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Reply on RC2

Elke Magda Inge Meyer et al.

Author comment on "Reconstruction of wind and surge of the 1906 storm tide at the German North Sea coast" by Elke Magda Inge Meyer et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-325-AC3, 2022

We would like to thank the two reviewers for taking the time to review our manuscript and for their valuable comments and suggestions to improve our manuscript. We have written our responses below their points.

Reviewer 2:

recommendation: minor revision

General comments:

The manuscript deals with the reconstruction of a storm surge event occurred in 1906 in the North Sea. The article is well written, pleasant to read, and scientifically sound. I have only some minor comments that the authors should clarify before this manuscript can be published.

Specific comments:

1,18: potential amplification… I would not call it potential amplification but constructive superposition or similar

We have thought about the argument but found "constructive superposition" not fully suitable. We propose to keep potential amplification, but we will add a clear definition of what we mean by this wording. The same wording is used in

Grabemann, I., Gaslikova, L., Brodhagen, T., and Rudolph, E.: Extreme storm tides in the German Bight (North Sea) and their potential for amplification, Nat. Hazards Earth Syst. Sci., 20, 1985–2000, doi:10.5194/nhess-20-1985-2020, 2020.

Table 1: what do the two dots ":" mean in the tide gauge of Cuxhaven? If they mean "some other events" and then the 13thhighest of 1906, the date 2013-12-06 has to be eliminated and substituted by ":".

The reviewer may be looking at the wrong column.

We have drawn some lines between the columns to clarify which HW and Date belong together. If you have any further suggestions, we will change them.

Fig. 2: the entry "amplified water levels" is misleading. You may consider shifted storm surge or similar.

We will change it to "shifted storm tides".

9,15: missing information grid 2 and grid 3

We will complement 6.4 km × 6.4 km for grid 2, 3.2 km × 3.2 km for grid 3,

9,17: is this a bi-directional coupling, or only one-directional? If it is one-directional, what are the consequences of not allowing the higher and better simulated water levels leaving the fine grid and modifying the boundary conditions that are imposed? Please discuss.

This is a one-directional coupling. The differences are only a few centimetres for the total water level. The biggest differences are in the calculation of wind surge because the resolution of the bathymetry for grid 1 is low compared to the resolution of the fine grid 4.

Fig 7 and 8: wind speeds from reconstruction are normally lower than 20 m/s, but geostrophic wind speeds can go up to 40 m/s and even higher. Can you explain this feature?

First, figures 7 and 8 are for different locations. Figures 8 and 9 are for the same location. We intended to make the reanalyses comparable by calculating the geostrophic wind (figure 8). From figure 8 we could conclude that the median geostrophic wind speed is almost the same. Geostrophic wind represents a balance between pressure gradient and Coriolis force. Surface winds are generally weaker including friction, vertical stability, and other effects. A comparison is available, for example by the WASA Group (1998).

Moreover, the differences in 10m wind speed are higher between the individual models. The models for the reanalyses use different parameterizations for the calculation of the wind speed. Especially the products from ECMWF stand out due to the lower simulated wind speed in extreme situations, see also Cavaleri et al., 2019 for the ECMWF-forecast.

The WASA Group (1998). Changing Waves and Storms in the Northeast Atlantic?. Bulletin of the American Meteorological Society 79, 5, 741-760, available from: <https://doi.org/10.1175/1520-0477(1998)079<0741:CWASIT>2.0.CO;2>

16,1-3: please also see Ferrarin et al., 2021 and Cavaleri et al., 2019 and 2020 for adding different components of the water level and the importance of timing for storm surges occurred in Venice

Thank you for the references. We will consider them in the revised manuscript.

Fig 13: it would be beneficial to also see only the storm surge component. If the plateau of the storm surge peak were quite flat, then a small shift of the timing would not have a big effect.

Here some references. The authors should feel free and not obliged to use these if they think they are appropriate.

Thank you for the references. We will consider them in the revised manuscript.

Ferrarin, C., Bajo, M., Benetazzo, A., Cavaleri, L., Chiggiato, J., Davison, S., Davolio, S., Lionello, P., OrliÄ□, M., and Umgiesser, G. (2021). Local and large-scale controls of the exceptional Venice floods of November 2019. Progress in Oceanography, 197,102628. DOI:10.1016/j.pocean.2021.102628

Cavaleri, L., Bajo, M., Barbariol, F., Bastianini, M., Benetazzo, A., Bertotti, L., Chiggiato, J., Ferrarin, C., Trincardi, F., and Umgiesser, G. (2020). The 2019 Flooding of Venice and its implications for future predictions. Oceanography, 33(1), 42-49. DOI:10.5670/oceanog.2020.105

Cavaleri, L., Bajo, M., Barbariol, F., Bastianini, M., Benetazzo, A., Bertotti, L., Chiggiato, J., Davolio, S., Ferrarin, C., Magnusson, L., Papa, A., Pezzutto, P., Pomaro, A., and Umgiesser, G. (2019). The October 29, 2018 storm in Northern Italy — an exceptional event and its modeling. Progress in Oceanography, 178, 102178. DOI:10.1016/j.pocean.2019.102178

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-325/nhess-2021-325-AC3-supplement.pdf