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Discussions

Interactive comment on "Wavelet and index methods for the identification of pool–riffle sequences" *by* M. Mounir et al.

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I was accepted to undertake this review on June 24 and I completed the review today, July 22, so I did the job within the typical 4 weeks allotted.

Review Synopsis

The authors present two methods for mapping the locations and extents of 1D longitudinally arrayed riffles and pools and quantifying the spacing between successive pools and that between successive riffles. The method are applied to up to 6 reaches and then they are inter-compared as well as compared to a classic method from the 1980s. The study conclusion is that all 3 methods yield roughly similar results, with the proposed wavelet method also providing riffle-pool relief, though those this can be

obtained from a variety of pre-existing methods, too. There are no scientific conclusions about riffles and pools in the study reaches presented or discussed relative to the literature on the topic. Upon very thorough inspection and close reading, I have many questions and discussion points about the methods that would need to be clarified by better and more thorough writing in a major revision, likely including a meaningful supplementary materials file rather than blasting the primary manuscript to an unreasonable length. I think the index method as applied with the selected variables is technically unsound, but I would grant the authors a chance to explain better and justify their choices. The structure of the manuscript also needs to be overhauled to fit a better framework with a clear experimental design. I cannot come to a final conclusion without first having all questions answered, as detailed below in the broad narrative review and then the detailed specific comments I provide. Therefore, I recommend major revision and further external review, ideally from other viewpoints than my own by others who also work on spatial series of river topographic data.

Narrative Review.

As I understand the open review process for HESS and its sibling journals, the goal is to have an open discussion among the community about a manuscript to bring to light a wide range of issues related to the submission that can help make it better, while also performing a critical analysis to aid the journal's decision to accept or reject an article. As a long-serving associated editor, reviewer, and author for several journals, my preference is to offer the journal and authors my best effort at thinking through a manuscript in great detail to give the best insights and ideas I can offer to help to constructively improve the work and have it come to its fullest potential. My reviews are long and substantive, which I think is good for making a high-quality open discussion and final manuscript, but I know it can feel burdensome to authors, because then they have to reflect on all the issues I raise. In this case, this manuscript is right in the center of the scope of research I do on fluvial geomorphology and I have published several articles that use other methods to achieve the same goals as here, and more.

Therefore, I definitely have a lot of experience and insight about the contents of this manuscript. To be fair and open, I do mention my own research articles in this review, because it is one of the topics I am a recognized expert in, but I also do balance that by citing excellent articles by other experts from around the world. It is up to authors to decide if they want to cite my articles or not, I cannot expect them to, but I do think all the articles I mention in my review from myself and others do provide important insights that bear on the article here and it would be wise for the authors to at least consider their merits given that the manuscript's literature review of morphological unit analysis methods pretty much ends at 2001, not 2019.

In this manuscript the authors undertake analyses of cross-sectional river channel topographic and hydraulic data to achieve 2 methodological goals: (a) map the locations and extents of 1D longitudinally arrayed riffles and pools and (b) quantify the spacing between successive pools and that between successive riffles. The manuscript provides a literature review about how to do these two things beginning on the bottom of page two and continuing through section 2 ending on page 6, almost exclusively recalling articles from the 20th century. The fundamental premise of the article and the strategies taken are similarly rooted in 20th century data collection methods, which then constrain the analysis methods, results, and comparisons. In the 21 century, the emergency of meter-resolution digital elevation models (DEMs) of entire river corridors has fundamentally transformed the breadth and depth of not only analyses but the very questions that can be asked. On a practical level, there have been two developments that the literature review misses that are very important to the study's context and should be addressed to have a modern summary of the state of the science. First, meter-resolution topo-bathymetric DEMs of rivers have enabled for not only higher resolution bed elevation spatial series to be developed showing many more subreach scale fluctuations than considered in the 20th century, but also stage-dependent river width series (at elevations associated with various lateral geometric slope breaks and/or discharges) and other variables. These spatial series can now be produced for many variables in vastly higher resolution, such as a spacing of 1-5% of bankfull width

rather than one every $\sim$ 0.5-3 bankfull widths used in this study. Further, the advantage of joint analysis of depth and width is that the spatial series of the "geomorphic covariance structure" (sensu Pasternack et al., 2018a,b and preceding work) between them controls the morphodynamic process of flow convergence routing, as explained with citations in the detailed comments below. My own lab group's research program pursues this course of inquiry to not only map morphological units and their spacings, but to also link topographic patterns to morphodynamic processes, and even do that in a way that transcends spatial scale for the first time. Second, meter-resolution topo-bathymetric DEMs of rivers enable fully spatially explicit (lateral and longitudinal) analyses of river corridor terrain. This comes in three varieties. First, Prof. Martin Thom's research group is inventing entirely new 3D DEM statistical metrics to characterize whole surfaces, which may eventually lead to new ideas about morphological unit segregation or abandonment of that concept in favor of continuous surface analysis. Second, Dr. Carl Legleiter's research group has been using geostatistical methods to also evaluate river corridor DEMs, including their spatial autocorrelation and related topics. This has the same conceptual potential as Martin Thom's research but using different statistics. Finally, my own group and that of my former student Prof. Joe Wheaton are pursing methods at making spatially explicit morphological unit maps- in my group's case taking advantage of 2D hydraulic modeling to obtain depth and velocity grids that can be classified with decision-tree analysis at the proper discharge and in his group's case by doing direct topographic analysis to segregate fluvial landforms using 3D topographic geometry rule sets. In short, the 21st century is seeing an exciting and rapid expansion of approaches to mapping fluvial landforms, evaluating their spacings, and going beyond that to link patterns to processes. None of these 21st century developments are mentioned in the literature review, which means the authors may not be aware of them. In the detailed comments below, I have tried to cite examples from the above mentioned new works specifically, and of course that means I'm also citing my own group's research, but I only do that because it is directly producing the same kinds of results as here, just with some newer ideas and high-resolution datasets. So the literature review needs work as the methods for riffle-pool mapping and spacing quantification did not end in 2001, but what about this study as a whole, where does it stand with its ideas?

