Review Report for

Non-contemporaneity of flow events as a control on the morphology and sedimentary character of ephemeral tributary junctions

Daniel Cadol*, Sharllyn Pimentel, David Varyu, Jonathan B. Laronne

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Paper DOI: 10.59236/geomorphica.v2i1.39

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Round 1 Review

Comments by reviewer 1 (anonymous)

Completed: 2024-07-17 09:46 PM
Recommendation: Minor revisions

For author and editor

Nice paper, interesting and very easy to follow. I could find just a few typos (see below) and ended up with very few concerns: I believe the title is long but still somewhat vague; figure 9 is difficult to read (see suggestions below); and I missed photographs of the key sedimentary deposits that are then summarized in the conclusions.

Minor suggestions by line:

124 – “> 90º”, could you be more specific? (same for lines 131 and 132)

156 – define AGL after “above ground level”

I suggest adding camera resolution to the description of “data collection” as well as the model of the total station used.

274 – there is an r missing in “corelates”

Figure 9 is difficult to read. Everything is very small. Maybe dividing it in multiple enlarged figures with an inbox each for easy location would improve readability.

377 – remove “distally –”

383 – “repeated” instead of “repeat”, right?

Comments by reviewer 2 (Roberto Fernández)

Completed: 2024-10-02 03:00 AM
Recommendation: Reject

For author and editor

Dear author(s) and editor, I have included all my comments in the attached document. I would be happy to take a look at a revised version of this manuscript in the future.

I enjoyed reading this paper and thinking about the broader implications that shifting dominance between mainstream and tributaries imply for confluences in ephemeral river environments. The authors seem to have taken a lot of measurements during several field campaigns to look at the character of these confluences and analyze them. I have comments which I have separated between general and specific. The general comments require bigger changes in the manuscript or the addition of more data to better tell the story and ensure readers with other backgrounds beyond sedimentology or geomorphology, that the calculations are appropriate.

Perhaps my two biggest comments are the following:

The authors need to provide a better description and justification of all the methods used, as well as their uncertainties. This will become clearer as the authors read my general comments below.

The authors need to show the results of all the hydraulic calculations and discuss their implications.

Before moving onto the comments, I include a short summary of the paper which I hope reflects its contents.

Summary:

The authors present an analysis of the mouth/confluences of three small tributaries and analyze their sedimentary deposits by using structure from motion, hydraulic reconstructions and measurements, and trenches. They identify properties of the sites related to the differences of flow ratios between the mainstream and the tributaries. In certain periods, the tributaries dominate the sedimentary processes at the mouth but in others, the mainstream dominates.

This seems like a case study, but it could be generalized to a research paper with broader implications for confluences in similar environments worldwide.

General Comments:

Lines 27 to 64 read like a list of research on the subject, but the structure is not conducive to a true motivation of the topic of this manuscript. The introduction could be more succinct and to the point.

Lines 89 to 92 – This is the main objective of the study and the previous paragraphs in the introduction fail to lead the reader to it. The true motivation of the study is lost within a summary of available literature on different topics associated to confluences that do not focus on certain aspects that the authors of this manuscript consider relevant and see as a shortcoming. I encourage the authors to rewrite the introduction and keep in mind the objective of the study while avoiding unnecessary references.

Lines 206-209 – Can the authors include the measurements, and the stage-discharge curve developed in the supplementary material? Why was a power function chosen for extrapolation? What does it look like?

Equations (1) through (4) – I encourage the authors to report the values for all variables used in those equations or obtained with them (e.g. s, f, n, etc.). Of particular interest are the roughness coefficients and the author's thoughts on their applicability. It is not uncommon to find friction coefficients that are extremely high and do not make physical sense. Can the authors check that their values make sense from a physical point of view and if they do not, to discuss this?

The Manning-Strickler relationship (see equation below) relates the grain roughness height (ks) to the Manning's n value. The authors use a standard deviation s instead of roughness height for 'f' calculations. Do the ks values obtained with the Manning's n values estimated from Eq. (3) in the manuscript make sense? How do they compare to the 's' values used for the calculation of Darcy Weisbach friction coefficients?

$$n = \frac{k_s^{1/6}}{8.1 g^{1/2}} \qquad (Eq.\ A)$$

Why did the authors choose to use two different friction coefficients if they can all be computed from each other? See the equations below. More details on them may be found in Fernández et al (2020) – Eqs. (1) through (11) or García (2008, pp.24-29; Secs. 2.2.1-2.2.3).

