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Comment on nhess-2022-103

Anonymous Referee #2

Referee comment on "Wind-wave characteristics and extremes along the Emilia-Romagna coast" by Umesh Pranavam Ayyappan Pillai et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-103-RC2, 2022

General comments

The manuscript represent an important work and a substantial contribution to the understanding of natural hazards on ER area by the use of new methods like the hazard index. The used data and tools are up to international state of the art.

The scientific approaches and the applied methods are valid and discussed in an appropriate and balanced way. There are many references considering previous works on the area. The scientific methods and assumptions are clearly presented and can be reproduced thanks to the codes in annexe. The discussion could be broadened by a point on wind quality which is detailed below in this review.

The scientific data and results are precisely and clearly illustrated and presented with appropriate figures and tables. Rare remarks below are aimed to document still wider the results.

Specific comments

The forcing by a 6-hourly wind at 0,125° on a area with such an important space and time variability than the Adriatic Sea seems to be the first limit of the wave climatology. Several results show it : first of all, the degraded scores on summer. Indeed, you show the worse correlation and bias of SWH in this season. The wave model understimate the wave height of 11 cm. It is known that global atmospheric model at such a scale don't represent explicitly the convection and are not appropriate to simulate surface wind due to heating flux that occurs at this season. It is consistent with the lack of energy in the wave model compared to the observation.

This limit also appear through the small standard deviation in summer. Is the standard deviation of the observation so weak than in the model during this season ? A part of this decrease is due to the summer low wave height, but it could also come partially from a poor representation of convective wind.

I would recommand to better document this limit by some elements, for instance :

- a comparison with an observed wind climatology, even by a station on land. It could also be the climatology from a mesoscale model with hourly time step. Does it present the same standard deviation of wind speed in summer than the experiment, the same wind rose ? Are the relative quality of wind different between the seasons ?

- more explorative : what is the error in SWH or period depending on the wind direction observation (or from a mesoscale model) ? Indeed the global ECMWF model may have specific limitation for some local wind.

Moreover, a mention of this limit should appear in the discussion. Even in winter a 6-hourly wind has consequences on the model wave quality. Mediterranean sea is known for its very dynamic storms.

l.151 Sensitivity experiments for wave model parametrizations : no test of different parameters value of each physics has been conducted, at least there is no mention of it in the paper. The validation of the method by 3 model configurations seems all the same sufficient. But the conclusion could be less affirmative regarding the better capacity of ST6 to represent sea state on ER area. We could think that another parametrisation of ST4 than the one in Ardhuin et al. 2010 could have produced better results than EXP3.

So I advice to moderate the sentence l.421-422 (Summary and conclusions) in a way like : « The sensitivity tests has shown the good accuracy of ST6+SHOWEX physics for

wave hindcasts in the study area. »

l.160 and 191 : It would be very interesting to add the mean value of the observation for the sensitivity periods and the whole 10 years. Thus the reader would be aware of the relative error at this point.

l.267 : Is it possible to add a comparison against the observed wave rose of the station 6 ?

l.304 : The bimodality in winter and summer doesn't seem so obvious on the graphics. In winter, I consider visually 3 cases of double peaked spectra. In summer, it is more complicated to distinguish and I don't see a lot more than 1 case. Thus majority of cases appear to be single peaked.

I would recommand either to write clearly the number or proportion of cases that are bimodal by season in order to attest it solidly, or to pursue the exploration of data by adding more cases. Indeed all the days of February (August) of the 10 years could be examined. The proportion of bimodal cases could then be adressed on a significant number of occurrences.

If the results show effectively a proportion of around 30 % of bimodal spectra on these 2 seasons, I would nuance the conclusion. For instance, it would be appropriate to write « during winter and summer the spectra have often/sometimes bimodal characteristics », than « during winter and summer the spectra have bimodal characteristics » (l304-305).

Technical corrections

l.24 : « is crucial » instead of « is a crucial »

l.30 : « Other state-of-the-art models include » instead of « Other state-of-the-art models includes »

l.89 : the formulation isn't very clear. Proposition : « Armaroli et al. (2012) reported that waves originating from east correspond to a proportion of 91%HS < 1,25 m, owing to the controlled fetch. »

l.93 : « action-density » without space.

l.109 : « The model spectrum is sampled in 24 directions and 30 frequencies (0,0500-0,7932 Hz), with an increment factor of 1.1.

l.113 : ST4 isn't mentioned there, wich is a bit confusing. Indeed ST4 and ST6 are both introduced earlier, and are actually used in the validation. It would be clearer to add a mention here of JONSWAP and ST4, precising that they were used only for the validation.

l.176 : Please indicate in the legend of the table 3 the number of used buoys to be sure that it takes into account only the station 6 or the whole control points.

l.388 : The chosen value of Xc could appear in the legend of Table 4. Its value appears only in page 20.

l.418 : I would suppress the upper-case C of Conclusions, except if asked by the editor.