This study lays out two methods and presents their results. The so called index method and then wavelet analysis, both applied to identify locations and extents of riffles ands pools, and then to quantify their spacings. Considering just these methods in isolation, there are both positive developments and several concerns to be addressed. Going in reverse order, I totally agree that wavelet analysis is an important and meaningful tool for analyzing spatial series, though the real potential comes from analyzing meter-resolution river corridor DEMs. Wavelets are not new to hydrological or geomorphic research (see citations below), so merely applying them is not novel though it is meaningful. The wavelet study and associated developments in the manuscript are meaningful and really constitute the merit of the manuscript, with a variety of detailed methodological questions raised below notwithstanding. I must point out though an important criticism and caveat in the results that the authors unfortunately gloss over too much, which is that the results show that the wavelet method could only characterize units for no more than 50% of the main test reach (#6), and that makes it far inferior to pretty much all other methods one can use. It is unclear if the method was applied to all six reaches or not because of some poor methodological explanations, but if so, then the authors should explain more about how much of each spatial series get a riffle-pool characterization in each case given the different cross-sectional densities in each reach. Given better and more data, it can do more in theory, but it will always have to trim the ends of the series. Still, the real value of wavelet analysis in my viewpoint is that it can provide the required parameters to drive procedural river design of sub reach scale fluctuations in detrended bed elevation and in all the lateral offsets of lateral reach brake (e.g., bank top, floodplain top, Terrance toes and tops, etc.). Such parameterization is the basis of the procedural river generation software my lab group has published called “River Builder”, but we have not yet formalized a wavelet parameterization. These authors could make a helpful contribution by using their study results to report parameters that would specify how to make rivers naturally fluctuation in a

non stationary way down their length. That is the exciting potential of their research when mindful directed toward that purpose.

My main concern has more to do with the so-called index method. Conceptually, I have absolutely no problem with the idea of defining riffles and pools on the basis of spatial series of multiple variables, rather than just that of bed elevation. In fact, my group’s research articles already do that. The problem is that the choice of variables used in this study (detrended bed elevation, hydraulic radius, and Froude number) seems to me to be technically unsound, because they are highly co-dependent. In fact, both hydraulic radius and Froude number can be derived as a function of detrended bed elevation, while Froude number can also be derived as a function of hydraulic radius as well. Whatever independent information (i.e., unrelated to detrended bed elevation) is contained in hydraulic radius and Froude number is not native to those variable, but comes from the underlying variables that determine them, which, for a fixed discharge along a reach (as the study here is about changes in a variable along the reach), comes down to width, slope, and bed roughness (e.g., surface substrate grain size, form roughness, etc). Together, detrended bed elevation (essentially the inverse of depth but just with a shifted vertical datum), width, slope, and surface roughness entirely define and explain Fr and Rh, as can be derived analytical for a simple channel geometry. For a complex geometry, width is dependent on cross-sectional geometry more generally, but this is a minor technicality- if one has to extract and analyze spatial series to understand riffles and pools, then metrics for depth and width are what matter. To go further, if the river is meandering, the of course one would also want to add the spatial series of thalweg planform curvature, which is a topic the authors do not address even though I think their channels do meander. Meanwhile Fr and Rh are just dependent response variables, so they are not requisite. Further, the significance comes down to the fact that one can use spatial series of detrended bed elevation and width to not only assess locations of riffles and pools, but more importantly subdivide those two landforms in two types each with respect to how they will evolve under the direction of the morphodynamic process of flow convergence routing, per the journal articles my lab group published in

2018 in ESPL, as cited above and below. There are other technical questions as well about the index method I raise below, but the fundamental concern I raise comes down to this issue of the selection of 3 variables that are highly co-dependent. I argue those are the wrong choices and I am based my arguments on physics, whereas the authors provide some exploratory multivariate statistics using PCA to try to substantiate their selections, which doesn't hold up against physics foundations. The fact that one can map riffles and pools with the index simply derives from the fact that we already know one can do that using detrended bed elevation alone, so any variable that is derivative of it will also work. Whether adding the other variables helps more or not, is clouded by the fact that the other variables are not the true independent ones that should be used, but are dependent response variables. Thus, my perspective is that the index method is not sound, whereas the wavelet method is. Therefore, my recommendation to the authors is to cut out the index method form the article and expand the work using the wavelet analysis per my comments because that is the more important method that will carry into the future.