Darcy-Weisbach friction coefficient – f Friction coefficient – Cf

Dimensionless Chezy – Cz (some authors include gravity in the numerator of Eq. C below and remove it from Eq. E but that leads to friction coefficients with dimensions).

Manning's – n

κ = 0.41; kappa is von Karman's constant

RH = hydraulic radius or flow depth (H) if the channel is much wider than deep S = channel slope

ks = roughness height

U = flow velocity (may be obtained with Darcy Wesibach Eq. E below)

$$c_f = \frac{f}{8} \quad (Eq.\ B)$$

$$c_z = \left(\frac{1}{c_f}\right)^{\frac{1}{2}} = \left(\frac{8}{f}\right)^{\frac{1}{2}} \qquad (Eq.\ C)$$

$$c_f = \left\{\frac{1}{\kappa}\ln\left[11\left(\frac{R_H}{k_s}\right)\right]\right\}^{-2} \quad (Eq.\ D)$$

$$U = \left(\frac{8}{f}\right)^{1/2}(gR_H S)^{1/2} \quad (Eq.\ E) - \textit{same as Eq. 2 in the manuscript}$$

Is Eq. (3) in the manuscript an empirical Manning's equation for steep channels where some of the assumptions of uniform flow needed for the standard equation do not hold?

Why do the authors use flow depth 'h' in some equations and hydraulic radius 'Rh' in others? If the width-to-depth ratio of the channel is smaller than ~20, the authors should use Rh throughout. For example, assuming a rectangular cross section of width B and flow depth H, the hydraulic radius is given as:

$$R_h = \frac{area}{wetted\ perimeter} = \frac{A}{P} = \frac{B \cdot h}{B+2h} \qquad (Eq.\ F)$$

It can be rearranged as follows:

$$R_h = \frac{B \cdot h}{B\left(1+\frac{2h}{B}\right)} = \frac{h}{\left(1+\frac{2h}{B}\right)} \quad (Eq.\ G)$$

As B gets larger than h, the second term in the denominator approaches zero and the hydraulic radius of the channel approaches flow depth. When B/h = 10, Rh = 0.83h (20% error); when B/h = 20, Rh = 0.91h (10% error); when B/h = 50, Rh = 0.96h (4% error) (for rectangular channels). As B/h gets smaller, the error of using h instead of Rh becomes larger.

Manning's equation and similar ones assume that the energy slope is parallel to the channel bed slope. The authors use channel bed slope in their calculations, but they mention (Line 261) that backwater effects propagate a long distance upstream into the tributaries under certain flow conditions. It would be best to use the water surface slope for the calculations when the flow is not uniform (i.e. there are backwater effects). Manning's equation can produce ok results assuming relatively uniform conditions but using the water surface slope instead of the channel bed slope. Are the authors able to do this? Do you know the water surface slope based on your depth measurements? If not, discuss the possible implications.

Lines 248-253 – How big were the samples and why was 50g chosen as the threshold to also do hydrometer tests? What percentage of the total samples does it represent? Were sample sizes based on the size of the coarsest grains? Was there material coarser than 2mm? Figure 8 shows sandy gravel material in all columns. What were the sizes of the gravel and percentages of the grain size distribution? How much material > 2mm? Were the hydrometer tests conducted according to any standard? The stratigraphic profiles show clay. Did the authors use deflocculant in the settling column tests to determine the sizes of the clay particles? Was there any consideration for the presence of flocs in this analysis?

Figure 2 – Authors should show the data, and the stage-discharge relation developed as well as the friction coefficients computed for each Arroyo.

Figure 3 – Authors should include the uncertainty of the DoDs based on the precision of their drone surveys. What are the mean errors between the point clouds once the 10-24 control points (Line 180) are used to geo-reference the point clouds? The colormap suggests a precision of 0.1m. Is this it? Was it better? Worse? Please include the errors of the surveys in the supplementary material. Same applies to all Figures relying on those data.

Discussion – The discussion reads more like a 'broader impacts' section and falls short of really analyzing the results on contextualizing them within the available literature and its shortcomings.

Conclusion – has some elements akin more to a discussion section and could be shortened with bullet points indicating the key findings of the study.

