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Earth Surface Dynamics
Discussions

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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General Comments This paper describes a method for analyzing width and depth variations and different flow stages to try and look for covariance of width and depth oscillations. I agree with the author's final statement that geomorphic covariance structures (GSC's) hold promise and I especially liked the broader implications section, but I also have some concerns with the current manuscript that should be addressed before the final version is acceptable for publication.

Q1: In particular, the authors need to clearly discuss the limitations of using a single set of topographic data to infer both high-flow and low flow depth variations. The current bed morphology is a reflection of the discharge history in the last decade or so, but the

authors do not discuss historic peak flows in any detail.

A: We answer the first part of this question in our response to Q3 below, so please see our response. However, we have also added more text and information related to peak flows in the study section. Hopefully this provides more clarity on the hydrology of the study river.

Q: In addition, the authors need to explain how variations in valley width are the primary control on depth variations if the covariance of depth and width are highest at intermediate flows, not the higher flows most impacted by valley width. This is particularly important given the fact that the measured bed topography might be expected to reflect the approximately 20-year recurrence interval flood that occurred just prior to LiDAR data collect, but apparently does not to a great extent.

A: We did not state that valley width is the primary control on depth variations in the text. Instead, we found that minimum bed elevation and flow width were significantly correlated for all flows, but were most correlated at low to moderate flood flows with recurrence intervals less than the 5-year event.

Q2: My main concern with the analysis stems from the fact that the authors used a single bed topography to infer depth conditions for flows that range from the mean annual flood to a 20-year recurrence interval discharge. My concern is that low flow topography is assumed to be static and is used in the 2-D model of high flow conditions on the river. It is very likely that the bed topography during the 20-year flow is very different than what is modeled, which then raises the question what does the covariance for W and Z mean if the channel morphology modeled is not a function of the discharge modeled.

A: First, we need to clarify a few things. The topography of the river is independent of flow and was mapped comprehensively for the entire lower Yuba River in one 2-year effort, with all of Timbuctoo Bend mapped in one survey effort during summer and fall 2006. Thus, the river truly has a single topography, and the goal of this study was to evaluate the coherent spatial patterns inherent to this snapshot in time. This study does not infer depth conditions and does not use depth. Instead, it uses standardized, detrended bed elevation, which is not stage dependent the way depth is, in terms of the hydraulic perspective of imposing flow on a static topography. Meanwhile, we needed some way to get at the width associated with different water stages to see how bed elevation and width relate. A way to do that is to run a numerical model at meter-scale resolution that accounts for the effects of channel non-uniformity on flow acceleration assuming a static topographic boundary condition. This study is nothing more than an analysis of topography, and thus the comments about what is going on during a flood do not apply directly to what is being tested in the goals of this study. We agree that during a flood, rivers change, but there is no way to avoid the reality that the process of mapping in a real, large river is a snapshot in time. Mapping a river's topography in real-time during a 20-year flood in meter-scale resolution over 37 km is impossible for the foreseeable future. Running a morphodynamic model with the same attributes is also impossible at this time, and even if it could be run, the results in terms of dynamic changes to bed elevation and width would be highly speculative at best given current models. Geomorphology is founded on the principle of using observable landforms to infer past processes and predict future responses (e.g., Thornbury, 1954). Therefore, the solution is to use meter-scale topography to make assessments of the processes as posited by existing theory and then see if metrics produced from topographic analyses match their expected values and/or ranges. This standard approach is what we have done, but applying it to much more detailed data and a new concept of topographic structure than attempted before. Perhaps someday geomorphologists will be able to track and evaluate dynamic fluvial changes during large floods.

Q3: The authors do a nice job referencing K.S. Richards' important work in the 1970s, but they have not addressed one of his main points, which explains that the observed channel morphology is a reflection of erosion and deposition inherited from a range of previous flow conditions. It is unlikely that the bed topography measured in the LiDAR survey conducted at very low flows corresponded exactly to the bed topography that

would have existed during the 20-year event months prior. In fact, many of the features responsible for the "topographic steering" described by the authors are depositional bars, but it is unclear what flows may have created various bars and how those bars may have been reworked at lower flows. As the authors state in the discussion (page 29, line 1-15), "the topographic structure of the river change with flow." The also state "subsequent more frequent flows erode through these (flood) deposits" (Page 25, line 23-24). The authors need to address more directly how these conditions could skew their results.

