Reviewer #1

GENERAL COMMENTS

This study is aimed at assessing the definition (say distance from the coast) of land-sea boundary. For this purpose, wind (from UKMO) and wave (using SWAN) model data are used. No additional oceanic/atmospheric variables are taken into account. The whole analysis is based on the calculation of anisotropy of 2-D fields, which is computed along four transects of the Catalan Coast (used as test site). The paper is well-written, but it lacks a connection with other methods currently employed to define that boundary (e.g. using oceanic variables, as salinity, or depth) and uses an unique strategy (anisotropy + quantile threshold) for the identification of the boundary. In my opinion the paper can be improved and worthy to be published after a major revision that assesses the stability of the boundary computation and its seasonality.

SPECIFIC COMMENTS

Title.

Since the definition is based on wind and waves I suggest mentioning them in the title. Or at least making it clear that in the study a methodology is proposed (which can be then used with other variables).

Thank you so much for the suggestion. We have adapted our title to it. The article has been renamed as: "The land-sea coastal border: A quantitative definition by considering the wind and wave conditions in a wave-dominated, micro-tidal environment."

Abstract.

"The more robust estimator [...]". How the robustness of the estimators expressed? Did authors check, for example, the sensitivity of the results on the quantile threshold?
(pp. 1, line 6) We have changed "robust" by "viable", as the selection of the 90th percentile is a convention commonly followed in Literature (Eastoe 2013, Bernadara 2013).

Can authors show the distribution function of anisotropy for wave and wind fields?
(pp. 1, line 7) It is the 90th quantile of the variance of the anisotropies. We have

rectified by introducing this specification in the text.

<u>Introduction.</u>

The land-sea border problem is presented by means of references mostly pointing to the same group that wrote the paper. My suggestion is to improve the overall view of the problem. o It seems to me that other references are more appropriate for the SWAN model.

The state-of-the-art has been improved with recents works in the same study area.

The number of citations from the same group has been reduced. Here is a list of the ones that have been obviated:
-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".

It has been added references, not only about the SWAN model, but on spectral wave modelling as well:

Bertotti, L., Bidlot, J., Bunney, C., Cavaleri, L., Passeri, L. D., Gomez, M., Lefe, J., Paccagnella, T., Torrisi, L., Valentini, A., and Vocino, A.: Performance of different forecast systems in an exceptional storm in the Western Mediterranean Sea, Quarterly Journal of the Royal Meteorological Society, 138, 34–55, https://doi.org/10.1002/qj.892, https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.892, 2012.

Booij, N., Ris, R., and Holthuijsen, L.: A third-generation wave model for coastal regions, Part I, Model description and validation, Journal of Geophysical Research, 104 (C4), 7649–7666, 1999.

Cavaleri, L., Bertotti, L., and Pezzutto, P.: Accuracy of altimeter data in inner and coastal seas, Ocean Science Discussions, 2018, 1–13, https://doi.org/10.5194/os-2018-81, https://www.ocean-sci-discuss.net/os-2018-81/, 2018.

Lionello, P. and Sanna, A.: Mediterranean wave climate variability and its links with NAO and Indian Monsoon, Climate Dynamics, 25, 611–623, *https://doi.org/10.1007/s00382-005-0025-4, 2005.*

Qi, J., Chen, C., Beardsley, R. C., Perrie, W., Cowles, G. W., and Lai, Z.: An unstructured-grid finite-volume surface wave model (FVCOM-SWAVE): Implementation, validations and applications, Ocean Modelling, 28, 153 – 166, https://doi.org/https://doi.org/10.1016/j.ocemod.2009.01.007, http://www.sciencedirect.com/science/article/pii/S1463500309000067, the Sixth International Workshop on Unstructured Mesh Numerical Modelling of Coastal, Shelf and Ocean Flows, 2009.

Roland, A. and Ardhuin, F.: On the developments of spectral wave models: Numerics and parameterizations for the coastal ocean, Ocean Dynamics, 64, 833–846, 2014.

Roland, A., Zhang, Y. J., Wang, H. V., Meng, Y., Teng, Y.-C., Maderich, V., Brovchenko, I., Dutour-Sikiric, M., and Zanke, U.: A fully coupled 3D wave-current interaction model on unstructured grids, Journal of Geophysical Research: Oceans, 117, https://doi.org/10.1029/2012JC007952, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2012JC007952, 2012.

Staneva, J., Wahle, K., Günther, H., and Stanev, E.: Coupling of wave and circulation models in coastal-ocean predicting systems: a case study for the German Bight, Ocean Science, 12, 797–806, https://doi.org/10.5194/os-12-797-2016, https://www.ocean-sci.net/12/797/2016/, 2016.

Wiese, A., Staneva, J., Schulz-Stellenfleth, J., Behrens, A., Fenoglio-Marc, L., and Bidlot, J.-R.: Synergy of wind wave model simulations and satellite observations during extreme events, Ocean Science, 14, 1503–1521, https://doi.org/10.5194/os-14-1503-2018, https://www.ocean-sci.net/14/1503/2018/, 2018.

WISE Group: Wave modelling-the state of the art, Prog. Oceanogr., 75, 603–674, 2007.

Zijlema, M.: Computation of wind-wave spectra in coastal waters with {SWAN} on unstructured grids, Coastal Engineering, 57, 267 – 277, https://doi.org/http://dx.doi.org/10.1016/j.coastaleng.2009.10.011, http://www.sciencedirect.com/science/article/pii/S0378383909001616, 2010.

Theoretical background.

I warmly suggest improving this section to make the reader more familiar with the concept of anisotropy. A couple of examples (high anisotropy, low anisotropy) using different set of wave/wind data will help to familiarize with the concepts here presented.

