

Interactive comment on “Projected sea level rise and changes in extreme storm surge and wave events during the 21st century in the region of Singapore” by H. Cannaby et al.

We thank the reviewers for their constructive comments on our paper. Responses to each individual comment are included below (in red text for clarity).

Response to comments from reviewer #2

Specific comments:

p. 2958, line 14: “during the northwest monsoon” - you meant the northeast monsoon, per p. 2957, line 27?

Yes, this has been corrected

p. 2961, lines 21-22: when is the central estimate based on the median, and when isn't it?

For all of the time series presented in the IPCC AR5 supplementary materials the central estimate is the median value (50th percentile). However, when we computed the local steric+dynamic(now oceanographic) terms we did this by computing the ensemble mean. So the oceanographic term is based on a mean and in all other cases the median is used. This is now clarified in the text as follows:

“Time series of each of the terms listed in Table 1 have a central estimate (based on the median for all terms except the oceanographic term, for which the mean is used)”

p. 2962, lines 4-5: specifically Greenland and Antarctic ice dynamics, which should be stated at least here once for clarity

This is stated in the preceding sentence.

p. 2964, lines 1-2: Is there any way that you can add to this sentence so that it is a bit more accessible (i.e., so that the data consequences of this choice are more clear) to those who are not familiar with this particular regional model?

The sentence:

“it was necessary to modify the z-envelope (which allows sigma levels to intercept land in regions of steep topography) such that the minimum number of layers in the vertical was set to 7”

Has been replaced with:

“it was necessary to modify the z-envelope (which allows sigma levels to intercept land in regions of steep topography, thus preventing steep gradients in the vertical levels that may introduce pressure gradient errors) such that the minimum number of vertical levels at any location was 7”

p. 2968, lines 7-8: Was any testing done to see if changes to the shape within a given simulation's GEV distribution were small?

We did not investigate changes in the shape parameter in this work, however our experiments with climate-model-forced century-scale storm surge simulations for the UK have suggested little or no added value in allowing the shape parameter to change. The approach of allowing change in the location and scale parameters (but not the shape parameter) has some precedent, for example:

Butler, A., Heffernan, J.E., Tawn, J.A., Flather, R.A. and Horsburgh, K.J. (2007) Extreme value analysis of decadal variations in storm surge elevations. *Journal of Marine Systems* 67 pp189-200

Howard, Lowe and Horsburgh (2010). Interpreting century-scale changes in southern North Sea storm surge climate derived from coupled model simulations. *Journal of Climate*. Volume 23, Issue 23 (December 2010) pp 6234-6247

Zhang, X, Zwiers, F and Li, G, 2004. Monte Carlo Experiments on the Detection of Trends in Extreme Values. *Journal of Climate*. 17, 1945-1952

Zang et al (2004) state that:

“Trend in the shape parameter ξ is not considered in this study because we decided to avoid the complications that arise from allowing all three GEV parameters to vary in time. We assume that it is not likely for there to be significant change in the shape of the tails of the kinds of variables that are typically considered in climate studies over the period of record (less than 100 yr) that is ordinarily available for analysis. Situations in which the tail does lengthen, or shorten, modestly relative to the main body of the distribution can be dealt with approximately by varying the scale parameter.”

p. 2969, lines 25-26: Does this not also suggest that the interannual variability for extreme water levels has not changed very much over the projected 130 years? This should also be explicitly mentioned.

Yes, this is now clearly stated.

p. 2974, lines 7-11: This is the estimation for a possible upper limit on the changes in local sea level which I mentioned in the general comments. It is a citation from another source, but I wonder if it might be good to include a high estimate of possible (maybe at the 90% level) sea level change plus storm or wave events, in order to put a number on what could be expected by 2100 in order to plan protection measures and infrastructure. Making such an estimate is not something which I believe you must do for the paper to be publishable; it's merely a suggestion.

We do not include this information in the current paper, however, we suggest in the text that “site specific projections of future extreme still water level can be obtained by linearly combining return levels derived from tide gauge data with the sea level change projections presented in Table 3. (Tide-gauge data represent the best information available about present-day location-specific return levels, however, it is worth noting that uncertainties in the present-day return levels derived from relatively short tide-gauge records are likely to be a large component of the combined uncertainty in projected future return-level curves.) In the longer term there is potential to develop better estimates of current risk by combining model-derived information with observed time series. The skew surge joint probability method (Batstone et al., 2013) provides an approach to addressing this problem.”

----- Reference errors:

All reference errors have been corrected.

p. 2957, lines 9-10: Christensen et al., 2013 should be Church et al. 2013 line 11: Allen et al. 2010 and Penduff et al. 2010 are not in the References line 24: Maren, 2012 not in the References

p. 2963, line 19: Madec, 2008 not in the References

p. 2967, line 16: Huerta and Bruno, 2007 (not just Huerta) lines 16-17: Kotz and Nadarajah, 2000 line 17: Méndez et al., 2007,2008

----- Some correction suggestions:

All of the following corrections have been applied as suggested.

p. 2964, line 4: "For the case of the 4 GCM-forced simulations," (add a comma) lines 24-25: "In order to allow calculation of skew surge, an..." (add a comma after 'surge')

p. 2965, line 25: "Three-hourly wind data were..." (not 'was')

p. 2969, line 7: This is the next new figure referenced after Fig. 3 on p. 2960. The next numbered Fig. should be Fig. 4 (which is referenced in the following major section). Renumber and reorder the figures; this way you won't get yelled at later by the editing department.

p. 2969, line 17: 18-yr (or 18-year) line 18: I would reword "like-for-like" as "fair". Also, insert a comma after "comparison". line 20: 130-yr

p. 2970, line 18: "state of the art" I'm not fond of quotes or the use of the term 'socalled' when qualifying something. It can sound like you don't believe it is true, or that C1469 OSD 12, C1467–C1470, 2016 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper you are quoting an unnamed person. Also, this adjective is itself sometimes criticized. I would suggest removing the entire thing, as it isn't really needed to make the point.

p. 2971, line 25: comma after 'timescale' and 'pathway'

p. 2972, line 17: comma after '77%' line 28: comma after 'simulations' p. 2975, line 26: change comma after 'activity' to a semicolon