Reply to the Anonymous Referee #1

Dear Referee,

We thank you for your comments, which will help improve the clarity of the manuscript as well as the choice of the methods.

According to both reviews we decided to make very substantial changes to the paper. This work is a methodological study that introduces relatively new wavelet analysis tools in the field of geomorphic analysis (namely, Wavelet Ridge Extraction), in order to identify the pseudo-periodicity of alternating morphological units from a general point of view (and not only pool-riffle morphology). We did initially introduce an index method as a benchmark, but this index was poorly designed due to a lack of physical basis for the choice of the variables. We also neglected some relevant literature on the identification of the morphological units using DEMs, which could be used as benchmark methods in this paper.

For that, we suggest changing the title of this paper to "***Automatic identification of alternating morphological units in river channel using wavelet analysis and ridge extraction***". This will be more general and focuses on the method and not on the pool-riffle morphology.

We have presented two methods in this article. The first one is the wavelet method which represents alternating morphological units (pools and riffles) as pseudo-periodic signals with a continuous wavenumber function K(x). The other one is the index method which is a benchmark method that gives a discrete identification of the morphological units.

With the suggestion of the second reviewer Prof. Gregory Pasternack, we will cut out the index method and replace it with an existing method "Mesohabitat Evaluation Model (MEM)" inspired from Hauer et al. (2009). For that, we will focus only on the wavelet analysis and ridge extraction in the univariate and the multivariate cases and compare its results with two benchmark methods: BDT (O'Neill and Abrahams, 1984) to the bed elevation data and MEM (Hauer et al., 2009) to three variables (velocity, hydraulic radius, and bed shear stress).

We will also minimize the use of modeled variables and apply the methods directly on field measurements (velocity and hydraulic radius variables at the lowest surveyed water level). We will use modeling results (Fluvia model) for bed shear stress only, as the energy slope cannot be determined in a sufficiently accurate manner with the measurements.

For the literature, we missed many recent studies and methods in relation to this work. So first we will add a table that summarizes examples of methods of identifying these morphologies and the variables chosen to do that. Second, we will change and add many recent works especially those working with meter-resolution digital elevation models (DEMs). Finally, we will clearly state the objectives of this study in the abstract and in the introduction.

Another important thing is that we propose a new structure of the paper:

I- Introduction:

First, we will state the scope of this study with adding more fields of its application. Second, we will introduce a literature review of metrics, variables used to identify and characterize the alternating morphological units. We will focus on two kinds of numerical criteria computed at reach scale:

- The distribution of spacings between morphological units (mean, mode, etc.),
- After computing the mean values of geometrical and flow properties (velocity, hydraulic radius, bed shear stress, etc.) in each class of morphological units (e.g. pools, riffles, runs, etc.) we will evaluate the covariance matrix of these parameters.

II- State of art methods for a quantitative assessment of morphological variability within a reach:

We will present some recent methods and works in the identification of these alternating morphological units (pool-riffle in our case) and state their objectives and limitations. We will start with the Bedform Differencing Technique (BDT, O'Neill and Abrahams, 1984), which is simple but uses bed elevation as the sole variable, and relies on a tolerance criterion on elevation differences. We will then review index methods like Mesohabitat Evaluation Model (MEM, Hauer et al., 2009) which classify each position in the reach into a given discrete morphological unit (pool, riffle, run, plane bed, etc.). These methods rely on expert judgement to define the thresholds that define parameter classes. Finally, geostatistical methods (e.g. Legleiter, 2014) give a continuous description of river channel properties in spatially stationary way, using longitudinal and transverse variography. For these reasons, we are searching for a method that gives a continuous description of geometrical and flow characteristics along the reach with a non-stationary description.

III- Study objectives

We will state that this work aims to introduce relatively new wavelet analysis tools in the field of geomorphic analysis, the Wavelet Ridge Extraction, in order to identify the pseudo-periodicity of alternating bedforms from a general point of view. In this study we will use a dataset that presents mainly pool-riffle morphologies, but the method can be applied to any morphology.