Other general remarks not specific to any section:

Have the authors considered the concept of morphological (sometimes called dominant or effective) discharge and how it might have a role in this context? How do the morphological discharges of the mainstream and the tributaries relate? In the absence of sediment transport measurements but having access to erosion/deposition volumes, could the authors do some transport estimates and calculate a morphological discharge for the tributaries and the mainstream? This might be beyond the data available but it might offer some very nice insights into the dominance of each component and how it varies as the ratio of discharges shifts.

Specific comments:

Line 27 – Rhoads 2020?

Line 39 – Rhoads et al. 2008?

Line 56 – Best and Rhoads, 2008?

Line 63 – Author's name? Year?

Line 66 – aerial photos

Line 75 and 76 – The references in these lines seem more relevant than many others in the introduction but the names of the creeks/rivers studied are not mentioned nor their relevant findings which seems appropriate.

Line 81, 82 – Remove 'as virtually all studies on confluences'. Unnecessary.

Line 113 – Replace ‘virtually every’ with ‘in any’

Line 114 to 118 – Why is this important?

Line 131 – is it an island or a vegetated bar?

Line 135 – What is a dirt diversion berm? Would the term ‘sediment’ be more appropriate than ‘dirt’?

Figure labels – The text has Cañas everywhere, but figure labels read Canas. Please correct figures so it reads the same everywhere. Alt+164 ñ.

References:

Fernández, R., Vitale, A. J., Parker, G., & García, M. H. (2020). Hydraulic resistance in mixed bedrock-alluvial meandering channels. Journal of Hydraulic Research, 59(2), 298–313. https://doi.org/10.1080/00221686.2020.1780489

García, M.H. (Ed.) (2008) Sedimentation Engineering - Processes, Measurements, Modeling, and Practice. American Society of Civil Engineers (ASCE). https://doi.org/10.1061/9780784408148

Review by postdoctoral colleague begins in the next page

Journal name: Geomorphica

Article name: Morphosedimentary character of the lowermost reach of ephemeral mid-Rio Grande tributaries: non-contemporaneity of flow events

This paper investigates the hydrologic, morphologic, and sedimentary characteristics of three river junctions where the Rio Grande meets three ephemeral streams with a junction angle of nearly 90 degrees. The authors quantified the water discharges of each stream to highlight the "non-contemporaneity" of water flow based on installed water-level loggers, site-specific rating curves, and empirical relationships. They also conducted repeated UAV surveys and compared the SFM-derived DEM models to reveal landform changes in the river junctions in response to flow events of various magnitudes. Sediment samples were collected from selected junctions to examine how grain size and texture are related to the hydrologic and morphologic characteristics of these junctions. The purpose of the paper is to understand how the planform and hydrologic properties of river junctions contribute to its "morphology and sedimentology" in the studied cases.

While the authors conducted extensive field surveys and presented their data in the results section, the integration of these data falls short of providing a definitive answer to the research objective outlined in the introduction (L88-92). The results are detailed but are primarily descriptive and lack sufficient quantitative analysis. For example, how did flow magnitude affect the distribution and quantity of channel deposition and erosion in different areas (e.g., upstream trunk channel, downstream trunk channel, tributary, etc.) of each junction? I recommend that these be summarized in figures, in addition to the general descriptions in texts.

The discussion section is very vague, primarily emphasizing the uniqueness of the general settings of the studied cases without sufficiently comparing any processes to other global examples. The purpose of the study (i.e., "determining how their planform, junction angle, and discharge ratio contribute to the confluence morphology and sedimentology through time" in L90-92) was not addressed either. I suggest the authors incorporate explanations and discussions in this section from a process-based perspective to highlight how the unique environmental settings contribute/relate to the distinct processes and dynamics observed at these river junctions with ephemeral tributaries.

Regarding SFS analysis, it is mentioned that "we employed the root mean square error to quantify point cloud elevation accuracy" (L180-181). However, it is unclear where these results are presented in the text. If omitted, the errors and any other potential uncertainties should be reported and possibly discussed in the methods section.

Recommendation by Associate Editor 2 (anonymous)

Completed: 2024-10-15 02:51 AM
Recommendation: Major revisions

For author and editor

Dear authors,

I read with interest your manuscript entitled "**Morphosedimentary character of the lowermost reach of ephemeral mid-Rio Grande tributaries: non-contemporaneity of flow events**", as well as the reviews from two anonymous reviewers. The comments from the reviewers are very constructive and please take this opportunity to make this manuscript more robust and convincing.
Based on my reading of your manuscript and the reviews, I would like to recommend for the manuscript to **undergo major revisions**.

