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Ocean Science Discussions

Interactive comment on "The long-term variability of extreme sea levels in the German Bight" *by* Andreas Lang and Uwe Mikolajewicz

Anonymous Referee #3

Received and published: 11 April 2019

formal assessment

Does the paper address relevant scientific questions within the scope of OS? yes

Does the paper present novel concepts, ideas, tools, or data? yes

Are substantial conclusions reached? yes

Are the scientific methods and assumptions valid and clearly outlined? yes

Are the results sufficient to support the interpretations and conclusions? yes

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes

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Discussion paper

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes

Does the title clearly reflect the contents of the paper? yes

Does the abstract provide a concise and complete summary? yes

Is the overall presentation well structured and clear? yes

Is the language fluent and precise? yes

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? yes

Are the number and quality of references appropriate? yes

Is the amount and quality of supplementary material appropriate? yes

general comments

The article "The long-term variability of extreme sea levels in the German Bight" by A. Lang and U. Mikolajewicz addresses an urgent and important issue in the current discussion on extreme sea levels (ESL) and how they might evolve in a changing climate. Using a millennial long, downscaled coupled model run of the past 1000 years they show by example of the Cuxhaven tide gauge how standard approaches of ESL estimation based on decadal to centennial long time series underestimate the uncertainty inherent in the natural variability of ESL. They show that the ESL variability exhibits a white spectrum and that the atmospheric pattern related to high ESL resembles the negative phase of the Scandinavian pattern. By comparing their time series with an additional one from a model run that downscaled a run with different initial conditions they show that the trajectories are different and the statistics comparable, which implies that external forcing (e.g. solar or volcanic) is not a major driver of ESL variability. The

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large uncertainty in ESL from natural variability should have an impact on how future ESL estimates based on observational time series are interpreted.

The article is innovative, relevant and well written and should be published in OS after some minor issues have been addressed.

specific comments

- The abstract is good and might be even easier to grasp for someone not working in the field if not all abbreviations are introduced there. The NAO or SLP could be introduced later, since they are mentioned only once in the abstract.

- Could you mention briefly how those processes that are not included in the model, like ice sheet melt or land uplift might alter your ESL analysis.

- At the end of the first paragraph in Section 3.1 you mention that the long-term trend from the observations was removed. Was it a linear trend and was the residual of the fit tested for being mostly white noise?

- In the caption of Figure A2 you mention that "The respective long-term mean has been removed for both time series." and you show the MHW as a dashed line in the figure. The information from the figure and the caption might become clearer if either both the long-term mean and the MHW would be indicated in the figure or was it anyway the MHW which was removed as implied in the text in Section 3.1?

- I am not against a conservative approach to show only the 50-year return values from a 50 year long record. Others calculate up to three-fold return periods given the length of a time series. Are there reasons to not calculate the 100-year return level at Husum and Norderney?

- In the last paragraph of Section 3.1 you argue that the model is spatially coherent along the German Bight coast and in Figure A7 you show the average along the coast. I think you need to show the median with interquartile range or something similar to support that claim. It would allow the reader to assure herself that local effects and

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such are not playing an important role.

- On Page 12 you argue that the pattern of the composites are robust against minor changes in the threshold of what constitutes "high ESL". It might help the reader to integrate "minor changes" into the story a little better if those changes could be related to a number. For example mean plus 1.5 standard deviations plus/minus 0.25 standard deviations, or 80 to 120 periods of "high ESL".

- In the discussion on page 17 the non-parametric estimates of return levels are compared to various estimates from fitting distributions with certain parameters to the sampled distributions. I think this discussion would become clearer if the non-parametric estimates had been introduced in a sentence or two. How did you exactly determine the non-parametric estimates? This could also be added to the method part.

- On page 19, line 29 you mention that the GCM does not account for melting ice sheets. Is the global mean thermosteric effect included in the analyzed sea level?

technical corrections

title: Uwe Mikolajewicz

p5l10: global Earth System Model MPI-ESM

p5l18: Topography and coastlines as well as ice sheets are immutable

p6l3: with the first 100 years again used as spin-up

p6l24: fitted to the comparably short data series.

p7l30: the return values inferred from observations lie within the spread of the model simulation at all periods.

p8l3: [I am not sure myself whether "section 4" is a name and should be capitalized?]

p13l9: This pattern is different from the meridional NAO-like dipole

Figure 9: Left: Pointwise regression of the SLP index derived from Fig. 8 onto annual

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maximum sea level

p15l23: plementary Fig. A5) stresses the dominance

p17l15: The non-parametrically obtained RL1000 lies with 3.7 m within both distribution ranges

Figure 10: the green line mark the RL100

Figure A2: The respective long-term mean has been removed from both time series.

p25l11: [Same issue as above: table A1 is probably a name that should probably be capitalized.]

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