We will present the scheme of the paper which include a methodological section of the wavelet analysis and ridge extraction in the univariate and the multivariate cases, a section that presents the comparison method (with defining more explicitly the two benchmark methods: BDT and MEM index), a discussion section, and conclusions.

IV- Data set and study reaches:

We will present the six reaches, more explicit information about data collection and about the numerical modeling (Fluvia), and the data that will be used in this study.

V- Wavelet method

1) Wavelet analysis and ridge extraction:

We will present a general introduction about wavelets including some methods such as the Wavelet Transform Modulus Maximum (WTMM, Gangodagamage et al., 2007) and other studies using the wavelets in the geomorphological field (Lashermes et al., 2007; McKean et al., 2009). Procedures such as the WTMM (Muzy et al., 1993) consist in extracting components of the signal, but they are not specifically designed to identify pseudo-periodic components in a univariate, let alone in a multivariate case. For this reason, we introduce the procedure called Wavelet Ridge Extraction (Lilly and Olhede, 2009).

2) Univariate case

We will present the methodology of this method in the univariate case using one of the three variables (velocity, hydraulic radius, and bed shear stress).

3) Multivariate case

We will present the methodology of this method in the multivariate case using the three variables (velocity, hydraulic radius, and bed shear stress).

VI- Results

1) Comparison method:

We will define more precisely the two benchmark methods: BDT and MEM index, and their classification of the morphological units (pool-riffle). We will define also the metrics and their computing method.

2) Application and results

We will present results of all methods for the six reaches and apply the comparison.

VII- Discussion

We will discuss results (longitudinal spacing, number of morphological units, etc.) with literature and with the two benchmark methods

VIII- Conclusions

Kind regards,

The authors

Major comments:

The manuscript "Wavelet and index methods for the identification of pool–riffle sequences" by Mahdade et al. presents two novel methods for the identification of pools and riffles in natural streams. These methods also allow the assessment of the main geometrical features of pools and riffles. The manuscript states that appropriate geometric description of pools and riffles is pivotal for flood modelling. I think this statement is correct when modelling floods (and flash floods) at the local scale. Conversely, previous

studies have shown that simplified representations of river geometry can be a cost-effective solutions for flood modelling at the large (basin to continental scale). In fact, I believe that an accurate representation of river geometry is essential for the implementation of hydrodynamic models used for the investigation of local flow conditions and sediment transport. The scope of the paper could thus be extended to biological and environmental modelling (oxygen exchange, fish habitat, sediment transport) and not only limited to flood forecasting.

Response:

We agree with you that this statement is correct only at local/small scale, in which we can quantify geometric variability and especially alternating morphological units. In the new version of the paper, we will add examples of application of our study such as the design of a synthetic river topography which is implemented in river restoration (e.g., Wheaton et al., 2004a), habitat modeling, ecohydraulics (e.g., Pasternack and Brown, 2013), biological and environmental modeling (oxygen exchange, fish habitat …) and also that this variability controls fluvial processes as sediment transport, but not focusing only on flood forecasting.

The paper is interesting, sections 1 and 2 provide a comprehensive literature review; sections 4 and 5 provide a detailed explanation of the methodologies; the presentation and discussion of the results in section 6 is quite extensive. However, I think that a number of major modifications should be introduced before the publication of this study.

Firstly, I think that the research gap and the novelty of this study should be clearly stated. Why did the authors propose two novel methods for the identification of pools and riffles? What are the advantages of these two novel methodologies when compared to the existing ones? I believe that these aspects should be clearly stated in the manuscript.

Response:

The goal of the paper is to introduce a new method for the analysis of river morphology. The rationale behind the method is that the existence of alternating morphological units along a reach (such as pools-riffles sequences, or step-pool etc.) should translate as a *pseudo-periodicity* in geometric and flow variables. Hence, identifying these bedforms amounts to identifying a local wavenumber K(x) and phase $\phi(x)$ for each variable, a task that can be performed by wavelet analysis and especially Wavelet Ridge Extraction (Mallat, 1999; Lilly and Olhede, 2010), in a multivariate framework.

