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Interactive comment on "Analyzing bed and width oscillations in a self-maintained gravel-cobble bedded river using geomorphic covariance structures" *by* R. A. Brown and G. B. Pasternack

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Authors response to RC1, reviewer Carl Legleiter

General comments This paper examines the relationship between bed elevation and water surface width in a large gravel-cobble bed river and attempts to do so in a spatially explicit manner intended to quantify variability at different scales. While this objective is important, the present manuscript falls short of this goal. Although the topic is of broad, general interest and the underlying data are suitable for this type of analysis, the implementation is flawed in several critical ways. For example, what the authors refer to as a geomorphic covariance structure is not, in fact, a covariance at all, just a local product of detrended and standardized width and elevation. Similarly, although a

stated objective of the study is to make comparisons among discharges, the use of a different spatial reference for each flow stage complicates if not precludes such comparisons. The authors use auto-correlation functions and frequency domain analyses to examine scale dependence, but a simpler approach based on correlograms or variograms would be more insightful. Although the study has potential, major revisions, including substantive re-analysis, will be required before the paper can be published in this or any other geomorphic journal.

Response to general comments In the reviewers general comments and throughout specific comments there are two primary critiques, related to terminology and methodology. Here we address these critiques broadly before addressing specific comments.

Terminology One of the reviewer comments is related to terminology. In our paper we demonstrate and apply a new method of analysis, called geomorphic covariance structures. The reviewer contends a lack of understanding in that what we are calculating is not covariance, as defined in classical statistics. We agree that we are not calculating covariances as defined in classical statistics, and did not in any part of the text, state so. For example, lines 1-9 of the discussion paper clearly define what a GCS is, and further, state that it is a relatively new form of analysis. What we are calculating is a spatially explicit metric that analyzes the literal co-varying structure of two series of geomorphic data. In using the term covariance over correlation we have sought to make this metric intuitive to all scientists, not just those in the field of statistics, who frankly, are not concerned with the broader aims of this paper or geomorphology. In describing and defining this methodology we were very explicit in its basis and calculation. We state that a geomorphic covariance structure is a spatial series that describes how two variables vary and do not vary with each other. Taking two variables X and Y, the GCS value at each node i is defined as xi*yi, and in this paper, where x and y are detrended residuals standardized by their mean and variance. It is important to highlight that the GCS is not any one point, or related to samples of points, but the entire series of products at every location. So it should be obvious that we are not calculating either correlation or covariance in our geomorphic covariance structures. Regardless, we have added text to clarify that the GCS is neither covariance nor correlation as described in classical statistics.

We did label the Y axis of figures 4 and 5 as covariance, and we will revise that so as to avoid confusion, instead using the term “magnitude” to denote the strength of the local products within the GCS.

One could argue that within the term geomorphic covariance structure perhaps correlation could replace covariance. We aim to discuss here why the former was chosen. Correlation and covariance have similarities in their basis, and many textbooks discuss this along with their differences. A key aspect is that correlations can be derived from covariance by dividing by the product of the standard deviations of the two variables analyzed (Newland, 1983; Schumway and Stoffer, 2006). For example, Newland (1983) even refers to correlation coefficients as a normalized covariance (e.g. page 23).

Further, we have already had the idea of a “geomorphic covariance structure” as defined in this paper peer reviewed in 3 previous journal articles and it was embraced by the reviewers in all those cases (Brown et al., 2014; Brown and Pasternack, 2014; Brown et al., 2016). Also, the other reviewer of this article is well versed in statistics and had no problem with our using the terminology as we have. Of course we fully understand statistics, but we would like reviewer to also consider and recognize that it is very common in science to take a word and employ it for another reasonable purpose in a different discipline.

In many cases words used in scientific nomenclature have different meanings in different disciplines. For example in power spectral density analysis “power” is used to describe the strength and distribution of variance to a statistician, as sometimes shown on the Y axis of spectral density plots. However, in physics “power” is defined as the rate at which work is or can be performed. This has not stopped the use of the term power in either case, because presumably both disciplines are comfortable and knowl-

edgeable with the lexicon of each discipline.