Looking beyond the concepts of the methods, I also find that the methods are not describe well enough for readers to understand them and accept them. I have raised many methodological questions in the detailed comments below. Of course, a manuscript can not explain every nuance, and that is why many people now submit supplementary materials files along with their manuscripts. I am unaware of one for this article and in searching the manuscript text there is no reference to a supplemental file, so I do not think there is one. Before I can really evaluate the full soundness of both methods, there are several fundamental questions that have to be answered by the authors to clarify the data itself (what is it and how was it obtained, for example) and the analysis methods. I recommend moving some of the current text to a supplementary materials file to shorten the main manuscript but then adding in all the details I raise with questions. Remember, if a study cannot be replicated, then it is not science, as is now commonly understood through the devastating replication crisis in science. Ideally, people should be able to apply these methods but to do that they have to be

explained well.

Considering the results of the study, the article focuses on inter-comparison of the two methods and between them and one pre-existing method. That is ok insofar as it goes for a methodological study. However, I always prefer to see a blend between a methodological development and a new scientific development, because why adopt a new method over the old one(s) if it does no better? In other words, what have the authors learned about rivers from the new results they have obtained that might inspire others to care about these methods? If their method is only as good as the older method, then it is hardly significant or worth considering. The new method has to bring something new and valuable. The introductory motivation is purported to be for flood forecast modeling and I do not see anything that suggests to me the new methods offer value toward that end that other methods do not already provide, especially considering the 21st century methods I mentioned above that look at spatially explicit analysis or depth-width geomorphic covariance structures. These newer methods use the same DEMs as modern 2D flood forecasting models, which is why they can have more bearing on that application. As I previously mentioned, I do see value in wavelet parameterization for sub-reach-scale river design though. In short, I think the authors need to add some results and discussion that states the scientific significance of their findings and relates that to research in the literature. This is all the more true when one deletes the index method and focuses on the value in the wavelet analysis. Can anything be learned about riffle-pool rivers especially form wavelet analysis that we do not already know? I am confident the answer is "yes", but the authors need to explain that.

The structure of the manuscript needs to be re-organized to fit the scientific method more strictly, as this would be clearer and better for readers. Right now there is too much blending of introductory text, methods, results, and discussion in many sections, rather than having mindful segregation with a clear "experimental design" to the study and its writing.

Finally, as the authors point out in the manuscript, while some studies are at the forefront of science with the most-advanced, highest-quality, highest-resolution topographic datasets and analysis methods, the reality is that many rivers around the world are not mapped in such detail and will continue to rely on a small number of cross-sections for the foreseeable future. It may be that for such widespread yet limited datasets, classic approaches from the 1980s and derivative concepts will still be relevant and used. In that light, I think there is value in further pursing this manuscript to see what value it can offer after a major revision accounting for the detailed comments provided here. I'd like to see a major revision and find out where it goes. Can the authors further explain and justify their methods? Can the index method and the specific selection of variables be defended successfully or can the authors amend the method to use better variables with a better outcome? What is the science discovered about the riffles and pools of the 6 rivers studied, or is this just a methods study? I'm open to seeing how the authors go from here and I always think authors deserve the opportunity to respond and produce a better manuscript. I always appreciate when I am given the chance.

-Prof. Greg Pasternack UC Davis

Specific comments:

It is unfortunate that the manuscript does not have continuous line numbering to aid reviewers and editors with referring to locations easily, even the repeating page numbers are only every 5th value, which is not convenient. Actually, based on page 8 where there is new numbering at the onset of section 4, I am totally confused as to how line numbering is done and it makes it harder to review the paper in a discussion format that requires me to write out all my comments rather than simply mark up a manuscript. In future manuscripts, always include full and continuous line numbering.

First 2 paragraphs of the introduction. It seems odd to me that the main reason why anyone should be interested in understanding the sub-reach variability of river topography is because of the potential application of such information to flood forecast model-

ing. Even in the applied realm that is only 1 of many applications that could be referred to. In my own research, the primary motivations are that such data is required for river design for a wide variety of purposes including river rehabilitation and enhancement and also because it informs fluvial ecohydraulics. In light of systemic global ecological collapse, these are more important to society than flood forecasting, in my professional opinion. At a minimum, I think the authors should identify a few more reasons why knowing topographic variability matters and add a citation for each. Also, of course, geomorphologists want to understand it in its own right as a basic scientific question that requires no justification, and of course it is also the case that this variability controls fluvial processes, so the lack of knowledge about it means that we really know little about processes; less than I think most people realize.