In the initial version of the paper, we were comparing this wavelet-based method with two benchmark methods: the BDT (O'Neill and Abrahams, 1984), and an index method that consists in affecting a different numerical value for each class of a given variable/degree of freedom, and then sum these individual index functions into a composite one.

The second reviewer Prof. Gregory Pasternack has raised major concerns not about the index method in itself, but on the choice of the variables/degrees of freedom. Initially we used the first three axes of a Principal Component Analysis as the degrees of freedom, a choice which has very little physical meaning. We will entirely change this choice and build the index using the same variables/degrees of freedom as

in the existing "Mesohabitat Evaluation Model" (MEM, Hauer et al., 2009), which uses velocity, hydraulic radius (or the closely related cross-sectional averaged depth), and bed shear stress. We will also use the same threshold values for classifying each variable.

However, we intend to keep the last benchmark, the BDT method, as it is in the current version of the paper.

Second, the results of the new methods are compared to the results of the BDT method. Is the BDT method used as benchmark or to validate the new methods? Is the BDT method considered more accurate than the new methods? If so, why? What are the advantages of using the two methods rather than using the BDT method? Would it be possible to validate the results of this study using field data?

Response:

In this study, we consider the BDT method as a benchmark method. We do not consider a specific method to be the "true" or "reference" one, we only apply several methods to have a general idea on the uncertainties in the identification of morphological units. That being said, there is a substantial difference between the BDT and index methods on one side, and the wavelet ridge extraction on the other side:

- BDT and index methods classify each position in the reach into a given category (pool, riffle, run, plane bed, etc.); hence, in 1D, we have access to a discrete values of bedform lengths L_i (i=1…N), and we can compute statistics of this discrete distribution such as ***mean***, ***mode***, ***n-th order moments***, etc.;
- In contrary, the wavelet ridge extraction provides a continuous description of bedform spacing along the reach, through a continuous wavenumber function K(x). In turn, we can compute the statistics (again, mean, mode, n-th order moments, etc.) of this function in order to compare them with the values obtained in a discrete method.

Moreover, index methods use expert judgement in order to specify threshold values for each variable/degree of freedom. Since wavelet analysis is continuous in nature, such thresholds are not needed in our method.

Third, the computation of the index method relies on the results of the numerical model. Have the authors considered the impact of the uncertainties in the results of the numerical model on the results of the index method?

Response:

The use of model outputs is indeed a questionable choice that may add a lot of uncertainty in the results. The purpose of the numerical model used in the previous studies by Navratil et al. (2006) was simply to generate water surface profiles for discharge values other than the surveyed ones (i.e., interpolate/extrapolate the rating curves). In our revised paper, we will solely rely on measurements at the lowest surveyed discharge. However, since the calibration of the FLUVIA model on the reaches provides estimates of Strickler roughness coefficient K_s, we will use these K_s in order to compute the

third degree of freedom, bed shear stress $\tau_b(x)$, along the reach: even if partly relies on calibration, it seems a more robust way of computing τ_b than through the finite differentiation of the total head function $U^2/2g + z_{surface}$ between adjacent cross-sections to get the energy slope J.

Furthermore, I suggest discussing the transferability of the new methods to other reaches. In other words, how easy would be to implement the proposed methodologies to other study areas? Are the data and algorithms required easy to collect and implement? Can other researchers implement the proposed methodologies?

Response:

As stated previously, the wavelet methods intends to be quite general and can be applied in any morphology that presents alternating bedforms (pool-riffle, step-pool, etc). The code comes in the form of a small number of Matlab functions, and the data has to be provided as values of flow variables sampled at successive locations along the reach. The choice of the set of variables/degrees of freedom is up to the user, in our case we chose the triplet [U(x) , Rh(x), $\tau_b(x)$] but we could pick other variables, and add planform variables as well.

Moreover, I think the manuscript should clearly state which methods are recommended. A more explicit presentation of the conclusions of this study would highlight its scientific and practical relevance.