In our article, the word “covariance” is being deployed in its sight word literal translation to mean, how two variables co-vary with each other. Given that correlation is so similar to covariance, and is a derivative of covariance we believe it is more intuitive to use that word within our overall term of geomorphic covariance structures. We should not be required to reserve any one word for only one jargon usage in one other discipline, as exemplified above with the case of the word “power”, which is used differently in different disciplines. There are countless examples of this. The key is that we have provided our definition of covariance very clearly for readers to understand, and it was been accepted as a reasonable usage.

Methodology Outside of terminology the reviewer contended that the use of different sample pathways with flow was incorrect. In the early stages of this paper we analyzed several approaches for sample pathways and performed the analyses in this paper for each one. This included using the thalweg, a smoothed conveyance pathway for the bankfull discharge, and the valley centerline – all constant with discharge.

As stated in the text the thalweg is too tortuous to have cross sections that are orthogonal to the flow. To illustrate this we created a figure some of our preliminary work (Figure 1). Figure 1 shows the traditionally defined thalweg and flow dependent sample pathways used in this study with 5m spaced cross sections clipped to the 8.5 cms flow. Visual inspection of this section of the river reveals that the thalweg in many places changes direction in otherwise straight sections of river flow when compared to the momentum based sample pathway. For example the momentum grid shows multiple downstream oriented bands where the thalweg moves left and right. As can be seen the cross sections generated using the thalweg would cause significant overlaps from the tortuous path in areas where flow is otherwise straight (Figure 1b). This is just one example of why the thalweg is not appropriate at low flow, but we found this to be prevalent throughout the study reach.

Similar issues arise for higher flows. For example Figure 2 shows the momentum grid for the 8.5, 141.5, and 3,126 cms flows along with the sample pathways. First, this figure shows that the thalweg would not be appropriate as a sample pathway for generating sections because it remains tortuous and static, while flow paths change with increasing flow as more of river corridor is inundated. It can also be seen clearly from Figure 2c that when the gravel bars are overtopped at 3,126 cms flows follow the valley walls more closely and deviates considerably from the thalweg pathway. Using flow width sections generated from the thalweg for higher flows would lead to incorrect estimates of flow width. Overall we have found that the thalweg overestimates flow width at moderate to high flows in areas where it angles where flow is straight. Similarly, the valley centerline underestimates low flow widths because it does not account for the flow steering that occurs from gravel bars.

Understanding that the thalweg and valley centerlines are not appropriate due to flow steering we developed flow –dependent sample pathways as discussed in the text. Initially, we mapped all data to the bankfull sample pathway (e.g. 141.5 cms). However, given the strength of C(W,Z) at the bankfull flow of 141.5 cms we thought mapping to that sample pathway could be interpreted as bias our results. Thus, our approach was meant to deal with the issues stated, but by all means was not deemed perfect and as we stated it remains an area of future research. Given that both reviewers had confusion with figures 4 and 5, and using different sample pathways we decided to revise our work. To do this we mapped each flow dependent width series to the pathway associated with the lowest flow (e.g. 8.5 cms) using the spatial join tool ARCGIS. So each flow width series was referenced to a single sample pathway with minimum bed elevation, while preserving the fact that flow is steered by variable topography with increasing flow discharge.

Specific Comments (responses in italics): 1. Page 3, line 8: Another relevant citation in this context is Legleiter (2014a,b), a two-part paper in Geomorphology outlining a geostatistical framework for describing the reach-scale spatial structure of river mor-

phology. Legleiter used variograms rather than covariances, but the two quantities are closely linked and both serve as metrics of spatial structure and variability. Omitting this reference entirely is an oversight.

We are well aware of the reviewers work in geostatistics. In reviewing the discussion paper in its typeset form we are not seeing where this citation would be appropriate. Page 3, line 8 refers to scale-dependent organization in natural rivers, and not particular methods that analyze only one scale of variability, as in the reviewers suggested papers.

2. Page 3, line 17: You state "self-maintained bankfull river channel," but then go on to emphasize the influence of bedrock and tailings piles – is this contradictory?

