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Comment on nhess-2022-149

Anonymous Referee #2

Referee comment on "The role of preconditioning for extreme storm surges in the western Baltic Sea" by Elin Andrée et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-149-RC2, 2022

This study is a nice example of exploration of potential changes to the existing catastrophic events in future climates. It is based on the perception that extremely dangerous situations in the Baltic Sea are usually formed by a sequence of episodes that are dynamically connected in time rather than a combination of basically random reactions of the sea to various forcing components that are governed by some extreme value distribution. This is a reasonable way forward in the Baltic Sea conditions where the sea level "climate" of several sub-basins may contain statistically almost impossible outliers.

The analysis is sound and professional. All aspects of the modeling efforts have been explained in detail so that even an inexperienced in modeling reader can enjoy the line of thoughts and catch the main points. The use of English and technical aspects of the manuscript are fine. The outcome is carefully justified and the formulated conclusions fully supported.

Therefore, I recommend the manuscript for publication basically as it is.

However, there are some fairly minor items, adjustment of which may make the presentation even better. Only one issue definitely needs clarification for inexperienced readers: sea level elevations propagate in many occasions as (long) waves, so what moves is wave energy rather than water mass.

Abstract, line 4: it would be better to say "prior conditions may influence".

Line 6: consider saying "certain" instead of "different".

Line 7: consider saying “increase in the water level of 36 cm”.

Line 9: it is strongly recommended to say “water mass distributions propagate as (long) waves” (I guess this meant).

Page 2, lines 40–46: it might be useful to mention also wave-driven set-up that may in some occasions provide up to 1/3 of the total surge.

Page 4, line 95: it might be more appropriate to speak about “more unfavourable” preconditioning here, on line 114 and on page 15.

Line 117: from the presentation it seems that “at least two weeks” would be more exact.

Line 121: winds probably caused “intense net transport”.

Lines 121–122: “the maximum …. peaked” sounds strange.

Line 125: as mentioned above, the release of piled-up waters normally occurs in the form of a (long) wave. This wave travels to the southwest while water velocities in it are fairly small (I guess on the order of 10 cm/s); thus “flow” is conceptually incorrect.

Line 141: DWD was already explained.

Line 152: check “methods … is described".

Page 6, Caption to Fig. 1: check “forcing … are”.

Page 8, line 197: The water level was exceptionally high also in the Gulf of Finland. Soomere and Pindsoo (2016, Continental Shelf Research, 115, 53–64, doi: 10.1016/j.csr.2015.12.016) visualised modelled water levels above 80 cm near Tallinn for more than a week in March 1990.

Page 12, line 282: correct “Landort”.

Line 304: as above, it was motion of wave (energy), not really flow of water masses.

Page 13, caption to Fig. 4: correct “capitol”.

Page 15, lines 338–343: I guess that this almost linear dependence may partially reflect the way how surface drag is calculated from the wind speed. I guess that readers would appreciate a short comment on that.

Page 17, lines 363–364: it may make sense to add that a decrease in salinity in the Baltic Sea may add to the sea level rise signal at the entrance of Danish straits.