A: The reviewer has drawn attention to a long standing conundrum in geomorphology. That is, it is impossible to associate channel geometry with a singular flow discharge of certain recurrence because the role of flow depends on current channel form, which is a reflection of past flows (Yu and Wolman, 1987). With that, we do not believe these analyses can untangle singular or absolute flows responsible for the observed channel topography, because as the reviewer alludes, river conditions are a reflection of past and current conditions, neither of which can be decoupled from the other (Yu and Wolman,1987). This is a general theme that we have added throughout the manuscript to avoid the notion that any one flow is more or less responsible for channel topography.

Q4: The covariance results (Figure 7) indicate a strong relation between depth (Z) and width (W) for flows near the bankfull level and lower correlation for both lower and higher magnitude flows. In looking at the USGS flow records for the Lower Yuba River, it appears that the last approximately 20-year recurrence interval flood occurred in late 2005 months prior to the LiDAR survey. It seems very likely that riparian vegetation was damaged by that flood and had little time recover. It is also likely that flow events in early 2006 reworked the flood deposits to some degree. It seems very unlikely that the bed topography immediately after the 2005 event would exactly match the bed topography during the 2006 LiDAR survey, but we have no way of knowing how much change might have occurred. It is also worth noting that even the bed topography immediately after the 2005 event would have been modified by discharges on the receding limb of the flood hydrograph. This lack of data on flood channel morphology frustrates almost all studies of this nature, but the authors still need to clearly address how this lack of information limits their study.

A: This is a valid point that we did not emphasize or discuss enough in our submission. As such, we have provided additional text throughout the manuscript that addresses this point. For example, in the experimental design section we state that "this study aimed to deconstruct and reveal the coherent topography structure of a heterogeneous river valley as it existed at the moment of its mapping. This understanding ought to inform both how the river arrived at this condition as well as how it might change into the future, but this study does not involve analysis of morphodynamic change to directly seek such linkages."

Q5: It is also important to remember that width and flow interactions are not a one-way process. Valley width does not just impact the high-flow flow conditions; the flow of the river dynamically adjusts the valley width too.

A: Because velocity and Shield stress are not uniform across a channel, but focused along a particular streamline, it is easier to cause localized erosion, especially down cutting, compared to widening in this river. If a location is undergoing noncohesive bank migration on one bank, then chances are it is experiencing point bar development on the opposite bank, yielding no net change in width. A process such as avulsion can cause rapid and effective change in wetted width though. It would be an interesting study unto itself to evaluate the relative roles of vertical change versus lateral change in the river using this dataset.

Q6: Do the authors have any data (aerial photographs through time) that might highlight areas along the study reach where valley width has been increased versus more stable sections of the valley? I would be much more comfortable with this article if the authors directly addressed these issues.

A: Valley width in this river is predominantly bedrock, with the exception of two tailing

piles in the upper section of the reach. We have added text in a new study section that discusses this in more detail, including references that address the reviewers question such White et al. (2010).

Specific Comments 1. Page 4, line 4: I would appreciate seeing a general hypothesis at the end of the introduction. I have no problem with more detailed hypotheses appearing later in the paper, but I believe it is important to give the reader a general sense of what ideas are being tested at the onset of the paper.

We have added a general hypothesis at the end of introduction.

2. Page 7, line 8-10: This is the third time I have read what appears to be the exact same sentence (in abstract, introduction and experimental design sections). Obviously, the paper can be written more concisely in this specific case and in general.

This sentence and its duplicities have been edited for conciseness.

3. Page 9, line 21: It would be useful to know how flow regulation may have impacted the recurrence intervals for flows.

We have re-written the study background section to address this comment. In general flow regulation has resulted in increase flows in the summer and fall, where flows historically were highly variable.

4. Page 11, line 18-20: The authors should in the text (not just in the supplement) describe when the LiDAR data was collected and its relation to the flow conditions preceding data collection. It appears that LiDAR data and bathymetery data was collected a few months after a 3,228 m3/s event. The authors need to discuss how things might have been different if the LiDAR data was collected years after one of the larger events.

We have rewritten the study section to address this and other comments related to providing better context for the study river.

5. Page 18, line 16: I am concerned that here and elsewhere the authors talk about

point bars bounding, confining and steering flow. Point bars are depositional features that are typically comprised of some of the smallest and easiest to transport sediments along a reach. Considering that these features were deposited by flowing water, it seems misleading to suggest they control flows at various stages without the flows also being able to reshape the deposits at those various discharges.

Jackson et al., 2013 report substrate mapping results for the Lower Yuba River. In the study reach during the study period surface grain sizes range from gravel to large cobble, with a mean of 164 mm (Table 1; Jackson et al., 2013). In addition to facies mapping this study also stratified sediment distributions by landform type at the sub-reach scale (e.g. morphologic unit). Their study shows that lateral, medial and point bar morphologic units all have sediment size distributions dominated by cobbles.

