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Peer Review File

Long-term Channel Geometry Adjustments for Reference Streams in the North Carolina Piedmont

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Anonymous[1,2]

Anonymous[1]

Peter Moore, Iowa State University[1,2]

[1]Round 1

[2]Round 2

Round 1

AE Review Summary:

Three reviewers provided comments on the paper that discusses changes to urbanizing streams due to changing land cover. Unfortunately, there was a transcription error that a reviewer discovered. The authors quickly reanalyzed the data, revised the manuscript and resubmitted based on an updated analysis. While there were changes to the results, the conclusions did not fundamentally change.

Reviewers did not generally object to the hypothesis of the paper, or the research need and larger contribution to the literature and practice. Reviewers did ask for clarification of terms that would be best aided by providing more context and updating or adding a few references to contextualize the research significance. That way, the hypothesis driving the resurvey of urbanizing steams ("identify characteristics of quasi-equilibrium streams and compare them to streams experiencing disequilibrium.") could be cemented in the literature with proposed revisions.

The review comments were consistent asking for clarity of terms and concepts and rewrites/rephrasing of some sections to knit findings to the literature and benefit future stream interventions. I did not sense any discord between review comments and think comments were helpful.

Reviewer #1 comments (completed prior to resubmittal of manuscript, comments noted by line number on the original submitted manuscript):

13 "Stable boundary conditions" not well-defined without context; suggest rephrasing for a stand-alone highlight.

"Stable boundary condition" has been modified to "stable watershed conditions". Line 14

30 "boundary conditions" remains a poor word choice without context. How about "watershed conditions" or similar?

Boundary conditions has better defined with "such as water or sediment discharge, bed material size, or streambank vegetation" Line 30-31.

33 "adjustments" to what?

"long-term adjustments in riffle cross section dimensions (i.e., width, mean depth, and cross-sectional area) and discharge" has been added to clarify "what" is adjusting. Line 33-34

36 Presumably, "changes in watershed land cover and precipitation patterns" are your presumed drivers. Any others? Were these chosen *a priori*? They're certainly reasonable, but their basis should be explained (or the limitations of your evaluations at least acknowledged).

Yes, these two parameters are considered potential drivers of channel geometry change. The sentence has been reworded to state that "The adjustment in riffle channel geometry was quantified and evaluated by analyzing existing boundary conditions and changes in watershed land cover and precipitation patterns". Line 37-38. Other drivers could be vegetation removal or changes in sediment discharge or sediment size characteristics. Vegetation impacts had not occurred at the sites. We were not able to measure or quantify changes to sediment load with this study.

48 Some comment on your anticipated generalization of these results would be useful to readers trying to decide if this study has any relevance for them, or if it's just going to be of interest to those in the North Carolina Piedmont.

The following statement has been added, "The geometry adjustments and percent erosion reported for the more stable reference streams can serve as a gauge for evaluating the degree of change in channel geometry measured at both degraded and restored streams." Line 49-51.

56 To geologist, a "geologic time scale" is measured in millennia (or more). Many such adjustments can occur in a single storm or series of storms. So this is probably an overly conservative statement.

This statement has been modified to say, "This can lead to sudden degradation of the stream ecosystem through erosion and deposition on reach- and basin-wide scales and triggers channel evolution processes that eventually leads to the development of a new quasi-equilibrium condition." Lines 57-59. In re-reviewing Simon's paper he refers to relatively short time periods for disturbance to cause the response changes and indicates that they used data over an 18 year period for their study.

59 Best to update a reference now over 17 years old…

Two additional references have been added including a supporting study from 2022. Line 60-61.

86 What exactly do you mean by "regime systems"? In geomorphology these are typically applied to canals (originally in India, I believe) carrying only a suspended load. I don't think that's exactly what you mean here; I suggest you move away from this somewhat archaic terminology.

Overall, I think much of this "equilibrium" discussion is unnecessary and simply invites quibbles (like the one just above). You're trying to describe a relatively simple concept; your explanation should be similarly straightforward and brief.

The sentence has been modified to "Reference streams are stable reaches with hydraulic…." Line 90. However, a stream channel is said to be in regime if it is transporting water and sediment in equilibrium such that there is neither scour of the channel bed and banks nor sediment deposition in the channel. Yes, this terminology was first used for drainage canals. However, regime equations were later developed by Richard Hey (2011) for stable reference channels where he developed regime equations that related channel geometry to boundary conditions (water and sediment discharge, vegetation, etc.). Equilibrium of streams including a balance between water and sediment transport is described in the literature for many decades. Strahler (1957) described streams that maintain their form & character under continuous inflow of water/sediment as being in "dynamic equilibrium". Luna Leopold (1964) later introduced the equilibrium controlling factors of streams including width, depth, velocity, slope, discharge, sediment size, sediment concentration, channel roughness. Equilibrium and regime conditions are widely accepted by fluvial geomorphologists and ecological engineers who work in the area of stream restoration. The Strahler (1957) reference has been added to the paper. Line 88.

107-111 Since this is a set of channels selected by someone else for reasons different from your own, some greater detail of the criteria is warranted. For example, is "physical access" a plus or a minus? How unusual are streams the meet all of these criteria? Was watershed land use NOT a consideration (except, perhaps, indirectly in how it might be expressed by in-channel features)? Also, this is not a legal document. "One (1)" really isn't necessary—we can all count!

Physical access is a plus. It means the landowner would allow us on their property and we could safely get to and into the creek. Most sites were identified by practitioners and used for an analog stream restoration design procedure where geometry is summarized for the reference stream and then scaled to the design stream. The following sentence has been added – "All but one site had been used by restoration practitioners for analog restoration design procedures where geometry from the reference channel is measured, compiled and scaled to a design stream." Line 113-115. The (1) and the (3) have been deleted.

121 Any reason for a 6-hour criterion for "separate events"? In many parts of the world these would just be short hiatuses in a singular "storm." What's your basis for this?