Please also provide the link for the datasets from USGS and specify the version of any software used.

Best wishes

Anonymous Associate Editor

Decision by EiC (Alice Lefebvre)

2024-10-21

First, I would like to apologise that the review process took so long. One reviewer, who had agreed to review the paper, stopped answering emails when their review was due. This brought long delays, as we had to look for a new reviewer and let them some time to review the manuscript.

I have now received two reviews and an Associate Editor recommendation. I have looked at the paper, the reviews and the Associate Editor recommendation myself.

On this basis, we would like you to undertake a **major revision** of your manuscript.

Authors' responses to review comments

2025-04-25

Responses to Reviewers
Replies in blue italics

Reviewer 1:
Nice paper, interesting and very easy to follow. I could find just a few typos (see below) and ended up with very few concerns: I believe the title is long but still somewhat vague; figure 9 is difficult to read (see suggestions below); and I missed photographs of the key sedimentary deposits that are then summarized in the conclusions.
Thank you for the kind comments. We have modified the title, split Fig 9 into two parts, and added photographs of the five Pinos mouth bar sediment pits to the Supplemental Materials. We have also included images in the main text with the sediment column figures (now Figures 9 & 10).

Minor suggestions by line:

124 – "> 90°", could you be more specific? (same for lines 131 and 132)
Added.
156 – define AGL after "above ground level"
Eliminated the acronym and just entered the full text.
I suggest adding camera resolution to the description of "data collection" as well as the model of the total station used.
Added.
274 – there is an r missing in "corelates"
Thank you for the catch.
Figure 9 is difficult to read. Everything is very small. Maybe dividing it in multiple enlarged figures with an inbox each for easy location would improve readability.
We have tried splitting it into two parts and enlarging them.
377 – remove "distally –"
This sentence has been reworded.
383 – "repeated" instead of "repeat", right?
Yes indeed, thank you.

Reviewer #2:
I enjoyed reading this paper and thinking about the broader implications that shifting dominance between mainstream and tributaries imply for confluences in ephemeral river environments. The authors seem to have taken a lot of measurements during several field campaigns to look at the character of these confluences and analyze them. I have comments which I have separated between general and specific. The general comments require bigger changes in the manuscript or the addition of more data to better tell the story and ensure readers with other backgrounds beyond sedimentology or geomorphology, that the calculations are appropriate.
We thank the reviewer for their dedicated and comprehensive examination of our ms.

Perhaps my two biggest comments are the following:
The authors need to provide a better description and justification of all the methods used, as well as their uncertainties. This will become clearer as the authors read my general comments below.

The authors need to show the results of all the hydraulic calculations and discuss their implications.
We are grateful to the reviewer for their in-depth discussion of roughness-based flow equations. Like you, we also take our stream gauging very seriously, and have added more details about measured discharge at the Arroyo de los Pinos and the resulting stage-discharge relationship, and dug deeper into our estimates of discharge and roughness parameter (n) at the Cañas and Parida. We hope these additional details will provide the desired context. They have certainly been fruitful for our own thinking.

We have tried to address the specific comments individually below, but would also like to share two general lines of thought relating to this comment as a preamble.

First, the degree of uncertainty around any stage-and-rating-curve discharge estimate is fairly high, especially in alluvial channels with the potential for deep bed mobilization. For example, what uncertainty should we place on the USGS hydrographs for the Rio Grande that we rely on in this paper? Sauer and Meyer (1992) report typical errors in individual point-in-time discharge measurements of 2-20%, but converting this to a hydrograph using a continually-applied stage- discharge rating curve adds much more uncertainty. McMillan et al., 2012, in their data- benchmarking study, estimated discharge errors from the rating curve method from 50-100% relative error at low flows, and 5-20% for high flows. This certainly agrees with our general experience of gauging streams in the field and of using USGS data. Should all hydrographs in all papers graphically show this much uncertainty? Perhaps so.

Second, for our consideration of the junction morphology associated with asynchronous flows at ephemeral channel junctions, we do not believe that more precise gauging is strictly necessary. The main driver appears to be timing of flow, and this is well known and easy to measure. The discharge on the main stem mainly matters in terms of setting baselevel and, for high flows, providing a high enough water level that the tributary is back-flooded and a mouth bar develops. For the tributary flows, a very rough discharge estimate is adequate to classify the event as being able to remobilize the entire mouth bar or simply cut a short canyon to the mainstem. A 'large' flash flood on a tributary at our field site, whether 5 m3/s or 20 m3/s, will easily flush a mouth bar away and deliver the gravel-dominated sediment necessary to locally coarsen the mainstem.