6. Page 19-21, Section 5.1: I found the description of the flow at various discharges overly detailed and unhelpful. I believe this section can be written much more concisely with just general trends.

We have revised this section for conciseness.

7. Page 19, line 10: Is it possible to have a negative width expansion? Are you talking about positive GSC?

This sentence has been reworded for clarity.

8. Page 20, line 26: The authors describe the river as self-formed, but flow regulation, general incision and the impact of tailings piles all suggest an adjusting system. The authors should more clearly discuss how longer-term river adjustments might be impacting the observed channel morphology from a single year. The authors hint at the impact of the tailings piles on page 24, line 18, but a more organized section of caveats would be more helpful.

We have rewritten the study section to address this and other comments related to providing better context for the study river.

9. Page 23, line 25: If pools and riffle are defined by their bed elevations, it seems selfevident that they will correspond to high topographic extrema. Am I missing somethingmore involved with this statement?

Yes, but the second half of the sentence refers to the result that areas of relatively low bed elevation also have relatively low widths, and vice versa.

10. Page 24, line 22: Suggesting that "alternate bars channelize flows" implies that the deposits are more stable than in reality. These are sediments that can be reworked by most modest flows I assume (I do understand they are discussing low flows in this case, but the term "channelize" still seems misleading).

The reviewer is correct in that this statement refers to low flow conditions. We have replaced "channelize" with "confine" to avoid potentially misleading readers. Further, we have provided more information above related to the sediment caliber of various bars in the study river that show they consist largely of cobbles, and thus at low to moderate flows can steer water flow. In many cases in the literature point bars and other sedimentary deposits can steer flow, provided the energy of the flow is not great enough to mobilize the bounding sediments (Dietrich et al., 1979).

11. Page 24, line 28: As previously stated, suggesting that a point bar "constricted" a potentially channel-forming flow seems to ignore the basic process that forms point bars.

It is important to highlight that since the river is partially confined by bedrock that there are exogenous controls on river planform. Therefore, while unconfined alluvial point

12. Page 25, line 10-12: The authors suggest that depth variations adjust to width. It certainly seems logical that bedrock outcrops and other constrictions could impact depth significantly, but the authors need to clarify that the river had recently experienced a large flood that inundated much of the floodplain. Again, the authors should discuss how the bed topography might have been different if flows had not exceed

the 5-year recurrence interval for several years prior to topographic characterization. We have rewritten the study section to address this and other comments related to providing better context for the study river.

13. Page 26, line 1-4: Do the authors know if the riffles in the bend were formed during or after the 2005 event. Is it possible that the riffles and bends are features created at different stages than each other? Air photographs suggest that these riffles were present before and after the 2005 event. It is entirely possible, and likely, that the riffles and bends are created or maintained at different flows from each other.

14. Page 26, line 20-21: Does the coherent power connection with the 1.5-year event reflect the dominant control or just the most recent flow to impact the morphology?

We have addressed this general topic in Q4 above. To restate, it is not possible to associate channel geometry with a singular flow discharge of certain recurrence because the role of flow depends on current channel form, which is a reflection of past flows (Yu and Wolman, 1987). With that, we do not believe these analyses can untangle singular or absolute flows responsible for the observed channel topography, because as the reviewer alludes, river conditions are a reflection of past and current conditions, neither of which can be decoupled from the other (Yu and Wolman,1987). Again, this is a general theme that we have added throughout the manuscript to avoid the notion that any one flow is more or less responsible for channel topography.

15. Page 27, line 10-12: It is in relation to statements like these that more discussion on the flow history is needed. It is not surprising to me that moderate magnitude annual peak flows are most highly correlated with channel morphology, but it is more surprising in light of the higher flow event just prior to characterization of the bed topography in this study. Does this suggest the 20-year recurrence interval flow was unable to substantially modify the channel morphology established by the 1.2-2.5 year recurrence interval flows? Or did more recent flows modify the flood deposits?

We have tried to address this comment in Q3 above. It is also important to step back

from absolute metrics of flow and appreciate that flood events consist of a range of flows as the hydrograph rises and falls. In addition to magnitude the duration of flow is also important in modulating geomorphic work in rivers (Wolman and Miller, 1960). While the flood that occurred prior to mapping had a peak flow of 2,721.25 m3/s (96,100 ft3/s) this peak only lasted one hour before receding.

