

## ***Interactive comment on “The land-sea coastal border: A quantitative definition” by Agustín Sánchez-Arcilla et al.***

**Anonymous Referee #3**

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### 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 in-

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fluence of the land border in a specific application. 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.

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).

### SPECIFIC COMMENTS:

Title:

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

Abstract:

- 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.

Study area:

- 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).

Methods:

- 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.

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- 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.

- 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).

- 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.

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

Results:

- 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.

- 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 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.

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

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"limit".

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.

- 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.

- 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?

- 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.

References:

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

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