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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. 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. 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.

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.

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.

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.

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. 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.

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.

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.

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

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.

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.

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.

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

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.

9. Page 23, line 25: If pools and riffle are defined by their bed elevations, it seems self-evident that they will correspond to high topographic extrema. Am I missing something

more involved with this statement?

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).

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.

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.

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?

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?

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?

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).

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.

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.

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.

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.

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.

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

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).

Technical Corrections:

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

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

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

4. Page 20, line 10: comma after "riffle"

5. Page 29, line 5: comma after "example"

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

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.

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