Six hour inter event time between storms for the east coast is recommended by the US EPA (Driscoll et al., 1989). This reference has been added to the paper. Line 132

127 What's the difference between IC and DC? Define.

IC = Impervious cover is the estimated actual impermeable surface on the urban landscape based on the National Land Cover Database (NLCD) imperviousness layer. This layer estimates the impervious surfaces as a percentage of each developed surface. DC = developed cover and represents the amount of land area that is classified as one of four types of developed land cover (NLCD 21-24), which includes developed open space and low, medium and high intensity development using the NLCD data. So, areas with high intensity developed have higher impervious cover percentage estimate when compared to those areas classified as low intensity or open space. This description has been added to the paper. Line 140-141.

138-139 A little late to tell us this. You didn't "resurvey" the 2007 sections if you couldn't find the pins (not surprising, by the way—I can rarely even find my own!). Pick a different way of expressing this and note the likely uncertainty in co-locations.

This has been reworded – "Because the original survey pins could not be located, a map of the site prepared by Lowther (2008) was used to identify the same riffle cross section that was previously surveyed." Line 149-150.

143-152 Methodological details are a little sparse. What's a "modified" Wolman count? The definition of LBH is circular (what's a "floodplain"? The surface above the LBH) (that's circular). How did you determine ER? Etc…

These parameters, their measurements and methods are very common in stream morphology assessments, so we have referred to standard guidance documents rather than describe the methods and details.

160 Where did Doll et al. (2002) come from? How does it relate/overlap with the criteria used to generate your other comparisons?

The regional curves provided in Doll et al. (2002) were developed using geometry data collected from stable streams with USGS gage stations and from high quality reference reach streams in the N.C. Piedmont. The same criteria were applied for selecting reference reaches by both Doll (2002) and in the Lowther (2007) study. Reference reaches from this study that were not already included in the rural curve were combined with the 2002 data to update the curve. Minimal streambank erosion and a well-connected floodplain (i.e., little to no incision) were also the criteria applied for selecting the reference reach streams reported in the 2002 paper.

175 HG_{bkf} is only used once (here) and is undefined. Why do you need it? What is it?

HG_{bkf} has been removed from the paper. Equation 1 has been modified for A_{bkf} net adjustment as an example for how the adjustment for each parameter were calculated. Line 166.

188 What did you use to define "substantial"? Does +16% really qualify?

"larger" has replaced "substantial". Line 199.

194 Is the reduction in minimum DC change real? Or does this give an indication of the minimum amount of uncertainty in these measurements?

Most likely this negative DC value represents the uncertainty in the landcover dataset. The following sentence has been added. "Some of the results indicating very small or negative changes (e.g., -0.4%, 0.2%) may indicate uncertainty in the land cover spatial datasets." Lines 205-206.

232 Are the plotted regression lines on the combined data? Or just from one of the years (or both, but they perfectly overlap)? Do you find it curious that the regression slopes align to 4 significant digits? I do, and I'm not sure I believe it.

The data compares 2007 and 2018 separately. The lines do not perfectly overlap. There are slight differences. Because there is no significant differences between the slopes in 2007 and 2018, a pooled slope was assumed and different intercepts were calculated. Therefore, the regression equations have the same slope for both years. However, the equations in the table have been replaced with the individual equations for both datasets rather than the equations with pooled slopes to better illustrate any differences. This figure has been replaced based on comments and suggestions provided by another review. See the new Figure 5.

245 What is meant by "net and absolute" changes? I only see one set of changes.

Only net changes are shown, so absolute has been removed. Line 246.

250-251 These seem like critical plots for your story, and so they should be in the main text.

These plots were removed from the draft manuscript due to the figure and table restrictions imposed by the journal guidelines. However, we also agree that these figures are important to the story. Therefore, we have moved the figure into the body of the manuscript (see Figure 3).

259 Why "interestingly"? You wouldn't expect the longitudinal slope to change without truly massive adjustments via bed erosion or deposition, and D50 is as much a function of watershed supply as in-stream hydraulics. And if D50 doesn't change, why should tau_c*?

"Interestingly" has been removed (Line 256). Localized changes in slope, even relatively small increases in slope, result in increased shear stress that drives incision and headcutting in streambeds throughout our region. These localized increases in slope are often due to additional stormwater inputs following placement of new impervious cover.

270-272 This seems like a rather convoluted way to highlight the obvious—channels with major increases in IC behave differently than those that do not. The one seeming outlier, UTLI, likely is expressing a non-equilibrium condition as of 2006, insofar as it was still adjusting to the 17% imperviousness then. Did you check to see how long before that development had gone in for that watershed?

We could have made some subjective groupings of the site ourselves. However, the cluster analysis applies a structured procedure for grouping data into meaningful categories based on similarities and differences. While the separation based on IC and ΔIC may seem obvious, that does not negate the benefit of applying a cluster analysis that uses three steps to group these streams – 1) calculate the distances, 2) link the clusters, and 3) choose a solution by selecting the right number of clusters.

281 Should this be <2.6 km^2 as with the rest of this paragraph? If not, the change in size reference doesn't make much sense.

We are unclear relative to the point of this comment. The additional data for streams in smaller watersheds is important since most restoration work is occurring on streams with small watersheds. This is the point we are trying to make. Hopefully, this is clear.

285 Hopefully in the discussion you'll take up the fact that these relationships have unusual exponents relative to other such studies. For the w x d (i.e., A) exponent to be 0.588 means that either the velocity exponent has an unusually high value (up to 0.412) or the drainage area-to-bankfull discharge ratio is unusually low (which one might expect for the largest watersheds, but certainly not the smallest ones). Thoughts?

The exponents for all the hydraulic geometry relationships we report are similar to multiple other studies. A good compilation of regional curves and their associated exponents can be found in Bieger et al., 2015 (https://onlinelibrary.wiley.com/doi/epdf/10.1111/jawr.12282).