Considering all that, our response to this comment is: Yes, we needed to communicate uncertainty about the discharge values be]er, and we have now tried to do so. Yet, the conclusions stemming from our analysis of the sedimentary deposits and topography at the confluences do not hinge on precise gauging of the tributaries or even the main stem. We will add more replies to the related comments below.

Before moving onto the comments, I include a short summary of the paper which I hope reflects its contents.
Summary:
The authors present an analysis of the mouth/confluences of three small tributaries and analyze their sedimentary deposits by using structure from motion, hydraulic reconstructions and measurements, and trenches. They identify properties of the sites related to the differences of flow ratios between the mainstream and the tributaries. In certain periods, the tributaries dominate the sedimentary processes at the mouth but in others, the mainstream dominates.
This seems like a case study, but it could be generalized to a research paper with broader implications for confluences in similar environments worldwide.
We have attempted to emphasize broader application of our observations to ephemeral streams globally in additions to our Discussion (especially Lines 452-514).

General Comments:
Lines 27 to 64 read like a list of research on the subject, but the structure is not conducive to a true motivation of the topic of this manuscript. The introduction could be more succinct and to the point.
We feel it is important to give a comprehensive overview of the topics that have been studied in confluences. It is a relatively small body of literature, which is what motivated our effort to publish our observations. We have moved this later in the intro, and created a new intro that leads directly to our objective (Lines 27-41)

Lines 89 to 92 – This is the main objective of the study and the previous paragraphs in the introduction fail to lead the reader to it. The true motivation of the study is lost within a summary of available literature on different topics associated to confluences that do not focus on certain aspects that the authors of this manuscript consider relevant and see as a shortcoming. I encourage the authors to rewrite the introduction and keep in mind the objective of the study while avoiding unnecessary references.
Thank you for this advice. We have reworked the intro to first lead directly to these questions, though we have retained much of the literature review, which we hope will be valuable to those investigating tributary junctions for the first time.

Lines 206-209 – Can the authors include the measurements, and the stage-discharge curve developed in the supplementary material? Why was a power function chosen for extrapolation? What does it look like?
This is a valuable suggestion, and we have now included these data in the Supplemental Material (Figure S4). We used a power law for our rating curve simply because it is a standard approach that fit the data well (R^2>0.95). However, your query spurred us to conduct a data compilation that we have been considering for a while, i.e, to convert surface velocity estimates we have collected over the years into

discharge estimates at higher flow stages. These include surface velocity radar (SVR) measurements using a Decatur radar gun, and large scale particle image velocimetry (LSPIV) measurements taken from georeferenced video footage using the Fudaa-LSPIV software package. When assuming an average velocity/surface velocity ratio (alpha) of 0.85, following Le Coz et al. (2021), the resulting discharge measurements plot very close to the extrapolation from our manually measured cross section discharges that were collected with a Marsh-McBirney velocity meter. The manual measurements were at flow depths

< 0.23 m, while the LSPIV measurements went up to 0.95 m depth. We found that videos recorded at night, with illumination only from oblique truck headlights, produced surface velocity measurements that were below the trend, but we suspect this was due to poor video quality.

We have added a plot of our stage and discharge data and power function rating curve to the Supplemental materials (Figure S4), and caption text covering these main ideas. We are open to adding it to the main ms if requested, but feel that it is a digression from the point of the article.

Equations (1) through (4) – I encourage the authors to report the values for all variables used in those equations or obtained with them (e.g. s, f, n, etc.). Of particular interest are the roughness coefficients and the author's thoughts on their applicability. It is not uncommon to find friction coefficients that are extremely high and do not make physical sense. Can the authors check that their values make sense from a physical point of view and if they do not, to discuss this?
We have generated these equivalent conversions for the results of Aberle & Smart's standard deviation method (old Equations 1&2, new Equation 3), but only summarize the for peak flows because the values of n change with flow depth (Table S1 and S2). At flows greater than ~ 1 m deep, they converge to the values predicted using D84 as the roughness height and Ferguson's 2008 version of the Manning-Strickler relationship, which we typically use, and have now introduced into the manuscript (Lines 237-242, Equation 2).