P. 2, lines 3-8. While this is generally true, the authors seem to be unaware that my lab group has already published theory and code that is the first to procedurally generate river terrains exactly to specification from the equations and parameters, and this methodology does include sub-reach-scale variability that can go to as high of a frequency as one wants to make it, so quite small scale. There is always more to do, but I think this is relevant to the claim of this paragraph. I see that this paragraph has 4 citations for the first sentence alone, which seems like too many, so removing 1-2 of those could make way for citing this relevant work if the authors agree that what we published does in fact do what they say is an important thing to do, even if not perfectly, but still more than anyone else thus far. The journal citation is Brown, R. A., Pasternack, G. B., Wallender, W. W. 2014. Synthetic river valleys: creating prescribed topography for form-process inquiry and river rehabilitation design. Geomorphology 214: 40-55. 10.1016/j.geomorph.2014.02.025. The code is open-source and free to the world presently coded in R as "River Builder". The R package and user's manual can be downloaded from the CRAN website at https://cran.r-project.org/package=RiverBuilder. The code also includes the Perlin function that can create very small scale features, and that is a common method for generating landscape terrains in the video game and animation industries. In the future we hope to add the capability to parametrize the sub-reach-scale fluctuations in spatial series of detrended bed elevation and lateral topographic breaklines using wavelet parameterization.

The third paragraph of the introduction serves no required purpose and neither does Figure 1. Both can be deleted with no loss of understanding. Yes, rivers come in different types, but the main thing readers need to know is that this is a study of riffle-pool reaches and that the method can apply to other reaches; these ideas can be promoted without any of this paragraph, as is indicated by the first sentence of the very next paragraph just fine.

p.2, lines 15-16. The objective of what? The writing is unclear here. I disagree that the main purpose of quantitative analysis of channel topography is just to get pool spacing. In support of our River Builder software, one normally wants to analyze many aspects of reach-scale topographic variability so that they can all be parameterized and used to make realistic synthetic rivers. Other important variables would be parameterizations of thalweg planform curvature, base flow and bankfull channel width undulations, floodplain width undulations, and then how all of these are phased relative to each other (in time series that's "coherence" and "cross-phase"). Thus, pool spacing is certainly one useful data output, but not alone or necessarily most important. I also note that tat the authors never use their reach site results to present any conclusions about the science of pool spacing, so if it is sop important than its value should be evident in how the results are used to advance science.

p. 3, lines 1-4. No need to define wbf twice. Remove one of them.

p. 3, lines 7-17. A major problem with the historic work cited here that its all pre-2001and how it is presented is that the authors are not addressing the equal importance of channel width undulation to channel depth undulation. Richards in the 1970s understood it and wrote about the importance of width. However, because people didn't tend to make width profiles down rivers, the focus wrongly got limited to depth

undulation in the literature of the late 20th century. Of course, authors studying velocity reversal concepts did start to understand this problem pretty well by 1990. With modern high resolution DEMs since 2000, that problem is over and now we are in the era of looking at how depth and width co-vary to control pool and riffle topography and morphodynamics vis-a-vis the "flow convergence routing" mechanism explained by MacWilliams et al (WRR, 2006) and explored further by Prof. Jose Rodriguez in recent WRR papers as well by my lab group in several articles (Sawyer et al., Geomorph., 2010; Brown et al., Env. Man., 2015; Strom et al, Hyd. Proc., 2016; etc). My lab group has published a series of papers on the importance of linked depth and width undulations that has culminated in a new sub-reach scale channel unit classification relevant to this paragraph and this study. See these two articles, the rest leading up to these are cited in them: -Pasternack, G. B., Baig, D., Webber, M., Brown, R. 2018. Hierarchically nested river landform sequences. Part 1: Theory. Earth Surface Processes and Landforms. DOI: 10.1002/esp.4411. -Pasternack, G. B., Baig, D., Webber, M., Brown, R. 2018. Hierarchically nested river landform sequences. Part 2: Bankfull channel morphodynamics governed by valley nesting structure. Earth Surface Processes and Landforms. DOI: 10.1002/esp.4410.

p. 3, lines 20-26. Yes, I agree with all of this, though I don't think wavelet analysis cannot be called "new" as it has been published in geo/hydro journals for decades now; what's new is high quality topo data to apply it to, though that is present intros study. I'm surprised by the citations the authors offer here, as they are not very relevant compared to other options, such as (most importantly) Gangodagamage et al. (Geomorph., 2007) but also Lashermes et al. (WRR, 2007) and McKean et al. (Rem. Sens., 2009). One can use spatially evolutive Fourier analysis and autocorrelation analysis or, if one limits the analysis to a single reach, regular Fourier analysis where the average parameterizations are reasonable. One might even argue that the locations where the Wavelet analysis indicates a change in parameters could be a reach break. Certainly wavelet analysis is a very good way to go for this to objectively delineate reach breaks, but preferably with a multivariate strategy using both depth and width variables. A good comparison would be to look at the riffle-pool quasi periodicity analyses of Brown, R. A., Pasternack, G. B. 2017. Bed and width oscillations form coherent patterns in a partially confined, regulated gravel–cobble-bedded river adjusting to anthropogenic disturbances, Earth Surface Dynamics, 5, 1-20, doi:10.5194/esurf-5-1-2017.