289 Why are the high IC streams necessarily in "disequilibrium"? Why couldn't that have just reequilibrated to a higher Q? Explain.

Impervious cover increases stormwater runoff, which tends to destabilize streams. It takes times for the streams to re-equilibrate to the higher discharge. In urban areas, the equilibration is often restricted by infrastructure (e.g. roads, utilities, etc.).

319 Where did "project failure" come in? I agree that this is what the two cited papers discuss, but how is that relevant here?

If a restored stream begins to incise and exhibit pervasive streambank erosion, the general perception is that the project is a failure. This paper is intended to provide some guidance on how much adjustment in channel geometry is reasonable. Repeated cross section monitoring is required of most stream restoration efforts that are implemented for mitigation purposes. The results of this study will provide a baseline for comparing the results of monitoring from restored streams, helping to move away from subjective perceptions about project failure. We maintain that this data and the results and discussion as presented are a useful contribution to the stream restoration community, which is a faction of the ecological engineering profession. However, the statement has been modified to say "instability" instead of "failure" – see Line 343.

Reviewer #2 comments (after resubmittal; line numbers reference the revised manuscript):

This is an interesting analysis of temporal variation in the hydraulic geometries and regional hydraulic geometry relationships for small "reference" Piedmont streams, but there are a number of organizational, methodological, editorial, inferential, and presentation issues that need to be addressed.

Issues:

Are there any motivating hypotheses or questions for this work?

The goals and specific objectives of the study are stated at the end of the introduction. The hypothesis tested by this study were that existing boundary conditions and changes in key boundary conditions (discharge of water and sediment) would lead to larger adjustments in channel geometry. Watershed land cover and rainfall variables were used as a proxy for changes in water and sediment discharge. The following statements have been added to the end of the introduction, "We tested our hypothesis that existing boundary conditions and changes in watershed land cover and precipitation would influence the rate of adjustment in channel geometry. Temporal variation in watershed land use and precipitation were used as proxies for changes in Q_W and Q_S." Lines 103-106.

The Map in Figure 1 is not very helpful. Some hydrography, major roads, and some representation of urban areas are needed to put the reference streams in perspective.

The map has been revised to show variability in land cover across the piedmont region.

Lines 102-103. Are there basic criteria for reference streams? If so, please recap. If some of the streams no longer fit the reference criteria in 2017, this should be noted. Two of the streams have 2017 IC cover of 24% and 50%.

These are urban streams, and it's hard to imagine using them as reference streams or using them to evaluate quasi-equilibrium, because they are not in equilibrium.

The reference condition of the streams was verified during the initial 2007 survey. In 2018, we did not revalidate reference condition. Rather, we were there to quantify the adjustment that had taken place over the 10+ year time period and evaluate factors that may have influenced the rate of adjustment. This statement has been added to the manuscript (see Lines 119-120). The criteria for determining the reference condition include bankfull should be at or very near the top of bank (no degradation or aggradation). The stream should have no apparent incision or head-cutting and have stable, well-vegetated, stable streambanks. Riffles and pools are present in a regular alternating sequence. Even streams with high impervious cover can return to quasi-equilibrium after some period of time if there is room to allow for channel adjustment to take place. This is the premise of Simon's (1989) channel evolution model.

I was surprised that this collection of reference streams was uniformly small, with 16 of the 18 drainage areas less than 7 square kilometers and the largest drainage area only 21.32 square kilometers. Eight of the streams drained less than one square kilometer. I was curious how this relates to the size of streams upon which restoration has been done in the NC Piedmont. I would think that many users of these hydraulic geometry relationships would want an idea of whether the reference stream data are applicable to their projects.

The bulk of stream restoration projects for mitigation are performed on streams in relatively small watersheds. In a recent analysis of 44 Piedmont restored streams that we completed for the NC Division of Mitigation Services, the agency that has overseen hundreds of stream mitigation projects in our state, 60 percent of the sites had watersheds of less than 2.6 km^2. This statement has been added to the manuscript. See Lines 356-358. Not only are these sites applicable to their restoration, all but one of these streams was used in restoration designs produced for the NC DMS. That is how we were aware of these streams and how we were funded to conduct the work in both 2007 and 2018. More details have has been added to the paper. The following statement was added to the paper to make this point, "All but one site was previously used by restoration practitioners for analog restoration design procedures where geometry measures from the reference channel were scaled and applied to a design stream." Lines 113-115.

The methods do not define "developed cover." What NLCD land cover classes went into "developed cover?"

DC = developed cover and represents the amount of land area that is classified as one of four types of developed land cover (NLCD 21-24), which includes developed open space and low, medium and high intensity development using the NLCD data. This detail has been added to the manuscript. Lines 140-141.

Equation (1) – the changes in hydraulic geometry are reported in percentages, but this equation produces a fraction. Need to multiply the equation by 100%.

The equation has been corrected.

Line 190. "Stream hydrology in the Southeast U.S. is driven by event-based precipitation." I think the authors are trying to say that precipitation drives much of streamflow behavior, but this sentence as written is neither correct nor necessary.

This sentence has been deleted.

Table 1 – the caption should refer the reader to the figure S.1.

This has been added to the caption.

Figure 2 – the symbols in this graph are very difficult to distinguish. Suggest using open symbols for one set.

The graph has been revised/combined according to your comments and suggestions below. The resolution was increased and the symbols have been modified as well. See Figure 5. Because the revised regional curves are important to stream designers, we have also provided the new curves in English units in the supplementary information.

This paper should be made much more efficient by eliminating the entire section 3.1. The comparison presented in section 3.1 don't make sense given that the graphs mix "reference" streams with newly urbanized streams. Right now the paper presents the regional hydraulic relationships for all the unfiltered data, then runs a cluster analysis, filters the channels that have changed too much, and then presents the graphs and the equations again. Instead, present the correlations, run the cluster analysis and filtering, and then present the hydraulic relationships showing the filtered streams in a separate symbol. This would be more efficient and less confusing.

