Reviewer #3

GENERAL COMMENTS:

The paper addresses a relevant scientific question which is well within the scope of Ocean Science. The authors present a methodology with the aim to quantitatively define the land-sea boundary in wave-dominated and micro-tidal environments. The presented methodology builds on met-ocean datasets which are well-known and frequently used in the field. The authors conclude that the proposed land-sea boundary (coastal fringe) definition is a generic method. However, as also stated by the author, the correct choice of met-ocean or biogeochemical variables might be case dependent and the presented application is tailored to the case specific conditions at the Catalan coast.

It would significantly improve the general applicability of the method if the authors could briefly describe how one should choose the variables that reflect the influence of the land border in a specific application.

(pp. 5, lines 5-8) The following definition has been added to the text: “Although other definitions of the coastal boundary can be based on river plumes or bio-geochemical processes, it has been intended to focus on a more hydro-dynamical expression of such boundary for wave-driven coasts.”

Moreover, by comparing the results to other land-sea border definitions (validation) and by providing uncertainty estimate of the computed coastal zone limit the authors would make the methodology stronger.

Thank you very much for the remark. We have added several comments on this issue throughout the paper. Please find two examples below:

(pp. 2, lines 1-8) *“There is, thus, a need for a systematic and objective definition of the coastal fringe that considers underlying processes and that has general applicability allowing for the time/space dynamics of this fringe. This type of approach has been explored in the literature, where for instance Sánchez-Arcilla and Simpson (2002) reviewed a number of possibilities based on a dynamic balance of competing processes (i.e. drivers) such as inertial effects, geostrophic steering, sea bed friction or water column stratification. Another suitable option is to focus on the consequences of such processes, such as the nearshore morphodynamic features (Geleynse et al., 2012) (i.e. deltas, sand spits, overwash fans, beach berms). Both complementary classifications requires spatial data that needs to be updated accordingly within timescales that may range from years (i.e. long-term erosion due to sea level rise) to days (i.e. storm-scale).”*

(pp. 16, lines 13-17) We have added the comment: *“The coastal boundaries suggested by Sánchez-Arcilla and Simpson (2002) for the Catalan Coast can be 0.1-0.6km (frictional coupling of fluids between shelf and nearshore), 10km (non-linear coupling between shelf and slope), 1km (non-linear coupling between shelf and nearshore), among other suggested values of the same order of magnitude. The “l” provided in this analysis is slightly larger than the value given for the frictional coupling of fluids between shelf and nearshore, whereas it is similar or smaller than in the non-linear couplings. Nevertheless, the orders of magnitude are similar.”*

The scientific methods and assumptions are in general valid and clearly outlined, even though further clarifications are required at certain sections (see specific comments). The paper is well structured in general; however, certain elements should be better explained (see specific comments).

<u>SPECIFIC COMMENTS:</u>

<u>Title:</u>

- The title should indicate that the methodology to quantitatively define the land-sea coastal border was only tested for a case study in a wave-dominated and micro-tidal environment.

The new title is adapted to this idea: “The land-sea coastal border: A quantitative definition by considering the wind and wave conditions in a wave-dominated, micro-tidal environment”.

<u>Abstract:</u>

- I propose to change the term “90th quantile” to “90th percentile” throughout the paper. The authors refer to the 90th 100-quantile which is called percentile.

Thank you very much for this remark. Therefore, we have substituted “quantile” by “percentile”.

<u>Study area:</u>

- The authors state (page 4, line 5) that the focus area is the Spanish north-eastern Mediterranean coast due to the availability of in-situ and Sentinel images for support. It is not clear how the Sentinel images were utilised in the methodology as a support (unless they were used for the SWAN model validation, which is not stated in the paper).

We agree with this point. Please refer to the answer below, to the same referee, referring to Fig. 5.

<u>Methods:</u>

- Further background information on the Unified Model and/or the wind field data should be given. The wave data is explained in much more detail

compared to the wind data.

(pp. 6 lines 4-10) The following explanation has been added to the text: “There are two atmospheric prognostics: the dry one (three-dimensional wind components, potential temperature, Exner pressure and density) and the moist one (specific humidity and prognostic cloud fields (Walters et al. (2011)). Both long and short radiations (from the sun and the Earth itself) are included, whereas the effect of aerosols reflecting them is taken into consideration.”

- According to Cullen (1993) the operational forecast grid for the Unified Model is 0,833 degree (latitude) and 1,25 degree (longitude), whereas the standard climate and upper atmosphere configuration uses 2,5 degree (latitude) and 3,75 degree (longitude). Is it really the second configuration which is used in this paper? This resolution would mean approximately 250km (latitude) and 310 km (longitude). That is a very coarse resolution for this purpose.

Thank you very much for this remark. The horizontal resolution of the atmospheric model was a gridsize of 17 km, the same that the UK Met Office global deterministic forecast model. (pp. 6, 12-13) “The computational domain of the wind field spans the whole Mediterranean Sea using a regular grid with spacing of 17km and a time step of 1h.”

Also, we have improved the flow chart to clarify, along with the existing definition of the methodology, that we interpolate the wind/wave data field in order to obtain a finer grid from which to compute the geo-statistical anisotropy along the transects.

- I suggest adding steps to the methodology figure (Figure 3) for the wave and wind model validation, interpolation, as well as for the distribution fitting (Gaussian copula model).

