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Interactive comment on "Atmospheric Circulation Changes and their Impact on Extreme Sea Levels around Australia" *by* Frank Colberg et al.

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[10pt]article color hyperref In the text below, the reviewer's comments are shown in black, while our (the authors') responses are shown in red text.

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The duration (20 years) of the analyed time periods is lower than 30 year minimum duration often recommended. Effects of multidecadal variability could in this case hide a climate change signal that is not sufficiently strong. I think that is should be investigated wheather using longer time slices could have produced more robuts results.

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The choice of twenty-year time slices was to align the hydrodynamic model output to wave model simulations carried out using the same climate models over the same time period that was published in Hemer and Trenham (2016). Our aim was to be able to couple hydrodynamic extremes with wave-induced extremes (e.g. wave setup or runup) in future work. We acknowledge that 20 years may be too short to assess the role of future changes to interannual variability (i.e. ENSO) on weather events that cause extreme sea levels such as tropical cyclones, but as we already note, the GCMs do not adequately resolve TCs anyway so the focus of our study is on the contribution of large scale circulation changes to extreme sea levels. We feel that 20-year time slices are adequate for assessing how large scale circulation changes will affect drivers of sea levels around much of Australia's coast where seasonally varying weather systems are a major cause of extreme sea levels.

(Hemer, M. A. and C. E. Trenham, 2016: Evaluation of a CMIP5 derived dynamical global wind wave climate model ensemble. Ocean Modelling, 103, 190-203, "doi:https://doi.org/10.1016/j.ocemod.2015.10.009".)

Abstract

The abstract should state more clearly the main conclusions. I think the present one is not satisfactory on this respect. The text at lines 15-16 is too generic and not informative on atmospheric circulation changes, while they, being mentioned in the title, should be a main focus of the manuscript. The last lines mention a large increase in extreme sea level during austral summer in the Gulf of Carpentaria (note that it is difficult to locate it for those not familiar with Australian geography and it is not mentioned in the map of figure 1). However, this conclusion is rather uncertain because only 2 of the 4 models used show such increase (fig.9). Further, the abstract mentions a small reduction of sea level extremes along the southern coast. However, the four models (fig.11) agree only on a relatively small central fraction of the southern coast and on its westernmost tip. A limitation of this study is, in my view, that it is unable to identify significant change in surge extremes (there is very little agreement among models).

We removed the sentence in question. We understand that there is a limited amount of agreement between the different modle simulations and changed our wording around it, we give more detailed information regarding the SSH changes in the abstract. Overall we argue that the disagreement in responses between the differently forced model simulations and the difference in seasonally in their response is in itselft is a valuable result. It may emphasize that we need to work towards a better understanding of parametrized physics in climate models. These unresolved phyics may very well drive a large amount of uncertainty and may lead to large intra-model differences. We have changed the manuscript as to put a stronger emphasize on this aspect which we did not do so before.

We marked the location of the Gulf of Carpenteria in Figure 1 !

Introduction

page 1, line 24 it is not clear what authors mean here. Do they mean that storm surges are superimposed to low frequency modulation of sea level (forced by large scale patterns) or that the synoptic forcing of extremes is, in turn, modulated by large scale circulation patterns?

OK - We reworded this part of the manuscript and hopefully made this section more clear.

It seems to me that the author do not summarize adequately the existing literature. Page1, line 33 to page 2, line16, This paragraph looks rather incomplete to me. Only the last four lines refer to Australia. Is it reasonably complete list of available studies for Australia ? After a quick search with google scholar have found also

Yes, we agree with the reviewer and have added additional relevant references

Page 3 lines 12 to 19.Should be better explained what is new in this study. Which new information is missing and authors aim at providing?

OK - we strengthened the introduction to emphaisze what is new in this modelling study. SSH changes driven by synoptic weather changes for the whole australian coastline has not ben investigated before.

Section 2.1. which fraction of the total tidal amplitude is explained by using only 8 components?

The dominant tidal constituents are the diurnal constituents, K1, O1, P1, Q1, and S1, and the semidiurnal constituents M2, S2, N2, and S2 (Wollanksi, and Elliot, 2016). Other constituents that typically may contribute non-trivially to overall coastal tidal amplitudes include higher-frequency non-linear shallow water "overtides" and annual and semiannual constituents (which are typically due more to seasonal oceanographic and meteorological variability, rather than astronomical forcing). The ROMS model is capable of dynamically reproducing both of these types constituents (at least to some degree). We therefore follow a frequent convention used for shelf-scale models and the 8 major tidal constituents at the model boundaries.. It is not necessarily possible (or desirable) to obtain higher order tidal constituents via global tidal models.

Section2.3 the problem with introduction of tides in model adopting 360 day long year does not appear relevant because tides are not included in the climate change experiments (authors write this a few lines below)

Yes, we agree with the reviewer and removed this paragraph from manuscript.

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Page 5 , lines 11-13, RMSE, STDE and correlation are weak metrics for validation of extremes in a time-series. High correlation and low RMSE can be obtained also if extremes are poorly reproduced. Further, to validate a model percent errors should be considered, particularly for extremes. To compare magnitude of the error to the magnitude of the observed value is important.

We are aiming to compare the analyses to that given by Haigh et al, 2014a. The aim thus was to show that the model captures atmospheric driven variability generally well. We asses extremes via qq plots in Figure 4.

