

Interactive comment on “Sea level trends in South East Asian Seas (SEAS)” by M. W. Strassburg et al.

Anonymous Referee #1

Received and published: 11 November 2014

The authors of the present paper present a study of natural variations in sea level in the Southeast Asian seas linked to the Pacific Decadal Oscillation (PDO). The investigation builds on two recent sea level reconstructions (Church and White and Hamlington) and compares multi-decadal trend maps to the patterns known from the PDO. Establishing a significant link between PDO and sea level trends (not further discussed here) the authors state that a considerable fraction of the observed decadal to multi-decadal trends in sea level can be associated with internal PDO variability (suggesting that there is no anthropogenic signature in the PDO variability). This variability has altered multi-decadal sea level trends in the past and will probably do that also in the near future, suggesting a potential short-term predictability of sea levels in the region. The paper is generally well written and it is easy to follow. However, a key weakness of the current version of the manuscript is a lack of methodological information provided here. If the authors provide a more detailed description of the points discussed in more

C1890

detail below, I think that the manuscript should be suited for publication in *Climate of the Past*.

Major Comments:

The authors use two different sea level reconstructions, which are based on a sparse tide gauge network and short altimetry SSH maps. Looking, for instance, into the availability of tide gauge records used for the reconstruction of spatial sea level in the SEAS region (Fig. 1b-f Church and White, JCLIM 2004) it is obvious that that major parts of the SEAS region are not well covered by tide gauges used for the reconstruction (at least before 1980). From this and my personal experience with these reconstructions, the coastal sea level variability in these regions is often not very well reproduced. The authors should provide a careful (region-wise) assessment whether the observed sea level at tide gauges and altimetry is well reproduced by the reconstructions. This comparison should be shown in figures showing time series, correlations, but also explained variances. So far, it is simply not judgeable whether the maps produced here provide a realistic picture of what has happened in the past (at least outside the satellite era). In my point of view the comparison of 17yr trends in each region is not enough, especially when discussing patterns of variability.

There is a lack of methodological information in the current version of the manuscript. Instead of providing a brief introduction how the 17 trend maps are compared to the PDO, the authors simply refer to Hamlington et al. (2014b), where this information can be found in the supplementary material. While I agree that for a detailed description of the method you can generally refer to another publication, you should give at least a brief overview over the major computational steps (Trend EOF's and their relation to the PDO; a figure for illustration would also be helpful) allowing the reader to follow your work without reading into the references. You have enough space to be more specific in what you have done. Additionally, although in the NCLIM paper of Hamlington and colleagues information is given regarding the statistical link between the PDO and their own reconstruction, such a link has not been established so far for the Church and

C1891

White reconstruction. This should be done here.

The authors compare 17 yr trend maps from their reconstructions with 17 yr trends from the PDO. In a just recently published study Frankcombe et al. (2014; DOI 10.1007/s00382-014-2377-0) demonstrated that the relationship between sea level and the PDO in the study region strongly depends on the time series length used for the estimation of regression coefficients. They show that at least 50 years of data are required to separate internal PDO variability from the trend. Maybe it is simply the lack of methodological information discussed in my comment above, but I think the authors should carefully discuss this point in a revision.

Minor comments:

The authors should provide more detailed information on how they calculated linear trends and their associated standard errors. The sea level time series in the region have recently been shown to be characterized by strong temporal correlations (Bos et al. 2013; doi: 10.1093/gji/ggt481, Dangendorf et al. 2014; DOI: 10.1002/2014GL060538). Did you account for such serial correlations by reducing the degrees of freedom or any related technique?

Page 4131 Line 23: Tide gauges provide sea level information since the late 17th century (Amsterdam as the first known tide gauge, see Woodworth et al., 2011 for a discussion; <http://link.springer.com/article/10.1007%2Fs10712-011-9112-8>)

Interactive comment on Clim. Past Discuss., 10, 4129, 2014.