

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2021-244

Dominik Paprotny (Referee)

Referee comment on "Storm surges and storm wind waves in the Caspian Sea in the present and future climate" by Anna Pavlova et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-244-RC2>, 2021

The paper presents results of hydrodynamic modelling of storm surges and waves in the Caspian Sea. The paper is interesting as it analyses an area that is rarely studied, and as such would be a good contribution. However, the manuscript is rather poorly prepared. I agree with the assessment of the other reviewer, that it doesn't reflect the valuable work made by the authors. The text feels very disjointed, especially in the introduction and conclusions, like a loose set of paragraphs rather than a continuous text. It is also badly edited, full of typos and the language is often unclear. Also, the authors reuse their previous work (including many figures) mostly without proper attribution. Certain methodological aspects of the modelling work are also missing. Finally, the validation is limited and presented in an obscured way. My more specific comments are below. I look forward to the authors' revision.

Data and methods: the crucial aspect missing from the text is a description how the various parameters of the models were chosen. Calibration is not mentioned at all, so are they based on previous studies?

L137: input data for years 2018-2020 are not mentioned.

L188: what threshold which parametric distribution was used for POT?

Results: the result section mixes results, methods and discussion. Text related to methods and discussion should be placed in the proper sections.

L261: the paper doesn't mention earlier than this line that the model's setup and validation was published before (Pavlova et al. 2020). This needs to be explicitly highlighted already in the introduction and unnecessary overlaps with that paper should be removed (such as description of the ADCIRC). Additionally, validation for two stations is rather little. Further, the validation results are presented here in a biased way. Only by checking the previous paper the reader will know that Fig. 3 contains scatterplots for 2009 and 2015 for one of the stations only simply because in those years the correlation was the highest of all years and stations.

L378: here, it is not even mentioned that the validation is taken from a previous paper (Myslenkov et al. 2018). It is also not clear why only one location is used for validation and why "visually, the simulation quality may be assessed as satisfactory." (L382)

L385: what does "34990 points" refer to? Spatial or temporal data points? This needs more description.

L401: 13%? Is that correct?

Section 3.3: very little space is given to future changes, with only one Fig. 18, even though this would be the most interesting and valuable part of the paper.

Discussions and Conclusions: this is section largely condenses the results, without much discussion (which was done in the results section). This section should contain some recommendations for future work as well as information how the information produced in the paper could be used in adaptation to coastal hazards and climate change.

Two figures (1 & 3) are identical as in that earlier paper, and Fig. 4 is a slight modification of two figures from that paper, but this is not mentioned in the caption or paper. This has to be properly attributed. Figs. 2 is also taken without attribution from previous paper (Myslenkov et al. 2018).

The figures vary significantly in size and design, which should be homogenized as much as possible. The quality of the graphs (especially Figs. 3, 4, 15, 17) has to be improved.

Fig. 9: measured where? What is the source of the data?

The text and caption mention Figs. 12a and b, but Fig. 12 contains only one panel – apparently 12b is shown separately as Fig. 18.