8 Page 5, lines 4.It is not clear to me how is the seasonal variability component defined and computed in this study

We follow the methodology of Haigh et al,2014a. The seaosnal component is calculated by using a 30day running mean over the detided and detrended time series. This removes basically the high frequency (weather driven) variability. We changed the paragraph to make this more clear.

Page 5, line 31. If the 30-day running mean is subtracted to the signal, I expect that the steric contribution on the residual is small

We are not quite sure what the reviewer means. We use the 30 day running mean to tease out the seasonal signal. This is also done in accordance to Haigh et al, 2014a.

Sea level residuals. It is not clear to me what we learn from the considered examples. What have been the criteria for their selection

The examples have been selected to illustrate the main weather systems that cause storm surges along different coastal regions in Australia. The year 1997 was selected as it contained examples of extreme sea levels along each coastal region examined.

We are adding additional references in the paper.

Page 6, lines 33-34 to blame the inaccurate meteorological forcing is often correct, but it is also an easy way out. Can the authors provide an argument to support this?

Yes, this is true. Arguably there are acouple of reasons why the model is not able to reproduce SSH anomalies correctly (1) representation error of model and atmospheric (i.e. grid resolution, bathymetry errors, coastlines not resolved properly, errors in the atmospheric forcing, limited temporal resolution), (2) model physics - a 2D model will only ever resolve the first barotropic mode of coastally trapped waves. Higher order modes may be necessary to properly account for all the variability. These points are of course generic and speculative.

Page 7, line 13. It is not clear why in this specific location wave set up is expected to be a relevant contribution and could explain the underestimated sea level by the model. This should be explained in terms of location of the gauge and morphology of the sea bottom (including depth).

We agree with the reviewer and changed the paragraph. In fact what appears to happen here is explained by the following: The missing peak can be explained by a not propoerly cpatured/ modelled coastlly trapped wave (CTW). Studies like Woodham et al, 2013 suggest speeds of CTW between 2-4m/s. CTW travel antiklockwise around Australia. It takes about 5-6days for CTW to travel the distant from port kembla to Rosslyn Bay. On the 10th may a coastal low produced a surge in Port Kembla that excuted CTW. According to Woodham et al, CTW can cause sea level elevations of 0.25m which is in the order of what has been observed at Rosslyn Bay about 5-7 days later. The ROMS model does not capture this elevation,. This may potentially be due to its barotropic nature which does not allow higher order (bariclinic) modes of CTW to develop. Unresolved bathymetric features over the Great Barrier Reef are also candiates for explaining the model behaviour.

Page 8 lines 6-11. To which figures do these sentences refer?

These sentences refer to Figure 6. We made appropriate changes in the text.

Authors consider the total sea level ZTM , Its tidal ZT and meteorological ZM components, all computed separately by independent simulation. Defining the residual ZR= ZTM - ZT, they find that peaks (ranks) of ZR and ZM agree and conclude that time-surge interaction is negligible. However, this is in contrast with the lack of agreement between ZTM and ZT + ZM , which shows that tides substantially decrease the importance of the meteorological contribution to sea level extremes. Therefore, to me it seems that tides are not relevant for computing correctly the maxima of the storm surge, but actual sea level maxima are affected (decreased) by tide-surge interaction (practically high tidal levels reduce the contribution of the surge to the maxima). Further, the whole analysis applies at the location of the tide gauge. I suspect that at the actual coastal line, at the shore, analysis can produce different results.

We agree with the assertion by the reviewer which is what we wrote in the manuscript. We were reviewing the paragraph to make this more clear.

The statement that “climate models overall perform well” is too positive, considering the tendency of all simulations to underestimate high quantiles is some locations (fig.7).Such underestimate is particularly large for inmcm (note that the annotation in the figure is not consistent with the text which refers to this simulation as CC-I). Model simulations substantially underestimate extremes at several locations.

We understand the the formulation used here may sound too positive and we changed the paragraph to accomodate the reviewers concern. However we would also like to point out that the second part of the sentence in question puts the first part in perspective: "...climate models overall perform well in terms of generating a sea level response in the ocean model for the lower percentile ranges". So we are saying what the reviewer questions in his comment. Furthermore, we go on to say that the sea level response for upper percentile ranges is on par for certain stations and not on par for other stations.

Seasonal mean maximum sea level change I find this part should be improved in several aspects 16.1) It discusses the multimodel mean at annual scale, and only individual models at seasonal scale.

The shown multimodel mean for the annual average is to compare results from CFSR forced surge model with observations and to show that the CMIP5 forced models show similar results in terms of overall distribution.

There is no indications whether changes are statistically significant for individual models. I suggest to mask in figure 11 (central panel) values when models do not agree on the sign of the change or add, anyway, an indication of the level of consensus among models.

OK – we understand the reviewers concern and added his suggestions to the image.

There is a discussion of the link of the observed changes of extremes with changes of wind speed. However, it is not clear why changes of mean speed are relevant for extremes and whether figure 10 is a multimodel mean or it represents the winds driving the CC-A simulations.

Yes, we agree with the reviewer and clarified this. Figure 10 demonstrates a possible mechanism that may explain the observed increase in extremes seen for ACCESS-R

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and HadGEM foreced model simulations. Stronger mean monsoonal winds will pile up more water over the GOC. This in turn will increase the likelihood of extremes to happen.