No. The channel is partially confined by bedrock and tailing piles that are activated above the bankfull channel and associated flow discharge. We have rewrittem the study background section to clarify this. Also, see discussion in Section 3 for more clarity.

3. Page 4, line 4: 9 km or the 6.4 km in the abstract, which is correct?

Thank you, we have corrected this. It is 6.4km.

4. Page 5, line 5: "removing the initial bed profile"? This is unclear and does not adequately describe the D & R (2012) study.

Reworded for clarity.

5. Page 5: Your review of empirical/modeling studies of pool-riffle sequences is thorough, and then you go into extremal hypotheses, but I think a more well-rounded background section also would include some discussion of a more process-oriented approach to channel morphology. For example, the classic work by Dietrich on Muddy Creek and subsequent studies of the importance of topographic steering effects, such as Whiting and more recently by Legleiter et al.

We thank the reviewer for his suggestion, but feel our review adequately characterizes existing literature, while not being superfluous. Adding more references related to the papers suggested would further encumber the reader on background information that is not essential to understanding this paper.

6. Page 6, lines 8-10: Provide citations to support these claims regarding remote sensing and larger-scale modeling.

We have added the following citation: Carbonneau P, Fonstad MA, Marcus WA, Dugdale SJ. 2012. Making riverscapes real. Geomorphology. 137:74-86. DOI: 10.1016/j.geomorph.2010.09.030

7. Page 6, line 24: Maybe not width and bed elevation, but Legleiter et al. (2007) examined stage-dependent spatial structure of flow hydraulics in a mountain channel using a geostatistical approach similar in many respects to your covariances.

No comment needed.

8. Page 7, lines 12-15: This is a key point throughout the paper that first comes up here: in calculating a GCS, you must have some sort of moving window to obtain a sample for estimating the covariance, whereas this sentence implies that you are just pairing one observation of x with one observation of y. To estimate the covariance, you must have at least a handful of data points. Perhaps I'm missing something, but how the data are pooled to obtain a covariance value for each location along the spatial series needs to be spelled out more clearly and explicitly.

We have addressed this point in the introduction of this reply. To reiterate, we are not calculating covariance in the classic statistical sense, but a new metric.

9. Page 7, lines 19-20: Why were these particular flows selected for analysis? Were these discharges for which you had field data to calibrate/validate the flow model? Please provide some brief rationale for the specific flow studied.

We have added text to clarify the selection of flows investigated.

10. Page 7, line 21: The word "preference" seems subjective and anthropomorphic; something like "tended to" or "more frequently exhibited positive values" seems more appropriate. This sentence is also passive and much longer than necessary. Please replace "preference" with "tendency" throughout.

That is a fair and good suggestion incorporated in the paper. Modified as recommended.

11. Page 8, line 6: The phrase "but other complex responses are possible" goes without saying and doesn't really sound like a concrete, specific hypothesis. I'd just delete this phrase.

Deleted as recommended.

12. Page 8, line 17-18: For the spacing of features, presumably you want some kind of average spacing, which implies a long reach to encompass several "cycles" of the morphology, but your examples are very local – is this a dichotomy? Also note that this hypothesis implies an assumption of stationarity that you should make explicit – basically the analysis is assumed to be invariant under translation within the domain of your study.

Within the 6.4 km study reach there are approximately 10 riffles and pool units, depending on whether a topographic or hydrodynamic basis is used. The examples are meant to show, well, examples of areas within the study reach, and not to imply that only a few morphologic units are present.

13. Page 9, lines 18-19: What is "it" referring to in this case?

We have reworded this for clarity, but in this case "it" is in reference to the wetted extents of water.

14. Page 11, line 20: All data in the supplement should be in metric units, not feet. Corrected as recommended.

15. Supplement, line 34: Define TBR.

Corrected as recommended.

16. Page 12, line 11: Are you defining the thalweg as the location of deepest flow for a given cross-section? Please be explicit about this.

No, we are referring to the traditional definition of thalweg, as the path connecting the deepest parts of a river with downstream direction.

