The authors have done an excellent work in revising this manuscript based on the comments from the three reviewers. Most of my earlier concerns have been addressed by the authors in this revised version. The manuscript is now situated in context, with appropriate referencing, and the novelty is clearer. The discussion is better and the application (morphological characterization) makes it a more complete paper, suitable for publication in HESS. I only have a few comments listed below that I am sure the authors will be able to address.

1. I would suggest reducing the length of the title, for instance by removing "in the coastal zone" since the mention of "high-energy tidal environment" already suggests that it is in a coastal environment.
2. In the abstract (p. 1 line 17), I would replace the word "harsh" by either "difficult" or "challenging". I feel like these have a different meaning and environmental conditions are not necessarily harsh, but can be challenging for surveying. The authors use "challenging" to describe them in the main text, and the abstract should be consistent with the main text.
3. As noted in the previous round of reviews, I recommend not using digital elevation model (DEM) when considering both elevation and depth like in this study. Since the data include cottages and vegetation, I believe that the most appropriate term would be Digital Surface Model (DSM), as opposed to Digital Terrain Model (DTM) that represents a "bare-earth" model.
4. Also noted before: "landscape" has different meanings depending on the field of study (e.g. in landscape ecology or remote sensing). I recommend removing the two instances on page 17 and replacing them by "terrain".
5. Another one noted before: change "small-scale" and "large-scale" for "fine-scale" and "broad-scale" throughout the text and in figures (e.g. Fig. 4). The formers have different meanings in cartography and other fields than in this study. The latters are less ambiguous.
6. On page 8 (lines 3 to 14), this should be moved to the methods section below, likely between lines 19 and 20 of that same page. At line 9, I would change the first "surface" for a word like "top" to make it clearer, e.g. "…located in the river with its top just below the water surface."
7. I understand how the method that is described in this manuscript is more transparent, reproducible and user-friendly than current alternatives. However, I am unsure of the level of reproducibility for two reasons. First, while the authors use RiHYDRO, HydroFusion and LiDAR Survey Studio as examples for describing the lack of transparency in available software (p. 6), the proposed method still requires many software that may not be widely available for all users and are not necessarily more transparent (e.g. RiPROCESS, HydroVish, Fledermaus, MATLAB). Second, many steps involve manual processing (e.g. filtering, extracting the shallow surface) or subjective decisions (e.g. parameters for automatic filtering, classification trees, values of 4 for standard deviation to differentiate between features). I appreciate that the authors mention these limitations (e.g. p. 11 and 28), however I believe that care should be used when making claims like at page 22 ("it is open to the public"), especially since the software used may not be accessible to the public. I wouldn't go as far as suggesting the removal

of mentions of “reproducible”, but I would be curious to see if this issue will also be of concern to other reviewers or the editor.

8. On page 13, line 11, what do the authors mean by “only taking the top 95-100% of water points into account”? If 100% of the points are considered, then they are all accounted for and can’t possibly increase reliability. Please clarify.
9. On page 13, lines 16 to 31 are confusing. I am unsure what is the relationship between the 2 x 2 m water surface and the 0.5 x 0.5 m surface. I understand that the 2 x 2 m was built to remove outliers, but what is it used for then? This section requires rewording or clarifications.
10. On pages 16 and 17, please specify what the standard deviation represents (for the BPI). Is it the standard deviation of depth/elevation values within the window of analysis?
11. On page 17 (lines 3-4), I do not understand what the authors mean by “the altitude was exaggerated 10 times before the classification, to enable the BTM to detect the shapes of the landscapes”. A vertical exaggeration in the visual representation of data would not change the altitude (depth or elevation) values and thus would not impact the results. If this is the case, then this sentence is unnecessary and can be removed. However, if the altitude values were actually altered and multiplied by a factor of 10, I am not sure if the analysis is still valid, although it would likely not change the relative values of pixels and still identify peaks and pits (but maybe reduce the amount of flat areas?). This needs to be clarified.
12. Page 17, line 5: “the best results” based on what? Visual interpretation? Was it a subjective decision? Please specify.
13. In Figure 5 and associated text, is everything >0.94 m really a beach dune? Weren’t there cottages and other features? Would another term be more appropriate and all-encompassing of features that were above the water level?
14. On page 18, I believe that the window sizes are wrong. They would only make sense if a radius was used instead of a window of analysis. Window sizes need to be odd numbers and based on the pixel size (0.5 m), the window could not be of 100 m or 250 m wide. Please revise these measurements, indicate whether a window of analysis (square) was used or a radius (circle), and whether these numbers are the numbers of pixels of the window or the actual area covered by the window (in either cases it should be an odd number).
15. For your information, standard deviation (cf. classification trees) is used as a measure of rugosity in geomorphometry, so when the authors used it to distinguish between bars and larger features, they used a measure of broad-scale rugosity.
16. Page 18, lines 29-30: “4 were found to be a suitable ratio threshold”. Based on what (e.g. visual interpretation, etc.)?
17. In Figure 10 (B-C-D), I would add a category in the legend to characterize the grey areas (that I assume are no data, i.e. the areas that did not correspond to any of the criteria in the decision/classification tree). This category could be named “Transition zones”.

18. On page 25, lines 4 to 17 are repetitive to the methods section (p. 9). I would bring back the description of the environmental conditions at time of survey in the methods section, and keep the discussion on their implications at p. 25.

19. The use of English language could be improved before publication, although it is not a big concern at the moment as the text remains clear. For instance, p. 10 line 21 should read “Steps 5-8 represent” instead or “Step 5-8 represents”, p. 12 line 14 “currently” should be removed since it was at time of survey, p. 18 line 29 “was” instead of “were”, p. 29 line 18 “are” instead of “is”, etc.. These are only a few examples of where corrections should be made. Also, in some parts of the text that describe the methods and results, the past tense should be used.