16. Page 29, line 1: It would be wonderful if $C(Z,Wj) > 0$ could be used to identify spawning areas. However, if $C(Z,Wj) > 0$ characterize at least 55% of the reach at all flows and we then include adjacent areas, then $C(Z,Wj) > 0$ is not a very powerful tool to pinpoint zones that may represent a small portion of the study reach (I assume identifying spawning areas would not be an issue if the spawning areas existed over large areal extents of the study reach).

This was actually meant to be $C(Z,Wj) > 1$, so we have corrected this sentence.

17. Page 29, line 5: Riffles are depositional areas at high flow and it seems likely that bedload transport is fairly high at those times. Therefore, I question how valuable these areas are for flood refugia. Eddy and deadwater zones would seem to be safer places for juvenile salmon during floods. The importance of eddy zones would certainly seem to be consistent with the increased awareness that large-wood jams are critically important habitat features in many salmon rivers.

We are referring to laterally distributed hydraulics in this context, and have revised the sentence accordingly. While bedload transport will be directed within the core of the channel center, there are lateral zones where flow velocity and depths are relatively low.

18. Page 29, line 22: If you assume constant water slope, aren't you implicitly suggesting that variations in width are not important as controls on water-surface slope (no backwater effects). This seems like an odd statement to make in a paper that is trying to demonstrate the importance of valley width on channel morphology. Previous studies have shown a linkage between localized water-surface slope and channel

morphology.

We qualified the idea of using a constant water surface slope for flows above bankfull discharge, since in some cases water surface slopes could be relatively constant. Given that we stated it could be a "potential" relaxation from using numerical model we do not believe this is an incorrect statement.

19. Page 29, line 25: The authors have generally described the Yuba River as a constrained system, but here there is discussion of large alluvial rivers. It seems beyond the relevance of this study to apply the results to large unconstrained rivers.

In this context we are not applying the main results of this study to other large alluvial rivers. We are referring to the potential for the methods used in this study may be used to analyze and compare amongst rivers how topographic structure may change with flow. We believe future studies can further evaluate the utility of the method on a more diverse array of river reaches and segments.

20. Page 30, line 12: The authors really need to explain why they think covarying values decrease for flows with recurrence intervals of 5-years and higher. Again, this seems to suggest valley width has less control on depth than other factors.

We have added text to the manuscript in the discussion that speaks to this result and our interpretation. As the reviewer noted, this result suggests that incision may be decoupling the relationship between valley width and minimum bed elevation.

21. Page 37: It would be useful to understand why these specific flows were selected. A hydrograph showing flows for the last 10-years would also be very helpful.

We have added text in a newly rewritten study site section that discusses the flow regime in much more detail. In addition, we have explicitly stated why the flows were selected, so we hope it is more clear.

22. Page 38: The linear trends for width have negative slopes. Is the width decreasing in the downstream direction and why?

Given that the trends are a function of distance, starting downstream, the negative sign indicates a slight narrowing in the upstream direction, or widening in the downstream direction, depending on orientation.

23. Page 46. The R2 values are fairly low for the plot and the residuals don't look randomly distributed (no values in the Z = -1.5, Wj = 1.5 range).

No response required.

Technical Corrections: 1. Page 4, line 18: comma after "discharge"

Corrected as recommended.

2. Page 4, line 20: hyphenate "riffle-pool couplet" here and elsewhere.

Corrected as recommended.

3. Page 5, line 8: Comma after "perspective"

Corrected as recommended.

4. Page 20, line 10: comma after "riffle" This entire section has been rewritten to be more concise per earlier comment.

5. Page 29, line 5: comma after "example" Corrected as recommended.

6. Page 42: The letter headings should be lower case in the figure to match the captions. Corrected as recommended.

7. Page 43: The stations on the aerial map and plot do not seem to match exactly. The map begins at approximately 100 and end at 1600. The plots begin at 300 and end at 1700. It is not clear why? A similar issue is evident in Figure 5. We were using varying sample pathways in this submission, but have since revised the analysis using a common sample pathway. This should be much clearer in the revised manuscript.

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		Reach scale	Morphologic unit scale		
Substrate category	**Size class**	**Percentage**	**Medial bar**	**Lateral bar**	**Point bar**
Silt/clay	<0.0625 mm	0	0	0	0
Sand	0.0625 – 2 mm	0	0	0	0
Fine gravel	2-32 mm	0	20	10	10
Small cobble/medium gravel	32 – 90 mm	20	40	30	30
Cobble	90 - 128 mm	30	30	30	30
Large cobble	128-256 mm	30	0	10	10
Boulder	>256 mm	20	0	0	0

Fig. 1. Table 1. Median grain size classes at the reach scale and morphologic unit scale for channel bars in the study reach. Note that because the data is only to the nearest 10% and because it is a median