As a key hypothesis of the paper is that changes in discharge have accelerated channel adjustment rates. Changes in land use and precipitation were used as the two proxies for estimating the changes in discharge that occurred. So, we assert that section 3.1 and 3.2, which summarizing the results of the analysis to quantify changes in these two proxies for discharge are important. However, we have rearranged the remaining sections of the paper following the suggestions provided by the review.

Consequently, Table 3 shouldn't be presented in the current section 3.4, and it should show the 2007 regression, the 2018 regression, and the joint regression, as well as the statistics for all three.

We have combined the graphs as suggested. See Figure 5.

Lines 234-235. "For most of the study sites, between 1% and 17% adjustment was observed in Q_{bkf}, A_{bkf}, W_{bkf} and D_{bkf} with several sites adjusting by more than 25%,…" This is not specifically accurate and it is generally misleading. According to Table 4, bankfull flow changed by more than 20% in half the streams. Bankfull area changed by more than 15% in 6 of the 18 streams There is actually quite a bit of change in bankfull area and bankfull flow in a substantial fraction of these streams. It should not be minimized. However, because the variation in basin area across the dataset drives most of the channel geometry variation, and because some channels got bigger and some channels got smaller, the temporal dynamics of the channels had little effect on the regional geometry relationships. The residuals from the relationships are as interesting as the relationships themselves. The residuals are often quite large.

The statements characterizing the adjustments have been modified to reflect the very important corrections and points made by the reviewer. Changes are reflected in Lines 231-235 and 238-241.

Section 3.3.2. The correlation table should be in the main body of the paper. If it is important enough to be the lead of a results section, it is important enough to be in the paper.

We agree and have moved the correlation matrix to the main body of the paper. See Figure 3. The correlation plot was removed from the draft manuscript due to the figure and table restrictions imposed by the journal guidelines.

The discussion should note that temporal changes in channel cross sections, even in the urbanizing streams, were small relative to changes caused by alteration of riparian vegetation as measured in other studies (see select references below – I picked two relatively recent ones including streams from the Blue Ridge. I just checked on Google Scholar, and there are a lot of new ones I haven't read yet).

Faustini JM, PR Kaufmann PR, AT Herlihy. 2009. Downstream variation in bankfull width of wadeable streams across the conterminous United States. *Geomorphology* 108: 292-311.

Jackson, C.R., D.S. Leigh, S. Lynsey Scarbrough, J.F. Chamblee. 2015. Herbaceous versus forested riparian vegetation: Narrow and simple versus wide, woody, and diverse stream habitat. River Research and Applications 31(7):847-857.

Addressing the comments above will probably require rethinking the discussion and conclusions. These data have implications beyond the application of natural channel design, and it would be good to think about their more general relevance to fluvial geomorphology.

We have added this point to the discussion and both of the suggested references as well as three additional references that show larger variation in channel width due to the presence or lack of forested vegetation have also been added. All our sites were in forested riparian areas, however, this comparison also helps to reinforce why establishing woody trees and shrubs is critical to maintaining stability of constructed channels. See Lines 319-334.

Reviewer #3 comments (after resubmittal; line numbers reference the revised manuscript):

Page and co-authors present new channel morphology data from various stream reaches in the North Carolina Piedmont region that were surveyed previously in 2007. They use these data to evaluate whether there has been channel morphology change in response to changes in boundary conditions, specifically focusing on changes to catchment land use/land cover. They also use these data to augment existing regional curves published more than two decades ago by some of the same authors.

The field methods employed by the authors are widely used and not innovative, though few prior studies have returned to previously-measured sections to examine morphological changes through time. As an inquiry in the stationarity of regional curves, this is an interesting and valuable case study that could support the persistence of such curves across a few decades, provided that hydrological boundary conditions remain relatively unchanged. As regional curves are a key reference for stream restoration design, I think this manuscript could be appropriate for publication in JEED provided that the authors address some questions and concerns.

GENERAL COMMENTS

My first general comment is a semantic one. Some authors have used the terms interchangeably, but I view the distinction between *hydraulic geometry* and *regional curves* to be fundamental and important. Hydraulic geometry (specifically downstream hydraulic geometry, as described by researchers like Luna Leopold), which uses discharge as the independent variable, follows directly from conservation of mass of fluid flowing through an open channel, and there is a simple logic to the values of coefficients and exponents that must hold. Regional curves, which I think are the focus of this study, use drainage area as the independent variable, and are one step removed from hydraulic geometry. Regional curve coefficients are not simply constrained in the same way, but contain information about both the hydraulics of open-channel flow and the runoff regime of the contributing catchment. Hydraulic geometry shouldn't be influenced by land-cover change, as those changes displace the reference discharge upstream or downstream. Considering the connotations that the term "hydraulic geometry" has in this sense, I suggest the authors simply use "channel geometry" when referring to geometric attributes of individual sites instead of hydraulic geometry. The regressions of those attributes as a function of drainage area are then most accurately referred to as regional curves.

Your point is clear regarding the differences between hydraulic geometry and regional curves. However, as you point out, the steam restoration and the fluvial geomorphology community have used these terms interchangeably for decades. We have changed "hydraulic geometry" to "channel geometry" in many locations, however, channel geometry is not an appropriate term in most locations since we are also considering discharge versus watershed area. So, "hydraulic geometry" has been retained in most instances.

My second concern is the distinction between quasi-equilibrium and disequilibrium streams that is seemingly arbitrarily presumed to coincide with the split between clusters in the data. While many sites experienced some changes in land cover and some corresponding geometry changes, those sites that changed more than 20% are treated differently. However, the authors acknowledge that geomorphic adjustments can take a long time to manifest in the field. Isn't it possible that many more than the 5 sites in Cluster 2 are significantly out of equilibrium and simply adjusting more slowly for one reason or another? Is there any reason to think that there is a real physical threshold for stability that has been surpassed that leads to the distance between clusters? This issue should be explored more thoroughly in the discussion.

