There isn't a figure 1.2.
Line 128-129 Here, a statement may be added to explain that running a continental-scale hydrodynamic model regularly is not feasible in operational forecasting, making the simplified alternative like HAND a better solution. (Like line 795-798)
Line 212-230 "In addition to developed tooling, we introduce research" I don't like how the authors present the study's purpose. This is not a draft submitted to Environmental Modelling & Software. Presenting some research that is a byproduct of tool development won't interest the potential readers of this journal.
Although very comprehensive, the introduction section seems a little bit long. This is not a HAND-study review paper, so you don't have to go through all the studies. The authors may consider removing a few sentences to make it more focused.
Figure 2 How are the pruned headwaters derived? Why are they moved away from the original NHDHR endpoints?
Line 513 I don't think "work for many instances fine" is formal enough as a sentence in a scientific journal article.
Line 532-533 Four sections have been written in the background before the authors finally dive into the method related to the hypothesis of the current draft, which seems too much. I appreciate the comprehensiveness, but this is a journal article, not a technical document.
Line 544 "only find an MS stream network of stream order 1 (i.e., headwater)" From Figure 1, we can tell that most AHPS sites are located on big rivers, which means their stream orders are pretty low. Then why do you say they are headwater links?
Figure 4 This figure is confusing and needs some revision. Demonstrating one network is enough to explain the idea of level paths, but there are a few in this plot. Also, the annotations are too dense, and their alignment needs to be adjusted. For example, I'm confused when I see two numbers (6.13 and 10.95) on the left purple link. How come two numbers there?
Table 2 & Figure 8 If only staring at these index numbers and plots, the improvement brought by the current study looks relatively marginal. The authors may look from other angles to see the benefits brought by analyses with unary stream order networks. Otherwise, the significance of this study does not look very convincing.

Reviewer #3 Evaluations:
Recommendation (Required): Return to author for minor revisions
Significant: The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.
Supported: Mostly yes, but some further information and/or data are needed.
Referencing: Mostly yes, but some additions are necessary.
Quality: The organization of the manuscript and presentation of the data and results need some improvement.
Data: Yes
Accurate Key Points: Yes

Reviewer #3 (Formal Review for Authors (shown to authors)):

This paper details the methods employed and findings encountered as the authors endeavored to produce a nationwide flood inundation map (FIM) from National Water Model (NWM) streamflow estimates over the Continental United States. This is an application of large-domain hydrological modeling. The relevance of this work to society is not to be understated; a forecast of flood inundation extent and depth from an operational flood forecast model is essential to reducing risks associated with major hydrologic events. The originality of this work lies in the scale of the problem being addressed and the relevance of operational forecasting to mitigating risk to life and property.

This manuscript adds to the growing body of literature using HAND as a method for determining potential flood inundation in the absence of a full-physics (hydraulic) model. Ultimately, the process of producing inundation estimates for >2.7M stream segments at high spatial resolution (~10m) is one which required much iteration. In the end, the authors found that generalizing the stream networks, and using smaller basin areas (HUC8) combined with aggregating individual level-paths yielded better estimates of flood inundation when compared to results from HEC-RAS BLE maps. Novel methods and computational algorithms were employed along the way, resulting in potentially useful workflows and repositories that readers may follow to produce similar FIMs over the same regions (CONUS).

Although there is a growing body of literature surrounding the use of relative elevation maps such as HAND for flood inundation mapping, this study scales the process to the continental scale and addresses many of the limitations encountered along the way. Some of the most novel findings in the paper come from the methodological choices employed, such as identifying the appropriate thalweg elevation in the presence of competing channels (DEM-based thalweg vs. hydrography-based thalweg). Another innovation is in the mosaicking of the FIMs in the presence of tributaries and lower-order streams. Another very useful innovation lies in 'burning' levee elevations onto the DEMs to enforce flood control measures on to the HAND surface. The approach detailed in this manuscript can be seen as a guide for mitigating the 'catchment boundary problem', the importance of which should be emphasized more extensively in this paper.

As a reader, I find the manuscript title and some of the conclusions to be somewhat misdirected. The idea that Horton-Strahler stream order has a direct impact on the resulting HAND inundation product is misleading. The stream order of a given reach is determined by the drainage density, and as such is a product of 'upstream' decisions made during the derivation of the flow network. Stream order is not an input into the HAND model, nor is it very meaningful when comparing networks that are derived from different headwater locations (such as a pruned version of an existing network such as NHDPlusHR). Thus, simply reducing stream order should not have any impact on HAND. There are references in the paper to the impact of drainage density on maximum stream order, but the Conclusions indicate that Horton-Strahler reduction is the reason for improved FIM results, when that reduction is simply an associated result of reduced stream network density. For example, the authors indicate "We present two successive methods implemented that reduce drainage densities by reducing Horton-Strahler stream orders of the networks employed...". In reality, the reduction Horton-Strahler stream order is the result of, and not the cause for, a reduction in drainage density. It may be useful to emphasize that the discretization of the network into coherent regions based on level-path or the regionalization methods (such as using HUC8 boundaries), as well as the priority queue calculation (Equation 6) during the HAND raster mosaic process has a bigger impact on the result than the Horton-Strahler number of the network employed.

Further revision could include a more thorough description of how using level-paths eliminates the catchment boundary problem, when each level path may have many tributaries (and thus multiple catchment boundaries). There is a bit of missing information in Section 2.5.2 that is necessary for understanding how the catchment boundary problem is mitigated. It seems that the way around the catchment boundary problem is to calculate HAND for a given level-path while leaving tributaries out of the analysis, and allowing the mosaic process to result in a HAND/FIM result with more extensive coverage over catchment boundary areas.

Another limitation in the methodology lies in using synthetic rating curves (SRCs) that result from reach-averaged geometry parameter derived from the AGREE-DEM (a hydro-conditioning process). This results in an artificially impacted cross-section and bathymetry structure. The parameters used to constrain the AGREE method will have a profound impact on the SRCs derived from that DEM due to the excavation process in the AGREE method.

The manuscript is, in places, difficult to understand due in part to the large number of acronyms and persistent jargon that pervades it. There are also many grammatical and syntactical errors that should be edited before re-submission. The attached annotated manuscript contains many suggestions for resolving these issues along with other comments and feedback, which the authors may find helpful. For this reason and those outlined above, I suggest to return the manuscript to the author for these (minor) revisions to increase the clarity of the methods and significance of the methodological choices on the results.

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2 attachments

3_reviewer_attachment_1_1650493904_convrt.pdf
7524K

3_reviewer_attachment_2_1650493990_convrt.pdf
7524K