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Reply on RC2

Anna Pavlova et al.

Author 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-AC2>, 2022

Thank you very much for your comments.

you will find our answers to the comments below:

"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."

Answer: Since the proposed work is absolutely new for the Caspian Sea, therefore, the motivation for us was to gain new knowledge. We admit our mistake that we did not describe it in more detail and clearly. We will correct the text and make it easier to read.

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?

Answer: We will create a table for a more accessible understanding of the differences in model parameters.

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

Answer: Experiments with storm surges were not carried out from 2018 to 2020. We will make it clearer in the introduction.

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

Answer: We use different threshold from 2 to 5 m. This information you can find in manuscript. POT method do not use parametric distribution

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

Answer: These sections will be reworked, supplemented and divided more carefully.

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.

Answer: Thanks for the comment. In the introduction, we will describe our previous results from Pavlova (2020). We don't have data for more stations but we will try to find it.

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)

Answer: We used one point with direct wave measurements, because we have no other dimensions. Then we use Altika altimeter data (rads.tudelft.nl). The significant wave height (SWH) at 34 990 points for the period of 2013 to 2016.

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

Answer: this is the total data for the whole sea for the period from 2013 to 2016 . We will clarify it in the text.

L401: 13%? Is that correct?

Answer: the SWH ($4 \times \text{Square root}(m_0)$, where m_0 is the zero-order moment of the wave spectrum, approximately SWH is the mean value from 1/3 of the highest waves or approximately 13% probability of exceedance

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.

Answer: We will provide a more extensive description of the results for future changes.

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.

Answer: We will rewrite the Conclusion and Discussion sections more carefully and add some recommendations.

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).

Answer: We will add the correct links to the figures.

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.

Answer: We will homogenize and improve the quality of the graphs.

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

Answer: We will provide a link to the data source and the name of the station.

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

Answer: Figure 12 contains two panels. But we will clarify this point.