The Manning-Strickler relationship (see equation below) relates the grain roughness height (ks) to the Manning's n value. The authors use a standard deviation s instead of roughness height for 'f' calculations. Do the ks values obtained with the Manning's n values estimated from Eq. (3) in the manuscript make sense? How do they compare to the 's' values used for the calculation of Darcy Weisbach friction coefficients?

$$n = \frac{k_s^{1/6}}{8.1 g^{1/2}} \qquad (Eq.\ A)$$

We typically use the version of Equation A put forward by Ferguson. Although, we note that Henderson's (1960) interpretation of the Strickler data leads to a multiplier of 7.7, rather than the 8.1 you included. And Ferguson's (2008) version of it, which he labels as the 'deep' flow portion of his broader equation, has an equivalent term which he fit to a value of 7.5. We now include hydrographs based on it in this paper (Figure 2, Figure S3, & Figure S3) to help show the range of estimated hydrographs that can be produced using different assumptions. It also provides a baseline for comparison of n values that is available for all three channels (Table S1).

Why did the authors choose to use two different friction coefficients if they can all be computed from each other? See the equations below. More details on them may be found in Fernández et al (2020) – Eqs. (1) through (11) or García (2008, pp.24-29; Secs. 2.2.1-2.2.3).
You are absolutely right that f and n are interchangeable if the hydraulic radius is available, and so we have worked to make the paper more uniform by only discussing results in terms of n (e.g., Lines 237-268).

We originally brought the Darcy-Weisbach f into the conversation only because that was the roughness term used by Aberle and Smart (2003) in their effort to provide a bed-elevation- standard-deviation roughness metric (Equation 1 in the original ms, now Equation 3 and converted to n). We have been trying to use detailed drone DEMs to estimate roughness for models for quite some time, and this analysis seemed like an appropriate chance to deploy the technique. As the bed becomes finer-grained, the fraction of total roughness coming from grain size alone tends to decrease in our channels, so we felt the sandier Parida was a good location to use this approach. This experimental nature is the reason why we also tested the standard deviation method against the rating curve at the Pinos as a check. We have expanded the text explaining this reasoning (Lines 228-239).

Darcy-Weisbach friction coefficient – f Friction coefficient – Cf

Dimensionless Chezy – Cz (some authors include gravity in the numerator of Eq. C below and remove it from Eq. E but that leads to friction coefficients with dimensions).

Manning's – n

κ = 0.41; kappa is von Karman's constant

RH = hydraulic radius or flow depth (H) if the channel is much wider than deep S = channel slope

ks = roughness height

U = flow velocity (may be obtained with Darcy Wesibach Eq. E below)

$$c_f = \frac{f}{8} \quad (Eq.\ B)$$

$$c_z = \left(\frac{1}{c_f}\right)^{\frac{1}{2}} = \left(\frac{8}{f}\right)^{\frac{1}{2}} \qquad (Eq.\ C)$$

$$c_f = \left\{\frac{1}{\kappa}\ln\left[11\left(\frac{R_H}{k_s}\right)\right]\right\}^{-2} \quad (Eq.\ D)$$

$$U = \left(\frac{8}{f}\right)^{1/2}(gR_HS)^{1/2} \quad (Eq.\ E) \text{ – same as Eq. 2 in the manuscript}$$

Is Eq. (3) in the manuscript an empirical Manning's equation for steep channels where some of the assumptions of uniform flow needed for the standard equation do not hold?
This equation was developed for steep, gravel-to-boulder bed channels in Colorado, and is based on an empirical regression. We believe Jarrett would explain it as incorporating some influence from energy dissipation processes beyond grain size, such as the formation of hydraulic jumps that are common in these steep channels at high flows. We acknowledge that the bed material size of the Canas is small compared to most of Jarre]'s training data, but the channel is sufficiently steep. So, we considered it to be maximum

plausible n-value for these flow events (leading to a minimum discharge estimate), with values of n around 0.07 for the graphed flows (Table S1). We have added text to this effect (lines 260-268).

We have also added a comparison hydrograph showing discharge assuming n=0.023, the prediction from Manning-Strickler, to Figure S3.