17. Page 12, lines 16-26: I have to question whether a series of flow-dependent centerlines, or sample pathways as you call them, is appropriate. Under this framework, the same location would have a different streamwise spatial reference at each discharge and so your results would not necessarily be comparable from one flow to the next because the streamwise series would not be "lined up." For example, you emphasize the importance of bedrock outcrops, etc., that are not going to move as a function of discharge and yet would have different streamwise coordinates under your scheme. This point also relates back to my comment about stationarity. I think a more robust approach would be to use a single, representative centerline across the full range of flows so that you can be confident that your analyses are in sync with one another. I realize this would involve major re-analysis, but with a separate spatial reference for each stage, I just don't think your results are comparable among discharges.

We have considered this comment and others, and have ultimately revised the analysis using a single centerline to help readers and reviewers have a more simplified framework for comparison. To do this we mapped each flow dependent width series to the pathway associated with the lowest flow (e.g. 8.5 cms) using the spatial join tool ARCGIS. This tool can map, or join, features to another based on whether they directly overlap. Our revision of our original approach is based primarily on making the examples easier to understand by having a common reference.

However, as stated in the text we do not believe having different sample pathways has

any effect on the statistical tests applied in our article, except for the correlation comparison between stage dependent wetted widths, which were mapped to a common centerline. In any case, we have gone ahead and used a common sample pathway in our revision to make it easier for the reader to understand the zoomed comparisons.

18. Page 12, line 25: Constructal theory – how is this relevant? Either elaborate and define this concept or omit.

Given that we are now mapping to a common sample pathway we have deleted this sentence.

19. Page 12, line 27: Why square the velocity? Wouldn't dividing by the lateral cell size be more appropriate to give you a discharge per unit width as the product of depth and velocity?

We clarified the text to address this comment. Note that in classical physics momentum is defined as the product of mass and velocity squared. Therefore, unit momentum can be calculated on a grid of depth and velocity as (d_i*ãĂŰv_iãĂŮˆ2) , where d_iis the depth and v_i is the velocity at node i in the 2D model hydraulics rasters. Most importantly, the patterns generated by using (d_i*ãĂŰv_iãĂŮˆ2) and (d_i*ãĂŰv_iãĂŮˆ) are identical, so the choice between either one is not that important.

20. Page 13, line 5: How was this smoothing accomplished? See Fagherazzi et al. (2004) and Legleiter and Kyriakidis (2006) for one approach to this problem. We used a Bezier curve approach and clarified text to reflect this.

21. Page 13, lines 15-17: Does your analysis consider the cross-stream position of the minimum bed elevation, or is it essentially 1-D? You might want to consider a full coordinate transformation to a channel-centered frame of reference. Otherwise, you're underestimating the distance by assuming that all z values are on your sampling path when they could occur some distance to either side.

Yes, it is 1D. The cross section sampling interval is 5 m, or 6% of the average bankfull width for the reach, and we consider this relatively "tight". There are no cases where the deepest part of the channel immediately zig zag, so we had no issues underestimating distance. In reference to the channel centered frame of reference that is what our current approach does. It uses a sample pathway within the channel to reference bed elevation and channel width. However, given that the bed elevation is now referenced to the lowest flow sample pathway, it is analogous and similar to the thalweg.

22. Page 13, lines 25-26: De-trending the width series is not appropriate because the trend is so weak and probably not statistically significant, given the R2 values in Table 2. Unless there's a compelling physical reason to de-trend, as there clearly is for bed elevation, this step is not necessary. Just use residuals from the reach-averaged width instead.

We believe for consistency all of the data should be detrended to satisfy the statistical assumptions inherent to our data analysis methods.

23. Page 13, line 26: Standardize by the variance? I think standard deviation is, well, more standard.

No comment required.

24. Page 14, lines 5-8: This dependence on length (and location) is the essence of the critical assumption of stationarity, but you should be more explicit about this as it really is critical to this type of analysis.

We have considered the authors suggestion and have added text to the data analysis section.