Regarding the structure of the paper, I would like to recommend two modifications: - Section 2 lists a large number of studies and it is a bit hard to follow. More specifically, I think it is difficult to appreciate the differences between the large number of criteria listed in this section. The authors might consider adding a table to summarise their literature review.

I hope the authors will find my questions and recommendations useful to improve their manuscript.

Response:

As said before, the structure of the paper will be changed by splitting the results and discussion and adding a conclusions part, the later one will specify the added value of the wavelet method according to the comparison in the discussion section. In fact, we will compare the metrics computing (mean, mode, n-th order moments, etc of the distribution) using the three methods.

Minor comments:

I listed below a number of minor recommendations.

Comment of the reviewer	**Response of the authors**
Page 1, lines 7-8: the sentence "To better take this high-frequency variability in bedforms into account in hydraulic models" is a bit convolute. The authors might consider improving the structure of this sentence.	We will replace it with: "To include/consider this high-frequency variability of the geometry in the hydraulic models"
Page 1, abstract: the abstract should clearly state	We will change the abstract by including that this

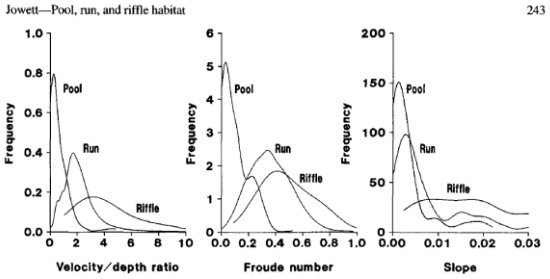
the research gap, the aim, and the novelty of the study.	work is a methodological study that introduces relatively new wavelet analysis tools in the field of geomorphic analysis (namely, Wavelet Ridge Extraction), in order to identify the pseudo-periodicity of alternating morphological units from a general point of view (and not only pool-riffle morphology). We will state clearly the aim of this paper which is for example extracting some quantitative properties of these alternating bedforms such as the mean and the mode of their longitudinal spacings, with a "continuous" vision of the topography instead of a discrete classification.
Page 1, line 9: the abstract mentions "several methods", however, only three (two novel methods and one benchmarking method) are listed explicitly.	As stated above, we will clarify the presentation: we introduce one new method (wavelet ridge extraction) and we compare the results with two existing methods (BDT and MEM index method).
Page 1, lines 12-13: the authors might consider avoiding the repetition of the word "compared".	Corrected
Page 2, lines 14-15: I am not sure whether this is the final format of the paper, however, I suggest positioning each figure after a full stop (Figure 1 is currently positioned in the middle of a sentence).	This is not the final format of the paper. We will change that in the revised version.
Page 2, line 15: please correct "dimensionless reach wavelength".	Corrected
Page 3, lines 4-5: this sentence is a bit hard to understand. Do the authors mean that the overarching purpose of their study is to provide a methodology for the prediction / modelling / assessment of cross sections variability?	We will cut out this sentence and change from line 14 to 16 in page 2 with: "In this study, we focus mainly on alluvial pool-riffle sequences, even though the method presented here could be used to analyze any alternate morphological units. The objective is to provide a continuous description of geometric and flow patterns along a reach, a description that could be subsequently used to create a synthetic river as in the River Builder (Brown et al., 2014). To do that we calculate the dimensionless reach wavelength, which is the distance …".
Page 3, line 8: words such as "methods" or "techniques" might be more appropriate than "studies".	Corrected
Page 3, line 11: could please the authors clarify the meaning of "descriptions of the water surface characteristics"? Is "water surface slope" (mentioned in Line 8) included in this latter category?	Descriptions of the water surface characteristics means a method that describe pools and riffles from the combination of all characteristics of the water surface (water surface elevation, water surface slope, etc.) and which include effectively the slope as Mosely (1982) mentioned in his paper. We also corrected the reference. *"Mosley, M. P. (1982). Procedure for characterising*