Why do the authors use flow depth 'h' in some equations and hydraulic radius 'Rh' in others? If the width-to-depth ratio of the channel is smaller than ~20, the authors should use Rh throughout. For example, assuming a rectangular cross section of width B and flow depth H, the hydraulic radius is given as:

$$R_h = \frac{area}{wetted\ perimeter} = \frac{A}{P} = \frac{B \cdot h}{B+2h} \qquad (Eq.\ F)$$

It can be rearranged as follows:

$$R_h = \frac{B \cdot h}{B\left(1+\frac{2h}{B}\right)} = \frac{h}{\left(1+\frac{2h}{B}\right)} \qquad (Eq.\ G)$$

As B gets larger than h, the second term in the denominator approaches zero and the hydraulic radius of the channel approaches flow depth. When B/h = 10, Rh = 0.83h (20% error); when B/h = 20, Rh = 0.91h (10% error); when B/h = 50, Rh = 0.96h (4% error) (for rectangular channels). As B/h gets smaller, the error of using h instead of Rh becomes larger.
In this case again, the use of depth (h) was to stay true to the Aberle and Smart analysis. They produced an equation that uses flow depth divided by standard deviation of bed elevation to get f (which we now subsequently convert to n). However, we still have to use Rh when converting the roughness to a discharge using Manning's Equation.

Manning's equation and similar ones assume that the energy slope is parallel to the channel bed slope. The authors use channel bed slope in their calculations, but they mention (Line 261) that backwater effects propagate a long distance upstream into the tributaries under certain flow conditions. It would be best to use the water surface slope for the calculations when the flow is not uniform (i.e. there are backwater effects). Manning's equation can produce ok results assuming relatively uniform conditions but using the water surface slope instead of the channel bed slope. Are the authors able to do this? Do you know the water surface slope based on your depth measurements? If not, discuss the possible implications.
The backwater effects do not extend so far up the channels that they would affect the water surface slopes at our gauging locations. There are hundreds of meters of distance and meters of elevation drop. Moreover, the asynchronicity of the flows means that the Rio Grande is never high while the arroyos are flowing.

Lines 248-253 – How big were the samples and why was 50 g chosen as the threshold to also do hydrometer tests? What percentage of the total samples does it represent?
The 50 g corresponds to about 10% of the mass of most individual layer samples we collected. It was the mass required by the standard procedure we were using (ASTM D7928), as all the equations assume a sediment mass of 50 g. We have added this information to the text (lines 287-294).

Were sample sizes based on the size of the coarsest grains? Was there material coarser than 2mm? Figure 8 shows sandy gravel material in all columns. What were the sizes of the gravel and percentages of the grain size distribution? How much material > 2mm?

None of the samples had any material greater than 2 mm. See the new photos in the Supplemental material (Figure S5). The sandy gravel was at the base of each sediment pit and was not sampled, since it represents the Arroyo sediments deposited prior to the high mainstem stage that produced the mouth bars being sampled. It is simply shown in Figure 9 as a correlation marker.

Were the hydrometer tests conducted according to any standard? The stratigraphic profiles show clay. Did the authors use deflocculant in the settling column tests to determine the sizes of the clay particles? Was there any consideration for the presence of flocs in this analysis?

We used method ASTM D7928 employing a 152H hydrometer and sodium hexametaphosphate as a dispersing agent. We collected measurements beginning at 40 s for coarse silts, and extending to 24 hours for clays (nominally, 1 micron). We have added this information to the text (lines 287-294).

Figure 2 – Authors should show the data, and the stage-discharge relation developed as well as the friction coefficients computed for each Arroyo.

This is now included as supplemental material (Figure S4 and Tables S1 & S2).

Figure 3 – Authors should include the uncertainty of the DoDs based on the precision of their drone surveys. What are the mean errors between the point clouds once the 20-24 control points (Line 180) are used to geo-reference the point clouds? The colormap suggests a precision of 0.1m. Is this it? Was it better? Worse? Please include the errors of the surveys in the supplementary material. Same applies to all Figures relying on those data.

You are correct, we estimated the threshold of change detection to be 0.1 m. This was found using the RMSE error of 1/3 of the survey points, which were held back as Check Points. The other 2/3 of survey points (Ground Control Points) were used for georeferencing. The average RMSE of the point clouds was 0.05 m (min 0.01 m, max 0.09 m, st. dev., 0.022 m), so we doubled this value to account for error in both point clouds. Any change between -0.1 and 0.1 m was set as transparent in Figure 3, meaning the background orthophoto shows through. This is now explained in the Methods (lines 211-213) and in the caption to Figure 3 (lines 359-364).

Discussion – The discussion reads more like a 'broader impacts' section and falls short of really analyzing the results on contextualizing them within the available literature and its shortcomings.

We have worked on clearly generalizing our results beyond our field cite throughout the discussion section.