p.5, lines 19-20. This explanation is incorrect on two levels. First, energy gradient is more than just water surface slope, because energy also accounts for the velocity head that is not in that term. Often velocity isn't changing over long distances or is assumed to not change, but along a riffle crest and in the transition to a pool it definitely changes quite a bit, so strictly speaking one has to account for that. Second, the energy gradient is stage dependent, because the steepest gradient is always associated with the vicinity of the smallest cross-sectional area, all other things being equal. At low discharge the way the authors describe it is true, because at low discharge riffles have the smallest XS area. However, once the discharge exceeds the value for the minimum cross-sectional area of the reach to be elsewhere, then it is not at the riffle any more. At some high flow it will become at the pool location, and of course this is the main reason why pools scour and riffles aggrade to maintain relief in alluvial channels, all other things being equal (especially substrate). This stage dependence is a key issue to account for in any scheme to evaluate where riffles and pools are located and it its why considering only depth and ignoring width has always been a mistake by the river science community. Now that we have width data commensurate to depth data, we can move on to the proper treatment of the problem considering their linked co-variance.

p. 5, lines 22-37. All of these methods retain the limiting viewpoint that they put a primacy on riffles and pools, either ignoring other morphological units (MUs) or treating them as irrelevant. Thankfully, 2D and 3D hydrodynamic modeling ends that mistake and enables objective mapping of all MUs with decision-tree analysis. This approach was explained by Wyrick et al. (Geomorph, 2014) and then applied in Wyrick and Pasternack (Geomorph., 2014) to not only show the greater diversity of MUs beyond riffles and pools, but also to compute simple metrics like pool spacing. Thus, Wyrick and

Pasternack (Geomorph.,2014) presented a novel methodology to extract pool spacing from 2D hydrodynamic model outputs of MUs using GIS tools. That is very relevant to this literature review, because it shows recent progress in automated extraction of this metric. The authors are arguing that their methods are more automated and better than pre-existing methods, but they have not actually considered more recent automated methods. Meanwhile, the sentences about the outstanding work by Almeida and Rodriguez as well as Parker goes off topic from pool spacing to get into the separate topic of riffle-pool morphodynamics, of which there is a very long and illustrious literature not addressed. Best to cut those at this location and stay focused on the directly relevant literature about pool spacing that is the focus of this study. They may be relevant if the revised manuscript ever addresses processes explicitly.

p.6., lines 5-27. Very good literature review and written well, just not accounting for many recent studies since 2001.

p. 7, line 6. The sentence about having surveyed "many" cross-sections is poorly constructed and, in my view, not accurate. Terms like "many" are relative, so it could be that for one person any arbitrarily small number of cross-sections would still seem like many; that makes it hard to argue the point. However, the key metric here is that one cannot analyze for topographically significant spatial frequencies at resolutions smaller than the minimum XS spacing, and that's already quite conservative. For that reason, my lab group uses vastly denser cross-sectional spacing than that used here. For example, in Pasternack et al. (ESPL, 2018b) we used a spacing of 3% of bankfull width. That's "many". For another group, Legleiter (Geomorph., 2014b) spaced a XS every quarter channel width. In contrast, in this study, an analysis of Table 1 finds that cross-sections are spaced between 0.46 to 2.9 times bankfull width, with two reaches not even having 1 XS every bankfull width. These numbers of cross-sections are more like the amount used in a conventional reach survey to obtain reach-average depth and width metrics, not to identify the underlying nature of variability. I think if the authors refer to previously cited articles above about spatial series analysis of rivers topography plus Legleiter, they'll get a better sense of what is needed to get at the detailed patterns of fluvial topo spatial series at the su-reach scale. This issue doesn't invalidate the study, but just recommends to back off the "many" and get to saying "a normal number of cross-sections typical for a 1D hydraulic modeling study" or something like that. Also, these cited works could be referred to in the discussion section to help compare and contrast undulation metrics from different studies, including when undulations may not have high enough amplitude to become a "riffle" or "pool" but are still big enough to make a difference for the intermediate morphological units that are mentioned but not investigated in this study.

p. 7, line 6. I think a bigger questionmark for the technical soundness has to do with the mindful decision to not have all cross-sections regularly spaced, but to place them primarily at hydraulic controls and morphological breaks. The authors then interpolate to get a grid, but the source data is not uniform. I fully understand why that would be done for a 1D hydraulic modeling study and given perhaps limited resources and no lidar data, but there is no question whatsoever that biased (aka mindful) XS placement impairs and calls into question spatial series analysis as far as objective identification of parameters. By placing the XS where the authors think important hydraulic and morphological things are happening, then necessarily the wavelet analysis and any other method is also forced to bias results toward the same outcome of where significant things are happening. On the other hand, when I put an XS every 3% of bankfull width along the series, then there is no chance anything will be missed and the algorithm can decide for itself what the frequencies, amplitudes, and phases (and other parameters) are for that reach. Equal spacing of XS is the best approach for unbiased results. I think there are some things that can still be analyzed with a small number of mindfully selected XS positions, but I would never take this approach. I do understand the lack of availability of lidar and other remote sensing data to facilitate high-resolution mapping though, but then one has to be thoughtful about what one can reasonably achieve. I think the way forward would be for the authors to explain their viewpoint on why they have a sufficient number of XS for the goals of their study in comparison to the highest

density used by the references cited above.