25. Page 14, line 9: Just multiplying one Z value by one W value at a given location does NOT give you the covariance, as this text implies. The covariance describes how two random variables co-vary with one another and thus requires some kind of sample. Under the critical assumption of stationarity, this sampling is achieved by pooling observations over some spatial extent, not just a single point. Think of it as

analogous to the R2 of the scatter plot with points drawn from within a moving window. Also, if you're using standardized variables, the correct term would be correlation, not covariance. This oversight suggests a fundamental lack of understanding about the statistical concepts involved and casts doubt upon the entire analysis. What you have calculated is not the covariance, so if nothing else the title you have given to your metric is incorrect and must be modified, but I think you will need to revisit the entire analysis.

We have addressed this broader comment in the introduction of this response and no further comment needed.

26. Page 14, line 13: What do you mean by "normative"? This is a very vague term that should be replaced throughout.

Normal conditions in this context refer to areas where both variables are close to the mean and thus the GCS$\sim$0. We have clarified this in the text.

27. Page 14, lines 25-26: Without a sample size, which your point-by-point product does not provide, you have no basis for assessing statistical significance. I'm sorry, but I think a major overhaul is needed to address this important issue.

We have removed the term significant from the examples, which are meant to show how inundation patterns, and thus the GCS, change with flow. Because we are not calculating covariance as the reviewer has assumed the comment of not having a sample size is without merit. However, please refer to Brown and Pasternack (2014) to see how we have assessed statistical significance using bootstrapping in the past.

28. Page 15, lines 10-11: The term "significant" is not appropriate for the quantity you have calculated.

As stated above, we have deleted this sentence and do not use the term significant in the examples.

29. Page 15, line 20: This is what you should be doing within a moving window if you really want to get a covariance. Another approach would be to use variograms, where

you pool pairs of points separated by a set of lag distances – see Legleiter (2014) for the details. I think that paper might help you gain some more insight into the spatial statistical concepts you're talking about but not really doing in this paper.

We appreciate the reviewer's suggestion for this reference, to which we are familiar.

30. Page 15, lines 21-24: This is why a common centerline would be a better choice, then you wouldn't have to resample from one discharge to the next.

No comment needed.

31. Page 16, line 10: Need to define n and k. This ACF is analogous to the variograms and would be a more appropriate way of examining spatial structure. Not clear what x is in this equation, but if you use Z as x in this equation, then you'd have a correlogram, which would be a more appropriate metric than your simple cross-product. To get at the spatial correlation between Z and W you could generalize your equation 1 to use both variables and obtain a cross-correlogram.

Given the similarities between variograms and autocorrelation in measuring variance with respect to distance we are not convinced that it would be more appropriate without further explanation. We have provided clarification on the variables listed in the equation.

32. Page 16, line 12: Be more explicit about the lags used, it's tucked into the distance and number of lags but you should state the lag interval.

Addressed as recommended.

33. Page 16, line 15: explain what a first order Markov process means in terms of geomorphology, and likewise for white vs. red noise.

We decided that the reference to Markov processes was not needed in this section. However, text was added to clarify red and white noise.

34. Page 16-17: The discussion of autoregressive models and red noise is opaque –

what was the rationale for this analysis?

Page 16, Line 16 describes two reasons for this analysis so no further comment is needed.

35. Page 17: The level of sophistication implied by this discussion of spectral analysis, etc., is inconsistent with the lack of basic understanding of the covariance and so the paper comes across as unbalanced. Moreover, this section gives the reader the impression that you're just using advanced methods without really knowing what they are doing. I would advise dropping the frequency domain analysis completely, scrapping your so-called (but not) covariance, and focusing on appropriately calculated correlograms or variograms.

We strongly disagree with this comment. The reviewer has assumed we do not understand basic differences between correlation and covariance. We have addressed this earlier in this response and no further comment or revision needed. The frequency domain analyses are important, because they distill information from the ACF more compactly, and in the process allowing inferences to be made across statistical tests. This are scientifically meaningful and technically sound tools to use for the purpose of this study. As is common in data analyses, many approaches exist and would also yield similar findings, but these are the ones we deemed meaningful for geomorphologist, and so we used them.

36. Pages 17-18: OK, so you acknowledge the impact of different sample pathways and apparently compared results from static vs. dynamic as you called them, but I still think a single pathway would be more logical and save you (and the reader) the confusion of having to line up the same feature at different streamwise locations for different discharges. The last couple of sentences of this paragraph are very confusing and need to be re-worded.

