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Interactive Comment

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.

Anonymous Referee #1

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In this paper, the authors apply the method of Slangen et al. (2014) to develop local mean sea-level rise projections for Singapore, and used the NEMO surge models and WAVEWATCH III wave model to develop projections of mean and extreme sea-level changes under the moderate-emissions Representative Concentration Pathway (RCP) 4.5 and the high-emissions RCP 8.5. The authors find no significant trend in the 100-year skew surge or 100-year wave height return levels, and so conclude that mean sea-level change is the main physical factor affecting the 21st century coastal vulnerability of Singapore. While perhaps not the most exciting result, this workman-like paper seems reasonably executed and yields a finding that should be useful for adaptation planning. As my expertise is in sea-level change, not surge or wave modeling, my comments focus on that portion of the paper.

I would encourage the authors to compare their sea-level rise projections to other published projections for Singapore, such as those generated by Kopp et al. 2014 (doi:10.1002/2014EF000239) as part of their global set of tide-gauge-specific sea-level rise projections. Examination of that paper's supplementary information indicates that they have produced projections for multiple tide-gauges in SIngapore, including the Raffles Light House tide gauge. For 2100 in RCP 4.5 and 8.5, their likely ranges of 35-79 cm and 54-102 cm, respectively, are in general good agreement with the authors' projections (29-73 cm and 45-102 cm), though Kopp et al. (2014) attempts to characterize the tail risk as well (for example, for RCP 8.5, they find a 95th percentile projection of 127 cm, and 99.5th percentile projection of 193 cm, and a maximum physically plausible projection of 275 cm). Kopp et al. (2014)'s SI also provides a process-level breakdown for each tide gauge similar to the authors' Table 2.

Page 2958, line 16: The authors may find useful information for their intro on pre-observational Holocene sea-level change in Singapore from Bird MI, Austin WEN, Wurster CM, et al. Punctuated eustatic sea-level rise in the early mid-Holocene. Geology. 2010;38:803–6. doi:10.1130/G31066.1.

Page 2958, line 20: As the authors correctly note later, the dominant GIA process in Singapore is continental levering. "Glacial isostatic rebound" generally refers to the uplift of the solid Earth that happens at the former location of ice sheets.

Page 2959, line 7: RCP 4.5 can be a "mid-range estimate of expected change" only if you place a probability distribution on policy choices. It is almost impossible to obtain without deliberate climate policy and so is not comparable to SRES B2. (Note that, in the comparison study of Rogejl et al,. 2012 (doi:10.1038/NCLIMATE1385), SRES B2 had a likely warming 2.6-3.7 C in 2100, while RCP 4.5 had a likely warming of 2.0-3.0 C.

Page 2960, line 2: Work subsequent to AR5 has attempted to fill in the sea-level rise probability distribution beyond the likely range (e.g., Kopp et al,. 2014, doi:10.1002/2014EF000239, and Jevrejeva et al., 2014, doi:10.1088/1748-9326/9/10/104008), so I do not think it is accurate to say that the IPCC "likely range represents the best scientific assessment of global sea level change available at present."

Page 2960, line 9: Please spell out what is meant by a 'nearest neighbor' approach. Why was a nearest neighbor approach taken in lieu of directly calculating the fingerprints for the geographic coordinates of Singapore?

Page 2960, line 13: ICE 5G is an ice model, not a GIA model. To produce a GIA model, an ice model must be combined with a model specifying mantle viscosity and lithospheric thickness (e.g., ICE5G might be combined with the VM2-90 rheological model, yielding the GIA model ICE5G-VM2-90). Please clarify what rheological model was used with the ICE5G ice model.

Page 2960, line 22: Other authors (e.g., Kopp et al., 2014) use the term "oceanographic" rather than "steric/dynamic" (or "steric+dynamic', as it appears in Slangen et al. 2014). Neither term appears to be used in Church et al. (2013).

Page 2960, line 26: What is the resolution of the GCM being examined to estimate the dynamic sea level terms? Are there any studies using high-resolution regional models to estimate sea-surface height, with which the GCMs might be compared?

Page 2961, line 6-9: The authors linear scaling of dynamic sea-level with thermal expansion seems a weak, or at least poorly explained, point in their analysis. What is the evidence that all models show a linear relationship between local steric/dynamic sea-level change and global thermal expansion? The authors should show this evidence. But if the authors have the evidence to show this (which they should), why don't they just use the CMIP5 projections of steric/dynamic change directly?

Page 2962, line 12: "IPPC" is misspelled.

Figure 2: While the difference between SMB and dynamic fingerprints for Antarctica

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makes sense (I assume it is due to SMB being dominated by East Antarctica and dynamic by West Antarctica), the reason they are different for Greenland is less obvious. Please explain.

Table 4: Values in the text are quoted in mm/century, which are more useful units.

Table 5: There is a mismatch between the units specified in the caption (m/century) and the values quoted in the text (mm/century). I presume the latter is correct.

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