The cluster 1 streams have substantially less impervious cover (mean = 1%) and change in impervious cover (mean = 1%) over the 10.5 year monitoring period, so the clustering results are not surprising. And given these low levels of IC and change in IC combined with a wooded buffer and no evidence of recent channel modifications, the assertion about equilibrium conditions for the cluster 1 channels is very reasonable. Similarly, the literature would assert that both high IC and high change in IC (as evident with the cluster 2 streams) would create conditions for disequilibrium as well as channel ecological decline. Maybe more adjustments will take place in the years to come. I fully expect them to occur. Over the time frame of our study, this is the adjustment that occurred. These results suggest that this is the case. We have modified our language to be less definitive if the clusters truly indicate a clean separation of equilibrium from disequilibrium (see Lines 301 – 304). Additional discussion regarding this issue of time frame for adjustment in response to watershed disturbance has been added (see Lines 326-328).

MINOR EDITORIAL COMMENTS

Throughout the manuscript, there are a handful of cases where there's a mismatch between singular/plural in subjects and verbs. Just to pick on one, in line 56, "…widespread efforts … has led…".

We have reviewed and corrected several subject and verb agreement issues in the paper.

SPECIFIC COMMENTS

L46. Punctuation missing at the end of the abstract.

Punctuation has been added.

L54. Simon would also say, however, that some adjustments only require a few decades to complete.

Agreed. The geologic time frame has been deleted from this sentence.

L56. Widespread efforts…HAVE led…

Corrected.

L59-62. Given the terms used in this definition of stream restoration, it may be appropriate to cite Harman or Fischenich or some other source for the Stream Functions framework.

Agreed. Both references have been added. Line 68.

L123 and onward. I think "landuse" should be two words.

Landuse had been changed to land cover. We feel this is actually a more appropriate term.

L135. "…longitudinal survey WAS used…"

An "s" has been added to survey.

L136-138. This gets to what I think is a weakness of the widely-used field protocols for channel geometry: many morphological indices are quantified at one riffle cross-section that is deemed to be representative of the reach. However, a reference reach spanning two full meander wavelengths should have ~4 riffles, and few if any studies have investigated how variable these indices might be among riffle cross-sections. I know some practitioners who select the "smallest" (by cross-sectional area) available riffle cross-section, as that will be the most effective at constraining flow. But if "representative" means something different to you and to Lowther, this could introduce a bias. If you selected different riffles than Lowther, can you be confident that any resulting changes in geometry are real and not just manifestations of biased selection in the presence of variance? I'm also curious if you used the same kinds of bankfull indicators as Lowther? If bankfull indicators are different, that could also bias your numbers.

Lowther selected the most stable riffle along the survey reach. We selected that same riffle for our survey. We also used the same bankfull indicator – flat sandy depositional surface. The second author, Doll, was present during both surveys and was the person who identified bankfull.

L148-150. What criteria were used to identify "active erosion"? Steffen's NRCS criteria? BEHI? This probably warrants some elaboration and/or a citation.

We identified banks with the presence of erosion rather than necessarily confirming the erosion was "active". This has been reworded to reflect this fact. Line 163.

L161. I assume the alpha refers to a statistical significance level? This should probably be stated explicitly.

This has been explicitly stated. Line 175.

L202. "…mean annual events WERE within…"

Where has been corrected to were.

L204. Not sure I like the use of "alternative findings" here. Maybe describe this in a different way that doesn't undermine your case.

This statement has been removed.

Figure 2. These are important graphs, but at the size displayed in the MS it is difficult to tell the difference between triangles and circles. I suggest changing one of the symbols to be unfilled, e.g., open circles and filled triangles.

The symbols and resolution have been improved and this figure has been combined with Figure 4 as requested by another reviewer. See the new Figure 5.

L308-311. Is there any reason to think that 20% is a real threshold between quasi-equilibrium and disequilibrium? The clusters are a somewhat compelling suggestion that something fundamental differs among those groups, but I can also imagine points 5,7,10, and 15 falling somewhere along the same spectrum of differing magnitudes of adjustment. (see general comments)

See our response above in the general comments section.

L333. There seems to be a word missing in here somewhere. "compared TO…?"

"To" has been added.

Round 2

The second round of reviews by two of three reviewers and editors pointed out several details that require more attention by the authors. Therefore, we request minor revisions and submission of a revised paper with responses to these comments:

AE / Co-EIC summary and synthesis
There remains a consistent use of "hydraulic geometry" (e.g., L40) and that was requested by the EIC and Reviewer #3 in the first round of reviews. They highlighted the difference between hydraulic geometry and regional curves and while some practitioners may use these terms interchangeably, we suggested that you use "channel geometry" to ensure clarity for the broader JEED readership. Please correct this throughout.

"Hydraulic geometry" has been changed to "channel geometry" when referring to measurements of the channel cross section. However, when referring to the relationships between cross sectional measures and drainage area, "hydraulic geometry" has been retained. Mulvihll and Baldigo (2012) define hydraulic geometry as, "regression equations that estimate bankfull discharge, width, depth, and cross-sectional area as a function of drainage area." In addition, Bieger et. al (2015) points out that while the concept of Hydraulic Geometry (HG) was first introduced by Leopold and Maddock (1953) to describe the dependency of channel dimensions on discharge that, "as the use of discharge as the independent variable limits the applicability of HG equations to gauged stream reaches (Ames et al., 2009), Dunne and Leopold (1978) introduced the use of drainage area as a surrogate for discharge." The National Engineering Handbook published by the USDA (2007) specifically indicates that, "the hydraulic geometry relationships at bankfull should then be plotted with respect to drainage area on the regional curve to generate hydraulic geometry relationships for bankfull." Referring to these relationships as hydraulic geometry or bankfull regional curves is well documented in the literature in addition to being the accepted terminology used by state and federal agencies and the ecological engineering design community.