As stated above we have altered our approach and have mapped all sample pathways to a single one, so that it is not confusing to the reader.

37. Figure 3: Add numbers to your quadrants, as you haven't followed the mathematical convention of quadrant 1 in the upper right, then cycling counter-clockwise. I find this figure very confusing and I think your (b) and (c) might be mislabeled as positive and negative – revisit to confirm this.

We have modified this figure based on these comments.

38. Page 18, starting on line 12 and Figure 4: Need to specify flow direction an whether stationing increases upstream or downstream.

We have added an arrow for flow direction.

39. Figure 4 and related discussion on pages 18-19: Because you have a different sample pathway for each stage, the features and stationing don't line up from one panel to the next so the comparison is difficult. You need to label the same features and extents on all three panels, or, better yet, use a common centerline for all discharges. Also, what you have labeled as broad riffle has a low bed elevation, which seems contradictory. 40. Figure 4: The image does not cover the full extent of the plot on the right, which contributes to my confusion in the preceding comment. Zoom out on the image or in on the plot so the extents are equivalent. Also unclear from the legend which line is Z and which is W.

As stated above we are now using a common sample pathway for all flows, so no further comment needed.

41. Page 18, line 19: Given your detrending and standardization, what you describe as significant for Z just means more than one standard deviation from the mean, or a 68% confidence interval – not what most statisticians would consider significant. You might want to back off this terminology.

We have dropped this terminology in the discussion as it is not necessary.

42. Page 19, line 2: Don't you mean -1?

Yes, thank you.

43. Page 19, line 9: Impossible to assess these shifts when the spatial referencing is not consistent among discharges.

We are now using a common spatial reference so no comment needed.

44. Section 5.1: Throughout this section, the discussion would be much more concrete and easier to follow if you placed letters or markers on the plots and images to identify specific locations/features, rather than qualitative descriptive terms for morphologic units with indefinite extents. I found this whole section be hard to follow and not very insightful, though it could be if done more carefully and precisely. These labels need to be on all panels and the more I think about it the more imperative it is to use a common centerline for all stages so that this kind of comparison is even possible.

We have tried to incorporate these comments in the paper and on the figures. In particular we shortened this section significantly.

45. Section 5.1: Also, this very detailed, blow-by-blow description quickly gets to be a bit overwhelming and so I would try to back off and generalize, at least to some degree.

We have considered this comment and attempted to simply where possible.

46. Page 21, line 5: See my earlier comments about "preference" – tendency would be better.

Modified as recommended.

47. Page 22, line 6: This paragraph and Figure 7 are more in line with where I think you should focus your attention, and computing correlograms would allow you to make this analysis spatially explicit and examine the variation at different scales. You should also check out Lea and Legleiter (2016) for another example of this type of analysis.

Given that we are already using correlograms (e.g. autocorrelation) no revision is needed.

48. Page 22, line 11: Yes, but these correlations are all quite weak. That is not surprising, but should be mentioned. You might want to elaborate more on what this implies in terms of the actual geomorphology, particularly the stage-dependence. The observation that the z-w correlation increases from base flow to bankfull and then declines suggests that the bankfull flow really is the channel-forming discharge. This is a key result that you might want to emphasize.

In latter sections we emphasize the importance of this key result.

49. Page 22, lines 11-15 and Figure 8: I don't think you can make this kind of cross-discharge comparison given your different spatial referencing for each flow – one more reason to go with a common centerline.

As stated above we are now using a common sample pathway.

50. Figure 9: Presenting these as a continuous surface interpolated across discharges is inappropriate and misleading. I think these plots would be clearer if you made the correlation as the vertical axis, the lag as the horizontal axis, and each discharge as a separate line. As I mentioned previously, I suggest dropping the frequency domain analysis altogether.

We appreciate the reviewers comment, but no basis for plotting these data as a continuous surface is given, so we are unable to evaluate this comment. We believe the surface plots make the results easier to interpret than a plot with several lines for each flow.