	river channels, Water Soil Misc." Instead of *"Mosley, M. P.: Analysis of the effect of changing discharge on channel morphology and instream uses in a braided river, Ohau River, New Zealand, Water resources research, 18, 800–812, 1982."*
Page 3, line 14: I suggest clarifying the sentence "because it changes less with discharge".	It means that this morphological definition of pool-riffle sequences doesn't depend on discharge.
Page 3, line 20: please rephrase "goes with the notion".	We will change it with "involves the use"
Page 3, line 22: please rephrase "allows one to extract".	We will change it with "extract"
Page 3, line 30: please rephrase "using a threshold on a criterion index."	We decided to cut out this method but we will use instead of it a method that uses thresholds on 3 variables. We explained that before.
Page 4, Figure 2(A): I believe that this figure is not mentioned in the text.	Corrected, we will mention it in the page 3, line 26
Page 4, lines 3-4: I think this sentence should be moved to the section 6.2 as it motivates the choice of the benchmarking method.	We will move this sentence to the comparison methods section and modify it according the new structure of paper.
Page 5, line 7: "the areal difference asymmetry index by Knighton" has not been mentioned before, the authors might consider adding more context to this statement.	It was felt that there is no need to define this method because it's just an example of methods existing in the literature. However, we will add it in the table that summarizes all the previous methods and techniques.
Page 5, line 32: the manuscript states: "a common geomorphological and hydrological" methods, I suggest specifying these methods.	We will change the entire sentence because it wasn't clear enough (from line 32 to line 34): "Krueger and Frothingham (2007) identified pools and riffles in fifteen reaches of Ransom Creek, Clarence, New York with methods used in two different disciplines, geomorphology (BDT) and hydrology (Froude Number method), and compared their identification agreement."
Page 6, line 8: was the channel width/channel bankfull width used to compute dimensionless values of wavelength? I think this sentence is not clear.	Yes, it's not clear. Here we are talking about the dimensionless pool spacing, in which there are researchers who use the definition $\boldsymbol{\lambda^* = \frac{\lambda}{W}}$ (mean channel width) while others use $\boldsymbol{\lambda^* = \frac{\lambda}{W_{bf}}}$ (mean bankfull channel width). We will change it in the revised version.
Page 6, line 9: what do the authors mean with "certainty"?	"certainty of these ratios" means their efficiencies to give more consistent results, so we will change it to "efficiency"
Page 6, line 14: I suggest avoiding colloquial expressions such as "a great deal".	We changed it to: "Some researches have investigated"
Page 6, line 32: I suggest rephrasing this sentence and avoid the use of "we".	We will change all the sentence to: "These reaches contains mainly pool-riffle morphologies, they

	have slopes …"
Page 7, line 5: I believe that information on slope has been previously provided in page 6, line 32. Could please the authors explain the added value of this sentence?	This line has been added to define the thalweg elevation and how it can be estimated. That's why we will delete it and add this information in the line 32 p6: "they have slopes less than 0.015 (estimated from the thalweg elevation which is the lowest point in the section) …"
Page 8, line 11: please clarify the sentence "It is based on interpolations rather than extrapolations".	As said before, the role of the 1D hydraulic model (Fluvia) was simply to generate water surface profiles for discharge values other than the surveyed ones (i.e., interpolate the rating curves between values of surveyed discharges, and extrapolate slightly above highest surveyed discharge). In our revised paper, we will solely rely on measurements at the lowest surveyed discharge and use the model to provide estimates of Strickler roughness coefficient K_s, we will use these K_s in order to compute the third degree of freedom, bed shear stress $\tau_b(x)$, along the reach. So this part form line 10 to line 14 will be modified by an explicit description of the model and the data set.
Page 8, line 13: "visually": do the authors mean that they performed a manual calibration of the hydraulic model?	It is a typo, we checked the calibration visually, but we adjusted it with a minimization function.
Page 8, line 14: please remove the second full stop.	Corrected.
Page 8, line 14: "multi-section flows": do the authors mean that the numerical model is used to predict a number of quantities (e.g. the elevation of the water surface, wetted perimeter, wetted surface,: : :) at a number of cross sections?	The use of the numerical model (Fluvia) will be simply to generate calibrated estimates of Strickler roughness coefficient K_s that we will use to compute the bed shear stress $\tau_b(x)$ along the reach. For the other cross-section variables, we will use only measurements at the lowest surveyed discharge.
Page 8, line 3: why is the minimum discharge used for the implementation of the method?	We chose the minimum discharge (low flow) in the development of the method because it is the discharge through which we can visualize the variability of the bathymetry (alternating morphological units).
Page 8, line 6: does "it" stand for "relevant information"? The authors might consider editing the structure of this sentence.	This section will be removed from this paper as suggested by the second reviewer Prof. Gregory Pasternack and we will replace it by a benchmark method in the comparison methods part.
Page 8, line 8: I believe that "the trend" has not been explained before. I suggest clarifying this sentence. What does "detrended variables" mean? How are these variables computed?	The only detrended variable was bed elevation: we computed a series of bed elevation anomalies $\varepsilon_z(x)$ such that: $z_{bed}(x) = -S_{bed}\, x + b + \varepsilon_z(x)$, where S_{bed} is the mean slope of the reach and $\varepsilon_z(x)$ has zero mean. This part is not necessary anymore.
Page 8, line 13: "contain the most explained	The PCA analysis will be completely removed so