We have proceeded as indicated. The caption of the figure is also modified: “Flow-chart summarizing the methodology used in this paper. The dashed blue rectangle represents the input data, the red dashed rectangle indicates the output data. Only the wind velocity is obtained from an external source, the rest of the steps have been carried out for this analysis. Rectangles indicate data generation (input/output) and rhombuses the subsequent analyses of the proposed methodology.”

- It is mentioned (page 6, line 9) that wave fields have been validated. Validation results should also be included for the wind field data. Reference to the wind field validation is only given in the discussion section (page 15, line 3-4). I suggest moving this sentence to the Methods section where the United Model is described.

Validation of the wind fields is not included in this paper. Then, we have added the following: (pp. 5 line 26) “These wind data are validated in (Martin et al. ((2006)).”

- Is there any reason why the 90th percentile is used in equation 6? Is it based on expert knowledge or literature?

Indeed, the selection of the 90th quantile is a convention commonly followed in Literature (Eastoe 2013, Bernadara 2014).

<u>Results:</u>

- The figures 6,7,8,9 are presented on pages 11-to 14 while described on page 7. This makes it hard for the reader to follow the paper. Consider to move them closer to the place where they are described.

We agree with this suggestion. The figures appear after the text, in the source file. This problem happens because the graphs are large and self locate in these pages. We believe that this phenomenon would only occur in this pdf format, but it should be automatically solved in an online edition.

- In Figure 5 red dots are labelled as Altimeter data. Is this data coming from Sentinel images? If yes, please explain both in the legend and in the text, and also add which mission it is (e.g. 3A).

The altimeter data comes from Jason-2, Jason-3 and Cryosat. Sentinel 3A data has not been used in this contribution.

(Fig. 5) We have added the clarification: "The red dots are altimeter data from altimeter data (Jason 2, Jason 3 and Cryosat)"

(pp. 7 line 18) We have added: "The SWAN model simulations have been validated with significant wave height (H_s), registered with buoys and altimeter data, at the southern (Tarragona location) and northern (Begur location) coastal sectors (Figs. 4 and 5)."

- The calculated coastal zone limit values are not mentioned explicitly in the results section, even though they are depicted in Figure 8-9 and mentioned in the abstract. I suggest mentioning them in the text as this is the main objective of the methodology.

We have modified the text so now it reads: "The coastal zone limit "l", corresponding to the 90th percentile of the total variance (fringe between 0
and 100km), is calculated from equation 6 (Figs. 8 and 9) and is 3km. It is consistent with time interval (month of study) and location (sector)"

- Please use the word "coastal zone limit" consistently. Sometimes it is only called "limit".

The suggested action has been carried out throughout the text (pp.10 line 1, pp. 17, line 5).

Discussion:

- The authors write that "The calculated anisotropies should be as robust as the starting wave or wind fields that are employed in the analysis" – that is why the robustness of the wind field should be better defined in the Methods section.

We agree with the reviewer. The UK Met Office wind fields has shown systematically good accuracy (see Martin et al. 2006, Brown et al. 2012, Walters et al. 2011). We have assumed that the wind fields have state-in-the-art accuracy and we have focused on validating the wave fields in the Results and we hope that this could be a valid procedure.

- Figure 6-7: the description of the hexagons in the heatmap should be added to the figure as they are only described in the text. Also, the description of the blue dashed line at 20 km should be added as in Figure 8 and 9.

The suggested changes have been performed.

- Figure 10: The x axis represents the months within a year cycle. Which year is it? And why only months 1, 2, 3, 11,12 were selected?

These months (year 2016; 11, 12 and year 2017; 1,2,3) span the available data. As mentioned above, the available wind fields ranged this timeline (at the moment of writing this paper). We accept this shortcoming as a limitation of our contribution.

The following text has been added to the caption of the figure: "The plot shows the variation with time (horizontal axis) between November of 2016 and March of 2017. The parameters are placed in a manner that they start from January."

- The authors write (page 16, line 5) that the correlation between R_Vw and R_Hs is the strongest for the Begur transect. On the other hand, in Figure 10 the Begur transect (orange dots) has a correlation parameter around 0 (max ~ 0.026). This figure indicates that the Mataro transect has the strongest correlation parameter, not Begur.

I suggest clarifying this.

Thank you for pointing this out. We have clarified in the text that it is the "overall" dependence that is stronger in Begur: "The overall mutual dependence of R_vw and R_Hs is strongest for the northern-most transect (Begur), where the topo-bathymetric control of the Pyrenees and their submerged signature becomes better defined."

References:

- The number of references is rather high (57). Moreover the share of references originating from the same authors is also high.

Although all references are of strong interest, we have followed the suggestion of the referee to reduce the number of references. Here is a list:

-Bolaños and Sánchez-Arcilla (2006), as it can be represented by Bolaños et al. (2009).

-Bolaños et al. (2007), as it only appears once in the text and along other references.
-Pallarés et al. (2013), for the same reason.
-The thesis of E. Pallarés can be represented by Pallarés et al. (2014).
-Sánchez-Arcilla et al. (2008), as it is similar to Bolaños et al. (2009).
-Sánchez-Arcilla et al. (2016), as it only appears once, and along with another reference. Also, it is more about ports.
-Sierra et al. (2017) has been obviated, as it is well represented by the other bibliography that accompany it in the “Introduction”.

Additionally, it has been added new references, that the authors consider that suit better the general messages of this contribution. Note that some of these references come from the other reviewers’ suggestions.

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