Mulvihill, C.I. and Baldigo, B.P. (2012), Optimizing Bankfull Discharge and Hydraulic Geometry Relations for Streams in New York State. JAWRA Journal of the American Water Resources Association, 48: 449-463. https://doi.org/10.1111/j.1752-1688.2011.00623.x

Bieger, K., R. Hendrik, P. M. Allen and J.G. Arnold, 2015. Development and Evaluation of Bankfull Hydraulic Geometry Relationships for the Physiographic Regions of the United States. JAWRA 51(3): 842-858. DOI: 10.1111/jawr.12282

USDA, 2007. Developing Regional Relationships for Bankfull Discharge Using Bankfull Indices, Part 654, National Engineering Handbook, Technical Supplement 5. 14 pp. https://directives.sc.egov.usda.gov/OpenNonWebContent.aspx?content=17834.wba

While the regional significance is somewhat limiting, there is value for a broader audience and the manuscript would benefit from identifying opportunities for this work to be more transferrable – either with by applying similar methodology elsewhere or using results to refine local goals and set expectations for restoration success in other systems.

We have added a sentence to the conclusion regarding the potential to use this data to evaluate stream restoration projects.

The inappropriate use of "boundary conditions" was raised in the first round and not adequately addressed. This is a term that is not universally used. While it may be used within the restoration community, particularly with practitioners in the SE region, this should be revised to "watershed conditions" as recommended.

We also respectfully ask that the editors reconsider their request to change "boundary conditions" to "watershed conditions". First, changes in discharge of sediment or water can be driven by changes in climate and precipitation rather than changes in watershed condition. This is why rainfall data analysis was pursued as part of our study. Second, the channel evolution process can be present and worsen without any changes occurring in the watershed; changes in sediment discharge can be the result of unstable stream reaches above that are incising and eroding. Additionally, changes in streambank vegetation can affect streambank stability and sediment discharge under constant watershed conditions as well. Other examples of boundary conditions that can affect sediment and water discharge are impoundments, dam breaks and landslides. Changes in streambank vegetation, channel incision, streambank erosion, and precipitation are all factors that are potential drivers of channel morphology and channel geometry changes, which can happen independent of changes in watershed conditions. Davidson and Hey (2011) provide a thorough explanation including governing equations for morphological boundary conditions and how they control channel geometry indicating that, "Channel dimensions are determined by the simultaneous solution of a set of governing process equations, given knowledge of the controlling independent variables or boundary conditions, namely, discharge, sediment discharge, bed-material composition, bank sediment and vegetation cover, and valley gradient." This reference has been added to the manuscript.

There were two primary concerns raised with respect to your statistical methods. First, please state all assumptions that were tested for and met for each statistical test. For example, were the data normally distributed, independent, etc? And include a short label of the test used when reporting results and associated p value.

We have added more detail regarding the statistical tests, assumptions, and significance results to the manuscript.

Additional explanation is also needed when using absolute values instead of percentages.

Absolute values of change in channel geometry were used for correlation and cluster analyses because the objective was to evaluate the overall degree of change, as large changes, whether negative or positive, can indicate instability.

Second, we request that you double check the cluster analysis. Though it might be a slightly different algorithm, we were not able to reproduce your results as the minority cluster added two more sites when we reran the clusters (sites FCUS and UTBC fell into the minority cluster 2). It does not appear that the findings changed but we want to ensure that the results are correct and reproducible.

By carefully reviewing the cluster analysis and comparing the cluster data used in our analysis to what was reported in Table 4, we discovered that the Impervious Cover (IC) data provided in Table 4 did not match the data used in the analysis. Table 4 reported 2006 IC, but 2016 IC was what was applied in our cluster analysis. This is the reason that you were unable to replicate the results of our cluster analysis. We have corrected Table 4 to show the 2016 IC data. We also found a few instances of variation in the analysis data and the table values that were mostly due to rounding issues. We have corrected and updated all data in Table 4. We have also reviewed the cluster analyses to verify accuracy and correctness. We are providing you with a copy of the data and the R-code that was applied in the cluster analysis.

A reviewer indicated that revisions did not fully meet the intent of the reviewer's request who desired more information, context, or summarization. These are highlighted in the detailed reviews and summarized above. There are also references that need updating with more recent / relevant citations and moving text to the appropriate sections (Results presented in the Discussion and vice versa).

See below our responses and changes made for each comment made by the two reviewers.

Reviewer #1 comments:
Title: Change "for" to "in" or possible "of" – change was made.

L 14: consistently use "cross sectional area" instead of "stream hydraulic geometry" – this change was not made. See counter argument above.

L 16 more clearly specify date range. 10.5 years is a little awkward and recommend using 10 years. Also does this apply to both datasets? Please clarify here. – The streams were measured once in

2007 and then again in 2017 and 2018. The average time period between the two surveys was 10.5 years. This time span. The sentence has been modified to, "over the same 10.5 year period."

L 28 add "a" before hydraulic – "a" has been added.

L 31 change "morphological" to "watershed drivers" –This change was not made since watershed drivers are not the only potential drivers of changes to sediment and water discharge that would result in changes to channel geometry. For example, changes in streambank vegetation and stability, channel incision and head-cutting, impoundments, dam breaks, landslides, and changes in precipitation can all influence channel form, geometry and morphology and all can occur independently of changes in watershed conditions. Therefore, the term, "morphological boundary conditions" more completely encompasses the factors that influence channel change. We specifically tested for changes in precipitation to determine if this could be a potential driver of channel change in our study streams.

L40 discharge vs drainage area should be a separate category as this is not consistent with the definition of "hydraulic geometry" used in the scientific community. – Discharge vs geometry is a regular – There are numerous published manuscripts that refer to hydraulic geometry when evaluating relationships of channel area, discharge, width and depth versus drainage area. In fact, Dunne and Leopold (1978) introduced the use of drainage area as a surrogate for discharge for evaluating hydraulic geometry.