51. Section 5.3 and Figure 9: Are these results aggregated over the full study area or just for one of the examples you showed? Do you have any reason to expect higher correlation at a lag of 1400 m or 2100 m? How does this relate back to the geomorphology?

These are for the entire study entire. In the discussion we related those length scales to those of bars, pools, and riffles defined in other studies, in the process relating this

result back to fluvial geomorphology.

52. Page 23, lines 3-5: This is an interesting result suggesting that the flow field becomes more spatially homogeneous at the highest discharges. I think this would come across much more clearly with the correlogram approach I've suggested.

No comment needed.

53. Page 23, lines 6-19: Drop the frequency domain, not insightful.

No comment needed.

54. Page 24, line 1: Diagnostically is a curious word in this context, implying there's something wrong with the river. What are you trying to get at with this? If you're not trying to make some kind of point here, delete this word.

As stated in the introduction spatial analysis of river organization is important to assess rivers in light of worldwide degradation, so being able to diagnose functional rivers from non-functional river systems from topographic analysis would be important.

55. Page 24, lines 4-6: Regarding lagged effects, it seems like the topography would have to be lagged relative to the flow field if a perturbation has to advect downstream, which would require some time and therefore distance. This is related to the topographic steering concept and might be worth discussing further.

This is an interesting suggestion we have considered.

56. Page 24, lines 15-16: You don't really know the distance of such a shift unless you use a common spatial reference.

As addressed in the introductory response we are now using a common spatial reference, so no further comment needed.

57. Page 25, line 6: Regarding "top-down organization," these results suggest that every river is unique and contingent upon the local particulars of geology, land use, and

history and that our idealized notion of purely alluvial systems might be an oversimplification,if not altogether misguided. Perhaps something to consider further for your discussion.

Thank you for this constructive comment.

58. Page 25, lines 13-14: What do you mean by "non-persistent riffle"?

One that does not persist in a location over time.

59. Page 24, lines 14-18: This idea of diagonal steering sounds interesting but I'm having a hard time picturing the process – a simple conceptual sketch here would be helpful.

We thank you for your interest, but given that the paper already has 9 figures we believe an additional figure for a peripheral discussion component of the paper is unwarranted.

60. Page 26, lines 1-4: Legleiter et al. (2011) examined the stage-dependence of topographic steering effects in a meandering channel and some of the concepts discussed in that paper are relevant here, so might be worth checking out. In general, a scaling of terms in the force balance would be insightful. I suspect that at the largest flows the topographic steering effects are negligible and the force balance simplifies to gravity and friction.

This is a good suggestion, but we feel would detract from the overall point of this paper.

61. Page 26, line 17: This is also a matter of time scale, as reconfiguring the valley walls, particularly if bedrock controlled, is going to take a lot longer than reshaping a gravel bar. That said, these grain-scale, engineering time scale kinds of processes over time could influence the larger scale valley form as well.

No comment needed.

62. Page 26, line 22: Just report lengths scales, not frequencies.

No comment needed.

63. Page 27, line 4: If you use correlograms or variograms these periodicities will emerge from the analysis more naturally, if they are present, and will be easier to interpret.

We did use correlograms (e.g. the autocorrelation function), so we are unsure of this comments merit or point.

64. Page 27, line 17: "indicative of normative conditions" is an empty phrase, what do you actually mean by this?

Normal conditions in this context refer to areas where both variables are close to the mean and thus C(Z,Wˆj)$\sim$0.

65. Page 27, line 19: This is another place where a consideration of the force balance would be helpful.

As stated above, this is a good suggestion, but we feel would add a level of analysis not needed for this papers original goals.

66. Page 28, line 2: Chin – a reference to step pools seems out of place in this context – can you find a similar reference for larger, alluvial rivers?

Yes, we have deleted this reference and added a different one.

67. Page 29, line 13: Legleiter (2014a,b) compared the reach-scale spatial structure of natural and restored rivers and should be referenced in this context.

We disagree with referencing this work here. In this section our intent is not review all methods for analyzing the spatial structure of rivers, but to suggest how this newly developed method could be used.

68. Page 30, line 20: Another relevant, recent publication to cite here is Hugue et al. (2016).

Thank you for this interesting citation.

