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Natural Hazards and Earth System Sciences
Discussions

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

Anonymous Referee #1

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This paper investigates four projections of sea levels for the Australian coast and associates them to changes of atmospheric circulation. It first validates the wave model used. Subsequently, it compares a present (1981-1999) and a future (2081-2099 with RCP8.5 forcing)time slices to identify a climate change signal. The study is not methodologically new, but it aims at providing new information for the Australian coast. My concern is that this information is apparently not very conclusive, meaning that, according to my interpretation of results, disagreement among model prevents reaching robust conclusions except for the decrease of extremes along a relatively small part of the Australian Southern coast.

The duration (20 years) of the analyzed time periods is lower than 30 year minimum duration often recommended. Effects of multidecadal variability could in this case hide a

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climate change signal that is not sufficiently strong. I think that it should be investigated whether using longer time slices could have produced more robust results.

I think (see my comments below) that this manuscript and the figures should be improved for becoming publishable.

More specific comments:

—-Abstract

1) 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).

—- Introduction

2) at 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?

3) 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

- McInnes, K.L., Macadam, I., Hubbert, G.D. et al. Nat Hazards (2009) 51: 115.

- Church JA, Hunter JR, McInnes KL, White NJ (2006). Aust Meteorol Mag 55:253–260

Are they not relevant?

The rest is for European Seas and it looks a very incomplete reference to a very rich literature, with many studies published for the North and the Mediterranean Seas. Again, just searching with scholar, I have found

Vousdoukas, M.I., Voukouvalas, E., Annunziato, A. et al. Clim Dyn (2016) 47: 3171. https://doi.org/10.1007/s00382-016-3019-5

Woth, K., Weisse, R. & von Storch, H. Ocean Dynamics (2006) 56: 3. https://doi.org/10.1007/s10236-005-0024-3

Conte D.and LionelloP. (2013) https://doi.org/10.1016/j.gloplacha.2013.09.006

Androulidakis YS et al.(2015) https://doi.org/10.1016/j.dynatmoce.2015.06.001

Lionello P.et al (2017) https://doi.org/10.1016/j.gloplacha.2016.06.012

Debernard J, Røed L (2008) Tellus 60:427–438. doi:10.1111/j.1600–0870.2008.00312.x

R.Weisse et al (2012) https://doi.org/10.1016/j.ocecoaman.2011.09.005

. . . and this list does not mean to be complete

The fact that the authors do not adequately summarize the existing literature applies also to the following paragraphs on interactions between storm surge and sea level rise and on tropical cyclones

4) 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?

——In Model description and methods

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

6) 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)

7) 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.

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

9) 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

——Sea level residuals.

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

11) 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?

12) 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).

——Tide-surge interactions

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

14) 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.

—– comparison with current climate.

15) 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.

——Seasonal mean maximum sea level change

16) 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. 16.2) 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. 16.3) There is a discussion of the link of the observed changes of extremes

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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.

——Some minor comments

The authors are native English speakers, while I am not. However, I find that the text in some could be improved. Examples in the abstract

Line 7 “short term”: Do authors mean at the monthly, annual or decadal scale? I think they mean “high frequency” here

Line 7 attendant->expected

Line 10 conditions -> observations

Line 11 delete simulation

—-Quick comments on the figures

Figure 1: station names are too small

Figure 3 , titles and axis labels not readable

Figure 4, panel should have a larger size and blank areas among them should be reduced. The lowest quantile is 0.1 and , consistently the largest is 99.9 according to the caption. However, there are blue points above the highest red point (denoting the 99.9 quantile) which high quantile are shown here?

Figure 5 arrows (wind speed) are not visible (they are too small). I suggest to add dots to mark the position of the station considered in each panel

Figure 6 caption does not describe it properly

. . .in general I think captions could be improved and describe should more exhaustively the content of figures

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