A clarification has been added to the introduction (pp. 2, line 15): “A wind or wave field that has a high anisotropy can present a predominant wind or wave direction, respectively.” The interpretation of the geometric anisotropy values and the possible underlying physical processes are included throughout the Discussion section.

(pp. 15, lines 5-6) The phrase: “ which facilitates multiple simulations to perform a statistically stable analysis of anisotropy” is misleading and is not very much informative. Thus, it is eliminated.

The definition of R seems not to be consistent with the one given by Chorti and Hristopulos (2008). I suggest improving the presentation of the method.
(pp. 3, lines 2-7) We have improved the definition as: “Given a spatio-temporal field X(s, t), where s stands for a 2-D vector (zonal and meridional components) and t is the time, it is assumed that the iso-level contours of the correlation functions are invariant, i.e. ellipses in two dimensions. The main axis of these ellipses are termed u and v, respectively (see Fig. 1). The metric of the geometric anisotropy, then, becomes their ratio R = u / v (R exists [0, inf)) (Chorti and Hristopulos, 2008; Petrakis and Hristopulos, 2017). An R value close to unity means that u and v are isotropic, i.e. homogeneous across the different directional sectors. As R increases, the difference between the main axis increase, showing higher anisotropy at certain directional sectors.”

Is there a difference between the adopted method and others used to compute the consistency of spatial fields, such as the structure tensor?
We understand that the structure tensor is a similar concept to the anisotropy, but what we do here is to put emphasis on the anisotropy of the wind and wave conditions near the coast and to statistically quantify its spatial distribution.

For the Copula function, I suggest changing (u, v)-variables with other symbols as they are already used for the definition of anisotropy.
We have proceeded as suggested.

Methods

How the covariance of anisotropy is computed?

The covariance matrix is computed following (Chorti and Hristopulos, 2008):

From a X(s,t), where s is a 2D-vector (zonal (i) and meridional (j) component) and t is the time. We assume, for each time step:

1. The covariance matrix Q can be computed: Q(i,j) = E [dX(s)/ds(i); dX(s)/ds(j)]

Where E[·] are the ensemble averages.

I suspect the wind model resolution is not 2.5 and 3.75 . However, were they km (instead of degrees), the model seems not to be enough resolved to provide accurate data at 2-3 km scale, which is the final value provided for the border. The SWAN model at 600 m close the shoreline is at limit in this respect (the border encompasses 3-4 grid points). Can authors comment on this aspect?

Thank you for the remark. The spatial resolution of the wind fields is 17 km. It has been corrected in the paper.

We agree that a spatial resolution of 600 m could not be enough for solving wave breaking and shallow water processes along the whole coastline. However, such a resolution can provide a good assessment on wave generation and propagation (please refer to the error metrics in Table 1 and Figure 5.

The use of unstructured meshes avoids nesting, that may be an important source of uncertainty. This work shows that the continental shelf (mainly, the inner shelf) joint with the wind fields patterns, are strongly correlated with the wave fields.

Additionally, we have previously dealt with this issue by carrying out an inverse distance weight type of interpolation in order to gain resolution, before computing the geo-statistical anisotropy of Vw and Hs.

If you define the resolution in meters, I guess it is smaller near the coast, not higher.

We have substituted “higher” by the term “denser”, in order to make the text clearer to the reader.

Which period is spanned by the analysis? (February 2017? Why not using a longer period, say 2016-2017?).

The period ranges from November 2016 to March 2017. Such limitation comes from the availability (at the moment of writing) of the wind fields.

As far as waves are concerned, using the proposed methodology, the distance of the border should change between August (instead of November, Fig. 9) and February. A season-based classification of the border would be a sound improvement of the paper.

As mentioned above, unfortunately, the authors do not have data for August.

We agree that the anisotropy of the wind and wave fields may depend on seasonality. However, according to our data, the 90th percentile of the variance of the anisotropy does not depend on the seasonality. We would like to make an emphasis that it is the quantile, and not the absolute value, that stays the same throughout the year.

About the radius and quantile threshold. Those two values seem to me quite arbitrary and not directly physically-based. Authors could do a sensitivity analysis to show how the results depend upon those values (the quintile, in particular). This is an important task in order to evaluate the stability of the proposed methodology (and then make it usable in other contexts) and provide the uncertainty of the location of the boundary. o It would be useful to plot the quantile values on the heatmaps.

The 90th percentile in an environmental parameter (e.g. Hs) is a convention commonly followed in Literature (Eastoe 2013, Bernadara 2014). We have used this same idea, applied to the variance of the geo-statistical anisotropy of Vw and Hs. This concept is illustrated on Figs. 8 and 9.

Please explain what is the count in the heatmap's caption?

The comment "The counts are the number of elements within a hexagon.
" has been added to Figs. 6 and 7.

Additionally, the following text has been added (pp. 7, lines 7-11): Heatmaps are used to represent the spatial distribution of the geo-statistical anisotropy, showing how the density of R behaves as a function of distance to the coast and time (see Figs. 6 and 7). These maps are scatter plots that act as a 2D-histogram, in which two variables (in this case, R and distance to the coast) are grouped in pre-defined intervals. The elements selected to aggregate samples for the heatmap are hexagons with side 5km and a scale for anisotropy of 20 units for both R(Vw) and R(Hs) .

I suggest putting the wave model assessment in a dedicated sub-section

separated from the results of the anisotropy analysis.

The appearance of the validation graphs along with the text for the anisotropy analysis is because of the limitations of the LaTeX file. We are certain that the figures will appear along with the text, in the final version.

Is there a reason why in the panels of Fig 8 and 9 the dashed green vertical lines are not aligned (horizontal axes seem consistent)?

The figures are about 1 mm narrower above because of the number of digits in the y-axis. We believe that it should not significantly interfere in the interpretation of the graphs.