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

Anonymous Referee #2

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Overall assessment:

The research topic in this study is sea level behavior around Peninsular Malaysia on seasonal, interannual, and decadal time scales. The investigation makes use of anomalous sea level time series from tide gauges and satellite altimetry along with vertical land motion trends derived from GPS-station data. The authors relate the sea level behavior to various climate phenomena (i.e., seasonal monsoon, El Niño-Southern Oscillation, Indian Ocean Dipole) by means of a correlation analysis. For context, they compare to sea level trends in adjacent coastal regions as well as rates of global-mean sea level change. A main conclusion seems to be that accounting for vertical land motion is important in sea level studies based on tide-gauge data.

I will grant that the research topic is an important one: an understanding of regional

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sea level change is of interest to coastal communities; on these grounds, the paper is warranted and sufficiently motivated. However, I have very serious reservations; given the poorness of the analysis, interpretation, and communication (all detailed below), I cannot recommend publication of this paper. I submit that either a very major revision should be undertaken or this paper should be rejected.

Major comments:

The English usage is very poor. There are confusing word choices, far too many to count, some of which can seriously impede the reader's understanding. [e.g., What is the mechanism meant by "local adjustment to the global warming" on p. 1521 l. 20? How can the (vertical component of the) wind curl, a scalar quantity, blow from the Andaman Sea toward the Malacca Strait (p. 1527 l. 7)? The suggested mechanism on p. 1528 ll. 1-2 ("a combination of signals from atmospheric teleconnection feedback and oceanic lateral fluxes") is so vague and general as to border on tautology. By "quasi-periodic annual cycle" on p. 1527 l. 3 do you mean "semiannual cycle"?] I would recommend the authors consult a native English speaker who would give a very critical and thorough assessment of the paper.

Interpretations are physically unenlightening. The paper seems to boil down to the authors saying that "the correlation is such-and-such" and "the trend is so-and-so" with not much given by way of physical elucidation. Frequently, the authors "explain" things by appealing to ENSO or IOD, attaching causal verbs, for example "ENSO determines" (p. 1520 l. 8), "ENSO affects" (p. 1520 l. 20), "IOD modulates" (p. 1520 l.21), "ENSO alters" (p. 1523 l. 14), "IOD affects" (p. 1523 l. 16), and so on. Such "explanations" are problematic, not least because, as statistical indices and not physical mechanisms, ENSO and IOD cannot determine or affect or modulate or alter anything! [Alternatively, relational verbs (e.g., correlated with, associated with, linked to, etc.) can be used instead in these cases, as is exemplified on p. 1520 l. 23.]

There are clear methodological mistakes. For example, it is obvious that errors have

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not been correctly propagated in Table 1. Namely, errors in absolute SLR rate from tide gauge look to have been computed simply by summing the error in relative SLR rate and the error in VLM rate. This is not correct. The absolute errors should be propagated by taking the square root of the sum of squared relative and VLM errors. Conclusions based on these numbers are therefore suspect.

Some of the reasoning is invalid (i.e., conclusions do not follow logically from premises). In many places, two sea level trends are compared, and the authors claim that one is greater than or less than the other (see most of section 3.3). However, given the uncertainties, such claims are unwarranted and generally meaningless. As just one example (for others see elsewhere in section 3.3 or section 1), the authors quote a SLR rate in the Malacca Strait of 2.4 ± 1.6 mm/yr (page 1528 line 26) and in the Singapore Strait of 3.2 ± 1.2 mm/yr (page 1528 line 27); given the overlapping error bars, one cannot say (at least not with any statistical meaningfulness) that the former is "lower than" the latter, as the authors do. [Relatedly, it is unclear what the error bounds represent or how they are computed. Are they standard errors from a least squares linear fit? Or perhaps 1.96 times the standard error (i.e., the 95% confidence interval)?]

Methods and materials are not sufficiently justified or explained. What is meant by "research-quality tide gauge data" (p. 1524 l.10; cf. p. 1524 l. 18)? Do you mean Revised Local Reference (RLR)? If so, say so. Also, is the tide-gauge data corrected for isostatic response to barometric pressure (i.e., inverted barometer)? What AVISO product are you using? [AVISO (2013) is not a proper reference.] Are you using along-track data or a gridded product? What corrections are applied to the altimetry-based product? How is the altimetry data for Figure 4 chosen? Do you use all along-track data within some radius around the tide-gauge station? Or do you use the grid point nearest the tide gauge site from a gridded product? Why do you use a somewhat-dated GPS VLM estimate [Simons et al. (2007)]? Much-improved GPS-based VLM estimates have become available in recent years (e.g., Santamaria-Gomez et al. 2012).

Other comments

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The comparison between sea level from radar altimetry and tide gauges is too cursory and needs to be discussed more. Namely, there are clear interannual differences between the respective red and black curves in Figure 4. Why is this? Is it likely that these differences reflect errors in the altimetric retrievals close to land? Or could it be that the tide gauge data reflect highly localized small-scale processes (e.g., coastal currents, eddies, response to river runoff) that are smoothed out by the footprint of the altimeters? See, for example, Vinogradov and Ponte (2010,2011) for discussion of some of the issues that need to be kept in mind when comparing time series from altimeters and tide gauges.

More references need to be given with respect to vertical land motion. To the point, it is simply false to claim that "present in situ estimate (sic) of global SLR rates mostly rely on the GIA . . . (Church and White 2006,2011)" (p. 1529 ll. 16-17). See, for instance, Woepplmann et al. (2007,2009) and the dozens of more recent papers that have cited these works.

References

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