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Discussions

Interactive comment on "Sea level trend and variability around the Peninsular Malaysia" *by* Q. H. Luu et al.

Anonymous Referee #3

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Review of Luu et al, OSD 2014: "Sea level trend and variability around the Peninsular Malaysia" The authors present analyses of mean sea level (MSL) trends and variability along the Malaysian coastline. To my knowledge, this region has only barely been investigated so far, and hence, comprehensive studies of MSL in the region are required. However, the quality of this manuscript is far below the standards of scientific publishing, which is mainly related to the poor English, but also due to missing process-based investigations providing new insights into the variations of MSL in the region. Hence, I cannot recommend the paper to be published in Ocean Science in its present form, but I would like to encourage the authors to submit an improved version after doing additional analyses and after significantly improving the linguistic style of the text. Let me explain why:

1. The language is of rather poor quality. For instance, there are dozens of articles (the, a) missing making the manuscript only hardly readable. In fact, it is currently not possible to provide specific comments, since the quality is so poor that it requires substantial rewriting. I would like to encourage the authors, to ask someone else (native speaker, or a colleague who is experienced in publishing papers in international journals) for a thorough review before resubmitting the work again in OSD or somewhere else.

2. Futhermore, the paper provides no novel insights into physical processes affecting sea level in the region. The fact that MSL in the region is highly linked to ENSO or IOD events is already well known (e.g. Nideesh et al., 2013). Also the major mechanisms (at least on a larger spatial scale) are well established (wave propagation driven by remote and local winds). What I miss is a deeper analysis (i) how such signals propagate along the Malaysian coastlines (ii) whether single events are either linked to remote or local wind forcing (and therefore connected to the different indices), or even anything else. This requires, of course, more sophisticated statistical analyses (e.g. spectral analyses) or numerical simulations (or the use of available numerical simulations). In the present form the paper is just another one showing correlations between sea level and ENSO/IOD indices. . .

3. The authors state that one region experiences "two annual cycles", while the other one shows a "single annual cycle". What you require is a more sophisticated analysis of annual/semi-annual harmonics (see for instance Plag and Tsimplis, 1999, or dozens of papers in the last $\sim$15 years dealing with seasonal sea level changes).

4. There are several statements in the paper, where it is simply unclear what the authors refer to. The most prominent example is the statement: "sea level change is highly non-uniform spatially and temporally, mostly due to local adjustment to the global warming. . .". Yes sea level is spatially and temporally highly variable, but the physical explanations are gravitational and hydrological effects, local and remote atmospheric forcing and resulting barotropic and baroclinic adjustment processes in the ocean, ver-

tical land motion, local anthropogenic interventions such as embankments, etc.. How these effects have an anthropogenic signature is another question…At another stage the authors refer to “wind stress curl blowing from”. It is new to me that the wind stress curl blows. Such statements cast doubt on the technical understanding of physical processes.

5. I was wondering about the gap-filling procedure. The authors state that they use the ENSO index in combination with atmospheric reanalysis data (wind stress) via multiple regression to fill existing gaps. I suggest using nearby stations (first) for gap filling results in much better reconstructions. There are two reasons for that: First, a multiple regression will not/or only poorly account for non-linear local effects, which are probably much better captured by nearby stations, which are affected by similar mechanisms. Furthermore, you assume that sea level only responses to atmospheric forcing, which is not the case. Second, the quality of much reanalysis data sets in these regions are only of poor quality due to missing observations required for data assimilation. I would like to encourage the authors to be more careful with such approaches, or at least demonstrate in a more detailed and convincing manner how the filling procedure works.

6. The authors provide no information on how the trends and their uncertainties are calculated. Do you use OLS estimates with standard errors assuming no significant autocorrelation of the residuals? Please provide more detailed information on that point and ensure that your assumptions are valid. In fact, you have to account for the serial correlation present in sea level records: at least due to a reduction of the number of degrees of freedom, as for instance described in Santer et al. (2000).

References Plag and Tsimplis (1999) Temporal variability of the seasonal sea level cycle in the North Sea and Baltic Sea in relation to climate variability, Global and Planetary Change Santer, B. D., T. M. L. Wigley, J. S. Boyle, D. J. Gaffen, J. J. Hnilo, D. Nychka, D. E. Parker, and K. E. Taylor (2000), Statistical significance of trends and trend differences in layer-average atmospheric temperature time series, J. Geophys. Res.,

105(D6), 7337–7356, doi:10.1029/1999JD901105. Nidheesh et al. (2013), Decadal and long-term sea level variability in the tropical Indo-Pacific Ocean, Climate Dynamics

Interactive comment on Ocean Sci. Discuss., 11, 1519, 2014.