> Dunne, T. and L. B. Leopold, 1978. Water in Environmental Planning. W.H. Freeman and Company, San Francisco, California.

L43 semicolon before "however" and comma after the word

L 48 rework sentence "These data..." this is a fragment. - The sentence was modified to, "These data were used to update existing published North Carolina Piedmont hydraulic geometry regional curve relationships and other summary morphological data that can be used to help guide future stream restoration designs was compiled."

L55 this sentence is cumbersome and hard to understand but important "punchline" of your work. Please rephrase for clarify and consider how you can include the scope/aims of ecological engineering design here. - We request that the reviewer further clarify how they desire this sentence to be modified. The sentence is intended to set up the story of how modifications have been made to streams that have led to degradation, which is the impetus for restoration activities that is described in the next paragraph.

L 62 consider adding Thorne and Clure ref – Cluer and Thorne, 2014 has been added as a reference to this statement.

L 64 please add more recent reference here; ok to keep this seminal paper but it's quite dated. – two additional references that discuss the extent of expenditures on restoration have been added.

L 92 use of the word "stable" may not take into account dynamic changes within a certain range; please revise - The reference reach as it is used as a blueprint for natural channel design, which is the focus of this paper, was defined by Rosgen (1998) as "a river segment that represents a stable channel within a particular valley morphology." This reference has been added.

> Rosgen, D. L. (1998). The reference reach: a blueprint for natural channel design. In D. F. Hayes, Wetlands Engineering and River Restoration Conference, Denver, CO.

L 97 Refine sentence to only introduce and define terms – The sentence has been reworded to, "However, a change in boundary conditions can affect the discharge of water or sediment and alter the hydraulic geometry over a period of time as the channel adjusts to a new regime condition such that it transports the sediment and water without scour or deposition." This helps to define regime conditions. Boundary conditions have already been defined by this point in the paper.

L 103 add "these" before "reference" – "these" has been added.

L 103 add "in these channels" after "conditions" – "in these channels" has been added.

L 112 restate criteria for reference stream selection (or bring L 118 up to this point) – The paragraph as been trimmed and reordered to pull the reference reach selection criteria up close to the start of the paragraph.

L 122 provide NHD or other source for stream order – the stream orders were determined using NHDPlus flowlines. This reference has been added to the paper.

L 126 Add lat/long to site map – Lat/Long coordinates have been added to the map shown on Figure 1.

L 133 Remind readers about omitting less than 2.5 mm (initial abstraction?) – This is a typo. It has been corrected to 2.5 cm. All storms of less than 2.5 cm were omitted as storms of less than 1 inch of rainfall are less likely to generate morphological changes to the channel. The 2.5 cm threshold generated 10-14 storms per year on average for both time periods evaluated, so this rainfall depth was considered a reasonable level for capturing flows near and above the bankfull stage, thus capturing the predominant channel forming events. While this is not an exact identification of the flow that will generate bedload transport and have significant influence over channel form, it serves as a reasonable threshold for comparing the two different time periods.

L 200 Is "TC" part of the original reference stream collection? - Yes, this is Terrible Creek located in Fuquay Varina, NC. <u>All of the streams surveyed</u> by this study were included in the original reference stream collection surveyed by Lowther (2008).

L 205 delete "may," consider "likely indicates" – the sentence has been modified as suggested.

L 222 please report in metric also since this is used throughout – the value has been converted to metric.

L 223 explain why threshold is important (or not a different value); what is the basis for this threshold?

L 232 Delimitations should include that roughness, slope, etc were assumed to be consistent resulting in a difference in Area, only – Slopes and manning's roughness coefficients were updated based on the site survey data and visual assessments of streambed and bank roughness, so the change in discharge is not isolated to changes in channel area.

L 233 awkward phrasing. "were compounded by"; also Rh is part of A, which makes this difficult – This sentence has been revised to "Q_{bkf} adjustments were likely greatest as Q_{bkf} responds to both changes in channel area (A_{bkf}) and channel shape (i.e., changes to wetted perimeter in Rh)."

L 237 Relocate "the changes were due..." to discussion - -The statement has been moved to the discussion. The statement was edited slightly.

L 239-245 Only restate major trends (incision, width) since this is presented in the table – Three sentences have been deleted that do not represent major trends.

L 260 & figure This is awkward symbology. Ideally, if the relationship is "not significant" then there ISN'T a relationship. Redo this figure to only show those relationships that are significant. If not significant, there shouldn't be anything in that box. As it is, the eye is drawn to the least important findings, not the most. – the correlation plot has been revised so that only significant relationships have a star inside the box.

L 273 define clusters as quasi-equilibrium (Cluster 1) or not (Cluster 2) and move L 305-313 to Results as these are presentation of data, not interpretation of results.

L 284 "more dramatic" is clearer as "particularly large" – substitution has been made as suggested.

L 287 Relocate to discussion section – The two sentences from 286-289 have been moved to the discussion. The discussion was shortened to prevent repetition.

L 288 strike "with DA <2.6 km" and add "these smaller" before streams – change has been made.

L 302 strike "*" from the Table and note that all are significant – change has been made.

L 313 Avoid re-stating table and focus discussion on the interesting – I believe the reviewer is mostly referring to the sentence in Line 312. This sentence has been deleted.

L 315 Create new sentence starting with "Therefore, ..." and new paragraph with "Increases" – A new paragraph was created. However, the paragraph break was not made at the location suggested because of some text (Lines 305-313) that was previously in results was relocated to the discussion section. The move of this content influenced the selection of the best location for the new paragraph.