variances" do the authors mean that those directions can explain the variability of the data? I suggest clarifying this sentence.	this discussion is not relevant anymore.
Page 8, lines 18-20: does these results confirm/contradict previous studies?	The PCA analysis will be completely removed so this discussion is not relevant anymore.
Page 9, figure 4: could please the authors explain the meaning of Dimension 1: : :9?	The PCA analysis will be completely removed so this discussion is not relevant anymore.
Page 9, lines 5-6: I think this sentence is unclear. What is the relationship between bed elevation and hydraulic radius? The statement seems to be contradictory. Moreover, I was wondering whether any correlation between bed elevation and hydraulic radius is meaningful. Bed elevation is a geometric characteristic at the point scale. The hydraulic radius depends on discharge, river bed slope, cross section area.	Here we are not talking about the physical meaning of these variables but their variability. The hydraulic radius is the cross-sectional area divided by the wetted perimeter, so the hydraulic radius, the cross-sectional area, and the depth are positively correlated, while the water surface elevation is the depth plus the bed elevation, so the depth and the bed elevation are negatively correlated. So the bed elevation and the hydraulic radius are negatively correlated. It's just trivial findings. For that we will choose in the revised paper variables that are not dependent.
Page 9, lines 6-8: the explanation based on hydraulic radius and Froude number is reasonable and (almost) intuitive. I suggest to clarify the added value of this finding compared to the existing literature.	There is no added value of this finding, we were wrong about the justification of our choice of variables. We will change all this section as we mentioned it before.
Page 9, line 9: I suggest clarifying the importance of bed elevation.	Historically, bed elevation has been seen as the most relevant variable in order to characterize geometric and flow variability. Since water surface elevation cannot change in space as fast as bed elevation, local bed elevation (and slope) is an important driver of depth and velocity variations along the reach. However, width variations have been found to be important as well, so a multivariate approach must clearly be favored.
Page 9, line 10: what do the authors mean with "we smooth" the data?	The formulation was wrong; in fact the processing mentioned in this sentence was only applied to bed elevation: since the trend of bed elevation is not necessarily linear, we performed a more general removal of very low frequency components (wavelength larger than 7 times the mean bankfull width) before applying thresholds. Since we will not use bed elevation anymore in the index method (MEM), this processing is no longer relevant (and it was not a smoothing anyway).
Page 12, lines 11-13: I suggest improving the readability of this sentence.	We suggest another clear sentence: "They are functions used in representing data by processing it at different scales or resolutions. If we look at a signal with a large –window-, we would notice gross features. Similarly, if we look at a signal with