p. 8, section 4, line 1. "Hydrological" should be "hydraulic". I believe. These are not interchangeable. Hydrologic would be rainfall-runoff and water balance related, could be purely discharge but discharge alone does not identify riffles vs pools. Hydraulic means on the basis of the depths, velocities, and other flow kinematics.

p. 8, lines 8-16. I am confused by the writing. On line 8 it says hydraulic data were "surveyed" at 3 discharges. Please clarify that the data were measured in the field and then it is necessary to also describe how the data were measured. There are many different methods possible and one cannot undertake analyses of data without stating how it was collected. Moving on from there, if the data was actually measured, then I have absolutely no idea why the authors mention a method involving 1D hydraulic modeling of the sites. Given field observed cross-sections and hydraulic data, once could use a pure XS analyzer like the old, free software WinXSPro and many other GUIs to extract geometric variables like hydraulic radius with no numerical modeling. If the derivative variables like Rh and Fr are not based on field data, but instead are coming from a 1D hydraulic model, then it opens up a whole can of worms regarding the accuracy of the model outputs, which then necessitates an explanation of model calibration and validation performance. All of this is written unclearly and needs to be revised to explain to readers what is going on. This has profound consequences for evaluating the study.

Section 4. In the previous section it was stated that hydraulic variables were "surveyed" at 2 low discharges and 1 near bankful discharge. As the relative magnitudes of the variables between rifles and pools are stage dependent, it matters which flow was used to for the analysis in this section. The authors should state that. If somehow all three discharges were used, clarify how. In fact, at-a-station hydraulic geometry is an important tool for identifying riffles and pools more holistically considering the totality of the bankfull channel, so it is too bad few people take note of that and apply it fo this purpose.

p. 8, section 4, lines 4-5. Most people use PCA for challenging multivariate problems with complex interrelationships that are unknown and thus this is the first way to get a sense of how variables interrelate. That does not characterize the situation for riffle-poole geometry and open channel hydraulics. A wiser strategy here could be to use Buckingham Pi theorem dimensional analysis to create the variables of interest. Also, one can easily reason out that really the variables that matter are those that control or respond directly to morphodynamic processes, such as flow convergence routing or meander migration. That can then guide wise variable selection that is process based. Returning to this list of variables, several of these variables are highly correlated or define each other, so it does not make sense to throw them all into one multivariate analysis as if it is a mystery. For example, bed elevation, max depth, and hydraulic radius are all highly correlated and redundant. Meanwhile, A and P define Rh, so those 3 are also highly correlated. Similarly, Fr is defined by y and u, so the same situation arises. This “throw everything into the soup” strategy of multivariate analysis is not wise and possibly not technically sound, but the authors can review the PCA assumptions and limitations to evaluate that- not worth my time to re-study up on PCA. Even if it is technically ok, it still doesn’t make any sense as a strategy as if we do not already know how these variables relate to each other- we do know exactly how they relate.

p. 8, section 4, line 8. The topic of detrending is a huge issue that requires a bit of unpacking in the writing here, because the outcome of riffle-pool delineation can be largely depending on this very choice based on my own sensitivity analysis of this situation using different detrending methods. Earlier in the manuscripts the authors wisely commented about all the different way different authors measure and analyze pool spacing data (e.g., p.6, line 22). Well, the same challenge arises with detrending. There is no universally right or wrong way given the diversity of purposes for detrending, but each option has consequences for the scientific outcome for a specific purpose, especially for identifying the magnitude and length of residual highs and lows in a bed profile. Without going into all the options, what I request is that the authors state what

type of detrending they did. If linear, then was it one line per site (presumably no reach breaks within a site, but there could be) and was care taken to insure that the line began and ended at the same relative elevation to avoid biasing the slope, which is a significant problem

p.9. I am just not understanding why anything in Figure 4 and the associated results text is actually new results or anything other than trivial findings. By definition of variables, A, Rh, and y are positively correlated, while Fr is going to be negatively correlated to y and positively correlated to u. Also, Z has to be negatively correlated to A, Rh, and y. This is all be definition. PCA is not required to know this. Further, I do not agree that the PCA is adding any fundamentally new or useful information for riffle-pool delineation compared to wisely selecting the few independent variables underlying the physics-based analytical relations, especially bed elevation, width, and possibly slope, as together these three control relative velocity between riffle and pool units for a fixed discharge. If the channels are meandering, then thalweg planform curvature would be important, too, as it is well known in the physics to control meander migration. In fact, it is unclear and technically unsound to exclude metrics of channel width from this analysis, as width is the underlying independent variable influencing all the other variables in the list except for detrended bed elevation and depth (which of course are the same thing just inverted and with different vertical datums). The authors need to set up this methodology better to justify why it is necessary and better than what I am proposing as an easier, more process-based approach or else I do not see how this PCA analysis is meritorious.

