Nat. Hazards Earth Syst. Sci. Discuss., 2, C74–C77, 2014
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Natural Hazards and Earth System Sciences
Discussions

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

Anonymous Referee #2

Received and published: 19 February 2014

General comments

This work presents a study on wind waves in the Black Sea using long-term numerical wave hindcast and a number of statistical approaches to identify spatial distribution of mean and maxima annual wave properties as well as interannual and multidecadal trends of the storminess. Although it is an interesting subject and the manuscript represents a contribution to the understanding of storm hazards in the Black Sea, it could be more substantial.

Scientific and technical approaches and the applied methods are traditional but adequate for its purposes. However, the results are discussed in a rather inconsistent and way too concise manner. The problem could be solved by adding a discussion section.

Number and quality of figures and tables is suitable. The use of English still needs improvement (see suggestions in the technical comments).

Specific comments

1. Introduction

Review of current status of wave climate knowledge in the Black Sea is not exhaustive but nevertheless well set. It is not clear which are the gaps that authors would like to fill in implementing their research. Consider using past tense in the overview of previous studies.

Objectives are clearly defined. A point: the authors speak about "modern" i.e. present wind wave parameters but those could be obtained considering shorter time-span, e.g. 10-15 years. The advantage of the large temporal coverage, as in the proposed study, is rather reliability of, for example, extreme parameter estimates.

2. Study area, data and methods

Study area is well described. Much attention is paid to the method of obtaining bathymetry for model simulations. It is not clear what the advantage of using this bathymetry is with respect to already available ones, particularly in view of the fact that the coastal areas are still poorly represented and chosen numerical grid resolution do not allow downscaling to those areas.

To my knowledge NCEP/NCAR reanalysis of wind velocity at 10m height above the sea surface itself has not been used for wave hindcasting in the Black Sea yet. Therefore, results are of interest. With respect to the additional information on the comparison between reanalysed and measured wind speed, it is not clear what the conclusions of the cited study were and how those relate to the presented research. Are there proofs of underestimation of actual wind speed and, if yes, to what extent? This is crucial point for any effort for hindcasting in a basin of complex orography with scarce datasets of in-situ measurements to be assimilated in reanalysis projects.

Wave model set-up description still misses the resolution in the frequency-directional space. It is not clear if 30 min is computational time step (that is somewhat large in view of chosen grid resolution of 5 km) or the model output time step.

3. Results

The estimates of mean significant wave height are smaller in comparison to model result and findings of similar studies. The same applies to SWH maxima, wave period (not indicated which one: mean, zero-crossing, peak) and mean wave length. It seems that presented model results underestimate wave parameters in the Black Sea.

Regarding the model validation the authors refer to studies of other research groups and individuals but it is again unclear how they relate to their own results. Did mentioned studies use the same forcing? The authors just register discrepancies between their results and those published by Polonsky et al. (2011) and Akpinar et al. (2012) without guessing the reason. Furthermore, the authors state “The area with mostly expressed heaving is located in the central part of the sea to the west of the Crimean peninsula.” This finding is also in contradiction with already published results for which no reasoning is provided as well. There must be some explanation of the fact that SWH maxima occur in the most SW and NE “corners” of the sea while the area of largest waves in terms of mean/average SWH is located in the central northern part of the basin.

In this section a description of two prevailing synoptic situations is presented but this lacks balance between hydro- and meteorological elements. It is not clear which atmospheric pattern affects which part of the basin and to what extent, how storms propagate and what the wave parameter evolution is. Storms’ track length is an interesting estimate. It should be discussed in more details in order to fill the above mentioned gap (for example in which part of the basin storms of longest track occur). SWH distribution with respect to their direction (wave roses) for key locations over the basin could offer additional insight.

With respect to extreme value analysis, too little information is provided on why lognormal distribution was preferred and its parameters. There is no fit presented.

As for the storminess variability, the authors do not provide reasoning why they consider storm already an event of "several time steps" duration, during which SWH exceeded only 2m. This is quite common event in the Black Sea; taking into account these events could have significantly blurred statistical analysis of storminess. Only two storminess proxies are considered, namely number of storms and stormy hours, which could be misleading with respect to the wave energy, for example.

It is not demonstrated in a tangible manner which parts of the basin are affected mostly by the severe storms. It is left an impression that, even though the authors have ambition to cover the entire BS, the focus of the investigation is the central northern part of the basin.

References

Following citation in the text is not listed here: Efimov and Komarovskaya, 2009. There are two references to Rusu E., (2010), for which I’d suggest using (2010a) and (2010b).

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C74/2014/nhessd-2-C74-2014-supplement.pdf

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