References

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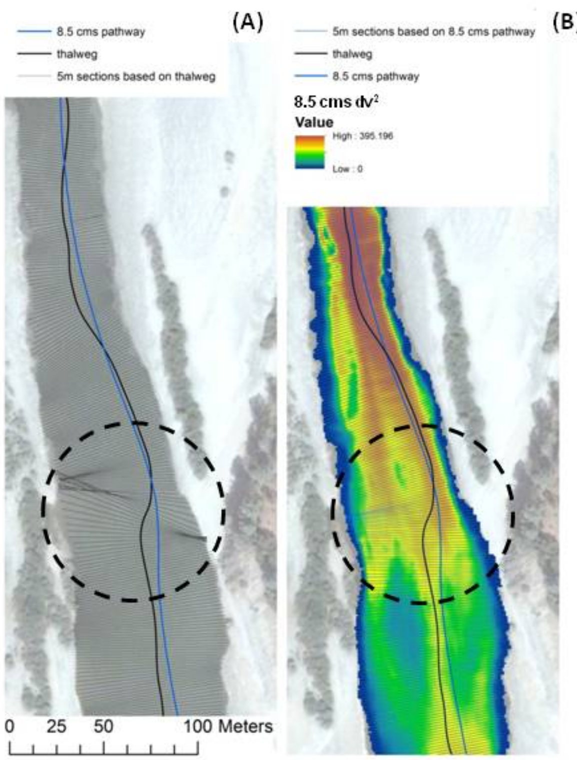

Fig. 1. Traditionally defined thalweg (A) and flow dependent (B) sample pathways used in this study with 5m spaced cross sections clipped to the 8.5 cms flow

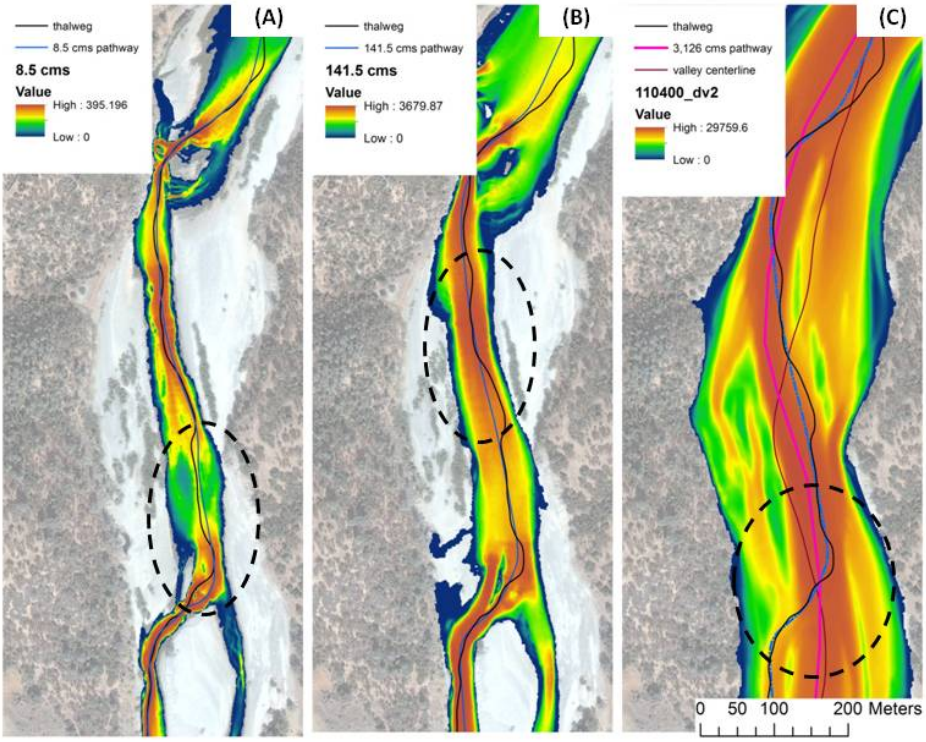

Fig. 2. Momentum grid for the 8.5 (A), 141.5 (B), and 3,126 (C) cms flows along with the sample pathways