

## ***Interactive comment on “Wind waves on the Black Sea: results of a hindcast study” by V. S. Arkhipkin et al.***

### **Anonymous Referee #1**

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This paper presents a new wave hindcast for the Black Sea and describes some of the aspects of the wave climate in the region. The topic of this work is interesting and can represent a significant contribution to the knowledge of the region. However, I think the paper does not go deep enough in the description of the results and on the physical processes behind the wave variability in the Black Sea. In particular I find several major issues that have to be solved before the paper is suitable for publication in Ocean Science.

Major comments - The wave hindcast can be a very useful tool but only if its results are reliable. The validation of the model is not included in the paper. The authors refer to other papers where the SWAN model has been compared with buoy measurements but it is not clear that the same model configuration with the same forcings have been used

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in those papers. However, I think that this is not enough. The goal of the paper is to show a new hindcast and the validation is an essential part of any new product. To refer to papers which are not easily accessible and to give few details on the comparison do not help the reader to gain confidence on the quality of the hindcast. As I mentioned in my first review, to validate the model, the authors could use wave buoy data, if available, and/or significant wave height obtained from altimetry (e.g. see the AVISO web page).

- I think that the English should be reviewed and improved. A detailed list of all the grammar mistakes would be too extensive to be included here.

- I feel that the analyses performed in the paper are limited and that the dataset can provide much more information. I recommend the authors to see other papers doing similar studies in other regions of the world to get ideas about paper organization and analyses performed on long-term hindcasts. Some examples could be found in:

Charles, E., Idier, D., Thiébot, J., Le Cozannet, G., Pedreros, R., Ardhuin, F., and Planton S., 2012. Present wave climate in the Bay of Biscay: spatiotemporal variability and trends from 1958 to 2001. *Journal of Climate*, 25, 2020–2035. doi:10.1175/JCLI-D-11-00086.1. Charles et al. 2012

Dodet, G., Bertin, X., Taborda, R., 2010. Wave climate variability in the North-East Atlantic Ocean over the last six decades. *Ocean Modelling* 31, 120–131.

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

- There is no detailed discussion of the results. The authors should compare their results with those of other authors and highlight the similarities and discrepancies with them, and not only say that results “agree or not agree” with them without further details (as it is done in the present version of the manuscript)

- There are other atmospheric hindcasts for the period 1950–2010 covering the Black

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Sea with higher resolution than NCEP/NCAR (see for instance the results of ENSEMBLES, PRUDENCE or EuroCordex/MedCordex projects among others). In such semienclosed domain surrounded by steep orography the spatial resolution of winds may play a significant role. If the authors are not able to run the model with higher resolution winds, at least they should discuss the drawbacks of using a low resolution forcing.

Detailed comments I would include section 2 in the introduction as it is a general background of the paper. L16. "We attempted to obtain modern climatic parameters .." What is a modern climatic parameter? I think the goal is not well defined.

Section 3.3. What is the period covered by the hindcast?

In the results, instead of showing only the mean wave field, the authors could show the seasonal averages which would be much more informative. Also, the mean direction and period would be an interesting information to show and discuss.

The authors describe the results (e.g. L8-15 pg 1201) without any support from figures or tables. Time series at given locations would be illustrative to show the storm waves occurrence. Is the November 2007 reproduced by the hindcast?

Fig 1 should include a colorbar in order to identify the depths. Also, what is the actual resolution of the bathymetry?

Figures 6 is referred before talking about Figs 4 and 5.

P1201. L 16-29. The authors describe the atmospheric situation that lead to extreme wave heights. Although the explanation seems plausible, the authors should demonstrate that it is that situation which is associated to the extreme waves. For instance, they could show the SLP fields at the time that the hindcast show extreme waves. Or maybe they could create composites averaging the SLP fields corresponding to all the events with wave heights exceeding a certain threshold.

More details on the method used to compute the return levels should be provided (L3-7  
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pg 1202)

P1202. L11. How are the storms defined? How many time steps are required to define independent storms? How the averaged area and the averaged path length presented in Table 1 are computed?

There is no discussion about the interannual variability of the storminess. Does it correspond to changes in the dominant wind direction? Or is because of the interannual variability of wind intensity? What can be the mechanisms behind this variability? Maybe because of changes in the Northern Hemisphere circulation? The authors could check the correlations with the NH climate indices as the NAO, EA/WR, EA, SCAN, ...

L.14-19 Pg 1203. I do not understand this paragraph and do not see the link with the results discussed in the paper.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 1193, 2014.