	a small –window-, we would notice small features. The result in wavelet analysis is to see both of them (Graps, 1995)."
Page 12, lines 16-18: please improve the structure of this sentence: "have been interested: : :but working", both the verbs have the same subject.	Instead of "but working" we will put: "but they have been working"
Page 12, line 18: "analysis" is repeated.	Corrected.
Page 14, line 12: I suggest replacing "evacuate" with something more appropriate (an option could be "remove").	Corrected.
Page 15, line 3: please clarify "It also represents" (what does "it" stand for?)	"it" stands for "the curve that continuously crosses the domain" and also "K(x)". We will replace it by "This curve K(x) also represents …"
Page 15, line 11: could please the authors better explain why this correction is applied?	Equation (21) actually gives the amplitude of the pseudo-periodic signal through inverse wavelet transform. In this reverse transformation we need to multiply by $\sqrt{s} = \sqrt{\frac{1}{\alpha K(x)}}$ where α is the Fourier factor (Torrence and Compo, 1998), since we multiply by $\sqrt{\frac{1}{s}} = \sqrt{\alpha K(x)}$ in the direct transformation (Equation 14).
Page 15, line 15: please correct the structure of this sentence: "we limit the study only with univariate analysis". Moreover, could please the authors justify this choice?	As we said before, we will focus in the revised version of this paper on both the univariate and the multivariate analysis and we will compare their results with the BDT (O'Neill and Abrahams, 1984) and the MEM (Hauer et al., 2009).
Page 15, line 26: please clarify the meaning of "multivariate case".	The multivariate case is the extension of the univariate to a set of N real-valued signals; it is described in Lilly and Olhede (2009). We will describe this case and develop its transformations in our revised version.
Page 17, Table 3: Table 3 and Table 2 show the results of the two methods for the same river reach. The authors might consider displaying these tables in the same page in order to allow a straightforward comparison of the results.	We will display these tables to the results section.
Page 18, lines 2-4: I think that this sentence is unclear.	We will replace it with a clear sentence according to the new results that we will have.
Page 19, line 3: I believe that these results demonstrate a good level of agreement between the two methods. In my opinion, these results do not provide explicit information on the accuracy of the methods.	Yes, these results do not prove the accuracy of the methods. For that, we chose presenting one method (wavelet method) and discuss it with two benchmark methods.
Page 19, line 5: the BDT methods is used to "validate" the results of the proposed	In this study, we consider the BDT method as a benchmark method. We do not consider a specific

methodologies. This choice implies that the BDT method is more accurate than the new methods introduced in this manuscript. Is this correct? If so, what are the benefits/ advantages of using the two proposed techniques?	method to be the "true" or "reference" one, we only apply several methods to have a general idea on the uncertainties in the identification of morphological units.
Page 19, lines 19-20: please clarify this sentence.	We will delete this sentence in the revised version.
Page 19, line 23: the manuscript states that the results of BDT "are closer to the other methods and to reality". I strongly recommend to better substantiating this sentence. Which are the "other methods"? What does "reality" mean? Was the BDT method compared with field data? In which case study?	True, This sentence is not clear, we will delete it.
Page 20, lines 3-4: could please the authors clarify this sentence?	We will change all the discussion according to the results that we will have.
Page 20, lines 6-7: please rephrase this sentence.	This sentence is not clear, we will change it in the revised version.
Page 21, line 2: a Froude number of 0.30 looks a bit large. Could please the authors explain this result?	We will dismiss the Froude number In the revised version. But for example in the study of Jowett (1993) and Clifford et al. (2006), they found values close to 0.3, so we think that these values are a bit large but acceptable.  Fig. 1 Kernel density functions of velocity/depth ratio, Froude number, and water surface slope in pool, run, and riffle habitats.
Page 21, line 3: it seems that the average values are driven by the results of the Graulade river, Are the average values representative of the sample?	If we exclude the Graulade river we will find and average of 0.20 for the index and 0.17 for the wavelet method. These results are nearly close the 0.23 and 0.20.
Page 21, line 10-17: these lines present a comparison between the results of this study and some of the previous studies. The authors might (or might not) consider using a table to summarise these comparisons.	This is a good idea.
Page 21, line 21: I suggest motivating this sentence. Why aren't the previous methods considered quantitative?	We will delete all this sentence
Page 22, line 1: is "crossing" the most appropriate word?	We will delete all these sentences
Page 22, line 3: please clarify this sentence	We will change this sentence in the revised version

