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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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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

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Discussion paper

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.

Specific Comments:

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 morphology. 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.

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?

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

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

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.

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

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.

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.

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.

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. Pleas replace "preference" with "tendency" throughout.

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.

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.

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

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

15. Supplement, line 34: Define TBR.

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.

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.

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

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?

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.

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.

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.

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

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.

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.

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

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.

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

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.

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.

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.

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.

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

34. Page 16-17: The discussion of autoregressive models and red noise is opaque – what was the rationale for this analysis?

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.

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.

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.

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

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.

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.

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

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

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.

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.

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

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.

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.

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.

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.

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?

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.

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

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.

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.

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

use a common spatial reference.

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.

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

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.

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.

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.

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

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.

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

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

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?

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.

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

Technical Corrections:

The paper included a number of wording errors, typos, omitted references, and other mistakes; please see the attached PDF for these relatively minor edits.

Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/esurf-2015-49/esurf-2015-49-RC1-supplement.pdf

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