p.9, lines 3-4. The claim that each descriptor adds additional information about the bedforms is easy to show as not true. Rh is defined by A and P, so how is Rh fundamentally new and additional as opposed to using a combination of A and P, unless one defines the mathematical operation of division as adding new content, which it does not. This continues the theme of my last few comments. The authors are applying blind statistical methods to what is a pure analytical problem with 100% defined and known elements. There is no additional information beyond the independent variables and the math operators to combine them into A, Rh, and Fr.

p.9, lines 7-8. These claims apply only to low discharges, due to the flow-dependent nature of riffle-pool hydraulics. How they develop as discharge increases depends on the shape of the cross-sections (especially depth vs width “geomorphic covariance”, per flow convergence routing theory.

p.9, lines 10-12. This single long sentence attempting to explain a sequence of mathematical steps applied to some data is opaque to me as a reader, as is plot (a) in Figure 5. This should be written out more thoroughly and clearly in steps. For example, presumably the smoothed data is each XS spatial series, but then what constitutes the “sampling” that is “homogenized”? I neither understand the samples nor what homogenization is and why it is needed. Is homogenization the same or different from normalization in this study? If so, why call it two different things that creates reader confusion, but if not then what is it? Sometimes normalization means the strict application of the function that makes the data fit the normal probability distribution while more often it just means to divide variable by another.

p. 10, first line. Why is this line bold?

p. 10, equation (5). This equation is an all-or-nothing type approach where every location is either classified as riffle or pool for an individual descriptor. This is in contrast to the aforementioned BDT approach that uses a standard deviation tolerance. Also, the method of Pasternack et al. (ESPL 2018a,b) uses a standard deviation tolerance. It would be useful to explain why no tolerance was applied.

p.10, line 10. From what I gather considering the equations and the potential values of I, the concept here is that for something to be defined as a riffle or pool versus an intermediate MU type, all three descriptor variables must agree and yield the same heavyside function value of 0 (pool) or 1 (riffle). Conceptually, the authors are substituting a cross-check among 3 variables as the countermeasure to cope with uncertainty

in place of tolerance within each variable as the countermeasure for uncertainty. I think putting the concept of the method in words like this would help readers understand the strategy and purpose of the math and procedure that is described. However, looking beyond that, one can ask if this actually works? In other words, is there a resiliency against uncertainty gained by using multiple variables and the specific ones chosen? The authors should address why they think this is so, because this is the kernel of new idea they are proposing but have not actually written out. I have to agree that using more than 1 INDEPENDENT variables would help serve as a check against uncertainty, so that is good idea, but (a) the variables chosen are not independent (both Fr and Rh depend on detrended bed elevation, which is a surrogate for the inverse depth and depth goes into both Fr and Rh) and (b) one can choose to use both a tolerance per BDT and multiple variables per this study. That would yield the best outcome. In Pasternack et al. (ESPL, 2018a,b), we do use both strategies, but for our choice of variables we limit our analysis to only detrended bed elevation and width, as these are the process-based controls on flow convergence routing, they are independent, and they underlie the derivative variables like Fr and Rh. However, we do not use slope, which independently controls velocity and Fr, and we make that choose for a specific process-based reason, but we do exclude it. We also do not look at thalweg planform curvature in those articles, though we have internally thus far. One could reasonably choose to include both slope and thalweg planform curvature. One could also choose to include grain size metrics, as I’m sure prof. Jose Rodriguez would be very insistent on given the importance of that variable to determining relative erosion and deposition on riffle sand pools. Unfortunately, it is incredibly difficult to obtain high-resolution spatial series of substrate grain size as of yet. In any case, I see both positive and negative to what is being done. At a minimum, the authors can explain the general idea in words as I have done, but then also some defense is needed if the authors stand by the decision of variables chosen, because I see the choice as technically unsound given that they are defining each other as explained.

p.11, line 2. I see that p.6 line 10 defined lambda-star as “dimensionless pool spacing”, yet here that variable has dimensions of m? Something is wrong.

p.12, lines 1-2. While most people only apply Fourier analysis to stationary series, the method is not in fact limited as thus, because it can be applied using the "evolutive" methodology to capture non stationary dynamics very similar to what one gets from wavelets. One can reasonably argue that wavelets are superior for non stationary data and because one can apply different wave forms, but to say that Fourier analysis cannot do non stationary analysis is wrong. Many applications of evolutive Fourier analysis exist, but for hydrological data see for example, Pasternack, G. B. and Hinnov, L. A. 2003. Hydro meteorological controls on water level in a vegetated Chesapeake Bay tidal freshwater delta. Estuarine, Coastal, and Shelf Science 58:2:373-393.

