

Interactive comment on “An ensemble study of extreme North Sea storm surges in a changing climate” by A. Sterl et al.

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Dear referee,

Thank you very much for reviewing our paper. After the end of the discussion period for the paper, we will provide a revised version in which we incorporate your suggestions. We here answer your questions and describe the changes we plan.

Your first main comment is about the fact that we only use one climate model. It is right that this is a potential shortcoming of our analysis. We have acknowledged this in the Summary of the original manuscript (see your point 18), and, following your suggestions 1 and 18, will incorporate it in the Abstract of the revised version.

We only use one model because there are simply no other models accessible from which about 16 runs are available. The use of a large ensemble of runs to narrow

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the statistical uncertainty is the main goal of the paper. Our Fig. 4 shows that this is essential to be able to assess possible changes in the 10,000-year return value. All other known studies (see discussion in Introduction) employ much smaller datasets, usually only one realization. To the extent that the results are comparable they point into the same direction: changes in surge heights due to climate change are small. We therefore added the sentence *However, the investigations reviewed in the Introduction, although focusing on much shorter return times, back our result* to the Summary.

Your second main comment regards the ability of the models to adequately represent the most extreme winds, and you wonder about the 10% increase of the ERA-40 winds. To start with the latter: We use the ERA-40 winds only to demonstrate that WAQUA/DCM98 is able to yield reasonable results for surge heights when driven by output from a coarse resolution model. Remember that we degraded the ERA-40 winds to the resolution of ESSENCE. Use of ERA-40 winds enables a direct (i.e., event by event) comparison between model result and observations, as ERA-40 represents the true temporal development of the winds. This is explained in sections 2.3 and 3.2, and used in Fig. 3b. The 10%-factor has been chosen “by eye” to bring the level of the ERA-40 winds roughly to that of ESSENCE (Fig. 1). From Fig. 3a we then conclude that winds of ESSENCE resolution and strength lead to realistic modelled surge heights (see section 3.2). This is clearly expressed in the last sentence of section 3.2 and confirms your view that “the best justification would appear to be the sea levels in Figure 3”.

Now to the more important question whether the models are able to generate the most extreme winds. Important for surges is not an incidental very high wind gust, but the integral over the wind over a long time (12–24 hours) and a large area (sizable part of the North Sea). Therefore, we display in Figure 1 the maxima of daily-averaged wind speeds. According to this Figure the models can represent the the extreme daily-mean winds correctly. The annual maxima lie on a straight line, and there is no indication that any kind of saturation occurs. The lines for all models, which have very different resolution (e.g., 25 km for RACMO, 200 km for ESSENCE) are parallel, and the ESSENCE

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line is above that of RACMO. So even the coarse resolution ESSENCE can create winds that are higher than those obtained from the high resolution RACMO. That the lines are parallel means that the relation between *changes* in return time and *changes* in intensity (wind speed) is the same in all the models. In the original text (p 1040, lines 13/14) the word *changes* is missing. It will be added in the revised version.

Detailed comments:

1. Page 1032, Line 13: Suggest “uncertainty. In the one model used in this study, we find”.
Yes.
2. Line 23: Suggest “The plan included massive”
Yes.
3. Page 1033, Line 2: Suggest “system to be able to withstand”
Yes.
4. Line 13: This of courses misses model biases, which may be dominant.
Yes, that’s why we deliberately added the word “statistical”. To make this clearer, we added the sentence *Of course, this conclusion relies on the used model’s ability to correctly represent the impact of climate change on wind speeds* at the end of the Introduction.
5. Page 1034, line 11: Suggest “models, four were”
Yes.
6. Line 17: Suggest “is not statistically significant”
Yes.

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7. Line 21: There is no right panel. Also Figure 1 is not yet referred to.
Right. Text changed. Regarding the order of the figures: If we would exchange Fig. 2 and Fig. 1, Fig. 2a would be at an un-natural position. Splitting Fig. 2, with Fig. 2b appearing as Fig. 1 and Fig. 2a as (new) Fig. 3 would make the discussion of the present Fig. 2 in section 3.1 clumsy. We therefore decided not to change the order of the figures. The word “below” when referring to the figure is meant as a hint that we know that Fig. 1 has not been referred to yet.
8. Page 1036, line 7; Suggest “In summary, all models”
Yes.
9. Page 1037, line 22: “results from”
No. But to make clearer what we mean we changed the sentence into “This yields 17 realizations ...”
10. Page 1038, line 23: “surge height, simulated”
Yes.
11. Page 1040, lines 15-18: I think this is a weak justification that the winds are appropriate, even though I not the emphasis on wind changes.
See discussion of your second main point above.
12. Page 1041, line 5: Should this be lower rather than higher?
Sure.
13. Line 13-14: Justify the 10% factor. On line 14, should this be “to other model data sets”, and if so what about compared to real winds?
10%: See discussion of your second main point above.
Yes, model data. Added to text.
Observations: there are no long time series of maximum wind observations on the North Sea. One would have to resort to coastal stations, but comparison

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is then hampered by the fact the “coastal” model grid cells are partly land and partly sea. The rougher land part would reduce (maximum) wind speeds, making a comparison with observed point measurements problematic.

14. Page 1042, line 17: “North Sea, Figure 5”
Yes.
15. Line 25: I think relevant is the wrong word here, these winds are still relevant. Perhaps change relevant to dominant.
Yes and No. We changed relevant to important. The point is that south-westerly winds do not lead to high surges at the Dutch coast.
16. Page 1043, line 6: Suggest “not change surge heights greatly at the”
Yes.
17. Page 1044, line 12: Suggest “water level on the coast as,”
Yes.
18. Page 1045, lines 23-25: This is an important point and should be in the abstract. Following your suggestion under point 1, we added “in the one model used here” in the second to last sentence of the Abstract.
19. Figure 2, 3 and 7: Some of the lettering is barely legible.
This is due to the page size of *Ocean Science Discussions*. The height of the figures makes it necessary to reduce their size. If they fill one column of *Ocean Science*, the lettering will be readable.
20. Figure 5: Some of the lettering is illegible. What is the scale? Has the ERA winds been increased by 10% as earlier noted necessary.
Lettering: right. We'll prepare a new figure.
Scale: As stated in the caption, the figure displays fraction or relative occurrence (number of 6-hourly wind exceeding 8 Bf divided by total number of 6-hourly

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winds in the record). For the sake of the discussion around the figure, the value of these fractions is not important as we only look at differences. In order not to overload the figure we decided to leave it out.

21. Figure 9: Some of the lettering is illegible. What is the units of the scale in (a)?
Lettering: See 19. Scale is metres. Added in the caption.