Conclusion – has some elements akin more to a discussion section and could be shortened with bullet points indicating the key findings of the study.

We have shortened the conclusion section substantially.

Other general remarks not specific to any section:

Have the authors considered the concept of morphological (sometimes called dominant or effective) discharge and how it might have a role in this context? How do the morphological discharges of the mainstream and the tributaries relate? In the absence of sediment transport measurements but having access to erosion/deposition volumes, could the authors do some transport estimates and calculate a morphological discharge for the tributaries and the mainstream? This might be beyond the data available but it might offer some very nice insights into the dominance of each component and how it varies as the ratio of discharges shifts.
We prefer to retain a focus on the junction morphology and temporary sediment retention. The substantial changes in the upstream channel geometry of the tributaries due to floods, even in the short duration of our observations, makes selection of a morphological discharge very

difficult. We have comprehensive sediment transport data from the Pinos, but no comparison data for the Parida or Canas.

Specific comments:
Line 27 – Rhoads 2020?
Line 39 – Rhoads et al. 2008?
Line 56 – Best and Rhoads, 2008?
Thank you for catching these errors.

Line 63 – Author's name? Year?
This was a dissertation by C Thornes which we found cited in a book chapter by JB Thornes. We have added the reference for the Thornes, 1994 chapter (line 80).

Line 66 – aerial photos
Fixed.

Line 75 and 76 – The references in these lines seem more relevant than many others in the introduction but the names of the creeks/rivers studied are not mentioned nor their relevant findings which seems appropriate.
The intro has been reworked to provide further details.

Line 81, 82 – Remove 'as virtually all studies on confluences'. Unnecessary.
Removed.

Line 113 – Replace 'virtually every' with 'in any'
Replaced.

Line 114 to 118 – Why is this important?
We believe the information is valuable in order to provide context for the flash-flood-producing rainfall-runoff processes, and to indicate the local climate as manifested by its ecosystem.

Line 131 – is it an island or a vegetated bar?
It is actually a vegetated bar, as you probably suspected. Thank you for this catch.

Line 135 – What is a dirt diversion berm? Would the term 'sediment' be more appropriate than 'dirt'?
I think the term of art used by the heavy machinery operators at the Bureau of Rec is a "non-engineered levee". It is simply a short berm pushed up by a bulldozer using any conveniently nearby material, likely

including some organic maJer with the sediment. It seems to be a feature typical of our rural riparian zones where locals are trying to encourage the river to stay away. We have changed the text to "non-engineered sediment barrier".

Figure labels – The text has Cañas everywhere, but figure labels read Canas. Please correct figures so it reads the same everywhere. Alt+164 → ñ.
Fixed.

Round 2 Review

Comments by reviewer 2 (Roberto Fernández)

Completed: 2025-05-24 02:31 PM
Recommendation: Minor revisions

For author and editor

The authors have done a great job of addressing all comments shared in the first review. I have gone through their responses and the updated manuscript and believe it is almost ready to be published. I am recommending minor revisions. My biggest comment at this stage is to encourage the authors to make sure that all figure are accessible.

Do not rely on color alone to convey meaning. For example in Figure 2 (and others) red-green are a combination hard to distinguish by people with certain types of color vision deficiency. I encourage the authors to read the paper by Fabio Crameri and coauthors (and visit their website) and make sure that all figures are accessible. https://www.nature.com/articles/s41467-020-19160-7 (The misuse of color in science).

I also found a typo on Line 188: sparce --> sparse

Recommendation by Associate Editor 2 (anonymous)

Completed: 2025-06-04 03:35 AM
Recommendation: Minor revisions

Dear authors,

I read with interest your manuscript entitled “Non-contemporaneity of flow events as a control on the morphology and sedimentary character of ephemeral tributary junctions", as well as the reviews from the anonymous reviewer. Based on my reading of your manuscript and the reviews, I recommend that the manuscript be accepted after addressing the points the Reviewer pointed out.

Best wishes

Decision by EiC (Alice Lefebvre)

2025-06-13

I would like you to undertake a **minor revision** of your manuscript.

I suggest that you check all your figures with this website
https://www.color-blindness.com/coblis-color-blindness-simulator/

Decision by EiC (Alice Lefebvre)

2025-07-18

Congratulations! I am pleased to inform you that your manuscript entitled "Morphosedimentary character of the lowermost reach of ephemeral mid-Rio Grande tributaries: non-contemporaneity of flow events" has been **accepted** for publication in Geomorphica.