L 316 Channels were likely adjusting before study period - and continue to change - so noting they are not in quasi-equilibrium is important. If "existing" watershed conditions influence channel adjustment, it's likely that the time scale of adjustments is longer than your study's baseline. This suggests that the sites with high IC in 2007 were still changing then, and would have continued to do so even if no additional IC had been added thereafter. It's probably worth noting that these channels were not (and still are not), in quasi-equilibrium... - In 2007, the five cluster 2 streams did not exhibit signs of disequilibrium – i.e. they had mostly stable streambanks, no signs of incision or aggradation and streambed habitat features were present and appeared stable. So, asserting that these streams were in disequilibrium at the time of the initial survey in 2007 is not warranted. We have already asserted they were in disequilibrium for the 2018 site visit. So, this point is already stated.

L 318 Additional and more recent references needed to strengthen this argument – three additional newer references that support this point have been added.

L 348 delete "to instability" – this change has not been made as this will make the sentence incomplete. Perhaps, the point of the sentence is not clear? Kondolf and Nagle both evaluated case study projects and measured large channel adjustments, which they used to assert their conclusion that the restoration projects were unstable.

L 351 remove "important contribution" and rephrase this sentence – change has been made.

L 357 add explanation about uncertainty – are your findings within the overlapping error of the two relationships? – uncertainty explanation has been added and the overlap in confidence intervals has also been pointed out.

L 369 please reevaluate cited sources here. Booth and Henshaw actually rejects a hypothesis the urban development consistently leads to channel enlargement. – The Booth and Henshaw reference has been removed and three additional new references were added to support channel enlargement due to urbanization.

Globally, avoid passive voice when feasible, eg "have been" to "were" – have been was replaced by were in one location.

Please list key words in alphabetical order. – change has been made

Review #2
Overall, I think the authors did a satisfactory job of responding to reviews, and their efforts improved the manuscript. I only have a few additional items, noted below.

L61. Critical functions are lost during the adjustment period, but evolution (in principle at least) tends to restore many of them when it is complete. – the sentence has been modified to reflect this point.

L162. The symbol for water surface slope has a different subscript here than it does when the variable was introduced earlier (SWE versus wse; differs in case and order of letters). This remains inconsistent later in the manuscript (e.g., L256). Make sure it is consistent, or simplify. S_o is used earlier in the intro, and it isn't clear why the notation changes, unless they are making a distinction between local water surface slope and a larger scale channel slope? If the latter is the case, that difference should be specified. – The SWE subscript was corrected in two locations. S_o is for larger scale channel slope rather than water surface slope. This has been specified.

L163-4. In my first review, I requested more information on the criteria used to identify "active erosion". The authors responded by changing the wording to "presence of erosion". While it may seem trivial, I still think there is inadequate justification for *the criteria that the authors used to recognize erosion*. There are various quantitative and qualitative criteria for assessing erosion (I mentioned BEHI as a common example in the first review), and they have varying degrees of reliability and sensitivity to field conditions. Since the metric "percent erosion" plays an important role in the conclusions, readers need to know more about how it was measured. - A streambank was classified as eroding if the vegetation and/or roots were sparse or absent and the bank showed visible signs of scour, unstable undercutting, or mass wasting. This sentence has been added to the paper. BEHI combined with Near Bank Stress is used to predict the rate of annual erosion that will likely occur. However, it is typically only applied on streambanks that are actively eroding. So, we chose not to use BEHI for classifying a bank as eroding or not.

L243-4. "…all where characterized…" change where to were. – correction made.

L250. "IC and ΔIC cover..." is redundant. – IC is the existing land cover as of 2007 and ΔIC is the change in land cover that occurred between the two surveys (2007 and 2018), so these two terms are not redundant. The sentence has been modified to better convey this meaning.

Round 3 (Sara Winnike McMillan, Editor in Chief)

January 29, 2024

Hi Barbara,

Thank you for submittng your revised manuscript, "Using Long-term Hydraulic Geometry Adjustments to Verify Quasi-Equilibrium Conditions for Reference Streams in the North Carolina Piedmont" to the Journal of Ecological Engineering Design (JEED). Jon and I reviewed your submission and appreciate your thorough and thoughtful responses to the review comments. I am pleased to inform you that, following careful assessment and peer review of your submission, our editorial team would like to move forward with publishing your manuscript pending a few minor revisions listed below.

1. Your request to continue using boundary conditions is reasonable and the description in the review response is very helpful in contextualizing variability of those drivers of change. As such, we request that you add a shortened version of this to the introduction (likely place is around Line 95 of the clean manuscript) and maintain the use of the term "boundary conditions" in the manuscript as you have done.

2. Figure 4 represents a key finding of your work. However, the shading makes it difficult to visualize the improved model. We request that you flip the colors (make the combined dataset orange and the 2002 grey) and keep the orange in front so this is most visible to the reader.

3. The focus of JEED is on informing design and your work has findings that are relevant and meaningful to the design community. We request that you add discussion of your results to emphasize the relevance to design, particularly the expanded drainage area range and higher confidence in the reference reach. As currently written, the conclusion is a restatement of the findings. Simplifying the summary and focusing on implications for practitioners, regulators, and scientists would make for a stronger paper.

Please let me know if you have any questions. Once you have completed the tasks above, we will review and move your submission into the production process. Our production editor will be reaching out to you with more information about next steps, including copy editing and proofing.

All the best,
Sara

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February 8, 2024

Dear JEED editors:

Please find attached our revised manuscript. I have included a track changes version and a clean version where all changes are accepted. A few notes regarding the three editorial requests that were made. First, the boundary condition discussion was added at line 101 rather than at line 95. This location seemed to work better for incorporating the new content. Second, in the case of Figure 4, the grey and orange were flipped, but the orange line was not moved to the front as this completely blocked the adjustment data and trend lines. Instead, we made the orange line solid and bold so that it shows up better. Also, we did some cutting on the conclusions and incorporated more discussion about the design-related considerations. Hopefully, the recap of the work and conclusions is adequate. If not, we can certainly work to trim more.

Thank you again for this opportunity and we look forward to moving the article into production.

Sincerely,
Barbara Doll