Section 5.1. I think there is too much redundancy between what was written about wavelets in section 1 (p. 3, lines 20-26) and this section. The introduction can more simply introduce the idea of it and state the scientific questions and hypotheses associated with using it, but then leave the literature review here, so there is only one literature review. My earlier comments about the literature of applying wavelets to geo/hydro data also apply to this section.

p.15, line 14. This sentence makes a key determination that flies against the same kind of decision-making applied to the index method of section 4. Specifically, the determination of riffles and pools is going to rely entirely on bed elevation. It seems odd that scientists who begin with the conjecture that multiple variables should be used to determine riffles and pools would now contradict themselves and only consider one variable. My view of it is that both decisions are arbitrary, as (a) the former was based on a questionably PCA analysis lacking a mechanistic basis and choosing interdependent variables rather than the proper independent ones and (b) the latter is likely based on the amount of work it takes to apply the wavelet methodology and so its application is being limited to only one variables and to only 1 reach instead of all the reaches. I am making my own guess with (b), but the authors provide no justification for limiting the analysis to only 1 reach after introducing so many reaches. Similarly why not do

all three variables the authors deem important. A quick check of the scientific literature confirms that multivariate wavelet analysis exists and is available for use. And then there is the issue of how the variables couple to affect riffle and pool occurrence, structure, and resultant processes. The decision-making here is too opaque and needs explanation per these issues. I expect the decisions cannot be justified, but the authors deserve a chance to try.

p. 15, line 15. Why choose the Orgeval reach, when it is not the longest or having the most XSs? I already deleted my table where I computed the XS density, so does this reach have the highest XS density? Otherwise, why? of course, why not analyze and compare all 6 reaches, as this is a scientific journal article and there could be interesting results in comparing the different reaches? The method itself of applying wavelet analysis to a spatial series is not so novel as to justify limiting to only 1 reach as a single case study.

Figure 8. This figure shows a fundamental problem with the wavelet methodology as the preferred tool for mapping riffle and pools as well as quantifying their spacings. Specifically, it cannot return results for some distance at the start and end of the spatial series. In the case shown, there is only results for the range of $\sim$ 81-241m out of 318 m. That leaves a whopping 50% of the reach unassessed. Wow. That's a lot of lost information. Of course, the longer the series and the more frequent the XS sampling, the less loss, but there will always be a loss. This makes the method less valuable than alternatives that retain the information.

p. 17, line 2. Again, why does lambda-star have units at all- it is supposed to be dimensionless.

p. 17, results header. Some authors like to blend methods and results in paired couplets working through a manuscript, and that is most appropriate when one couplet build son the results of another, but then one would not call section 6 here a results section, as many results have already been presented. If I was the associated editor for this manuscript, I would require the authors to separate the methods content from the results content and go with the traditional ordering of the scientific method, because there is no reason not to. One can state the methods from sections 4 and 5 in one unified methods section and then state the results in a unified results section. As the two sections do not build on each other, then one does not need to use the couplet approach. Then, one can have methods and results subsections for the inter comparison analyses. Finally, discussion should stand alone after all results are presented.

Section 6.1. Authors must clarify if the score technique is applied to the entire reach length or only the length for which results overlap. I think one must count the whole reach as it is a deficiency of the wavelet method that it leaves 50% of reach 6 unevaluated. Whatever the authors are doing, they should clarify that.

Section 6.2 This section now states that the comparison is limited to only 81<x<241 m. That's problematic because it's not a fair test of the actual utility of the wavelet method leaving half the reach unevaluated. This should be stated clearly. The comparison is still useful but it does have this huge caveat. A method that leaves half the reach unevaluated can never be better than one that assess the whole reach, if the goal is to characterize the whole reach.

Table 4. I do not understand. Previously it was stated twice that only 1 reach was assessed but now here are data comparing all six reaches. I think the writing of the manuscript should be improved to explain what is going on better. If all six reaches were in fact tested with wavelet analysis, then some comparison between reaches would be interesting for section 5.

Discussion section 6.3. These paragraph primarily consistent of more results not previously present, but there is a bit of discussion, too. Specifically, all the text in this section from page 19 line 18 to page 21 line 20 are purely results. In fact, p. 21 line 10 even says, "these results…" so the authors view these as results too. Really, there is no suitable discussion putting the results of this study into the larger context of methods

and results about riffle-pool ID-ing and quantifying their spacings. There should be such a discussion.

Section structure. I think there are problems with the way the manuscript's sections are structured. In general I can follow what the paper is trying to do, but the structure would be better following a traditional scientific method with all methods first then all results second, ands then all actual discussion last. By mixing them all up it is somewhat confusing and more importantly, impossible to tell what methods have answered what important scientific questions. For example, from the structure it is difficult to tell if this study is only a methodological comparison or also a scientific contribution presenting new results about pool spacings that can be compared with the results of other studies. It would be a shame to do all this work and have no contribution to the question of pool spacing in different river types. But getting back to my main concern here, the discussion, if present at all, iOS hidden in bits throughout the manuscript and would work better if isolated and thoroughly presented.

In conclusion, I have put many hours of work into thoroughly inspecting this manuscript to help the authors receive the best quality of feedback I can produce. That has resulted in a lengthy review with many issues raised, which can be demoralizing to authors, but the point of the effort tis to offer about as much discussion of a manuscript as anyone is every going to give about it and also to help the authors produce the best possible scientific article they can. I hope the authors can see what I am trying to do and I hope it will provide substantial value, though it will create more work.

Best regards

-Greg

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-381, 2018.