References:

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

Clifford, N. J., Harmar, O. P., Harvey, G., & Petts, G. E. (2006). Physical habitat, eco-hydraulics and river design: a review and re-evaluation of some popular concepts and methods. Aquatic conservation: marine and freshwater ecosystems, 16(4), 389-408.

Gangodagamage, C., Barnes, E., & Foufoula-Georgiou, E. (2007). Scaling in river corridor widths depicts organization in valley morphology. Geomorphology, 91(3-4), 198-215.

Graps, A.: An introduction to wavelets, IEEE computational science and engineering, 2, 50–61, 1995.

Hauer, C., Mandlburger, G., & Habersack, H. (2009). Hydraulically related hydro-morphological units: description based on a new conceptual mesohabitat evaluation model (MEM) using LiDAR data as geometric input. River Research and Applications, 25(1), 29-47.

Jowett, I. G.: A method for objectively identifying pool, run, and riffle habitats from physical measurements, New Zealand journal of marine and freshwater research, 27, 241–248, 1993.

Krueger, A. and Frothingham, K.: Application and comparison of geomorphological and hydrological pool and riffle quantification methods, Geogr Bull, 48, 85–95, 2007.

Lashermes, B., & Foufoula-Georgiou, E. (2007). Area and width functions of river networks: New results on multifractal properties. Water Resources Research, 43(9).

Legleiter, C. J. (2014). A geostatistical framework for quantifying the reach-scale spatial structure of river morphology: 2. Application to restored and natural channels. Geomorphology, 205, 85-101

Lilly, J. M. and Olhede, S. C.: Wavelet ridge estimation of jointly modulated multivariate oscillations, in: Signals, Systems and Computers, 2009 Conference Record of the Forty-Third Asilomar Conference on, pp. 452–456, IEEE, 2009.

Lilly, J. M., & Olhede, S. C. (2010). On the analytic wavelet transform. IEEE Transactions on Information Theory, 56 (8), 4135–4156.

Mallat, S., A wavelet tour of signal processing, 2nd edition. New York: Academic Press, 1999

McKean, J., Nagel, D., Tonina, D., Bailey, P., Wright, C. W., Bohn, C., & Nayegandhi, A. (2009). Remote sensing of channels and riparian zones with a narrow-beam aquatic-terrestrial LIDAR. Remote Sensing, 1(4), 1065-1096.

Mosley, M. P. (1982). Procedure for characterising river channels, Water Soil Misc.

Muzy, J. F., Bacry, E., & Arneodo, A. (1993). Multifractal formalism for fractal signals: The structure-function approach versus the wavelet-transform modulus-maxima method. Physical review E, 47(2), 875.

Navratil, O., Albert, M., Herouin, E., and Gresillon, J.: Determination of bankfull discharge magnitude and frequency: comparison of methods on 16 gravel-bed river reaches, Earth Surface Processes and Landforms, 31, 1345–1363, 2006.

O'Neill, M. P., & Abrahams, A. D. (1984). Objective identification of pools and riffles. Water resources research, 20(7), 921-926.

Pasternack, G. B., & Brown, R. A. (2013, January). Ecohydraulic Design of Riffle-Pool Relief and Morphological Unit Geometry in Support of Regulated Gravel-Bed River Rehabilitation. In Ecohydraulics (p. 337).

Torrence, C., and G. P. Compo (1998), A Practical Guide to Wavelet Analysis, Bull. Amer. Meteor. Soc., 79, 61–78.

Wheaton, J.M., Pasternack, G.B., Merz, J.E., 2004a. Spawning habitat rehabilitation - 1. Conceptual approach & methods. International Journal of River Basin Management 2(1), 3-20.