

Interactive comment on “Technical Note: Mean sea level variation in the Singapore Strait from long-term tide data” by P. Tkalich et al.

Anonymous Referee #1

Received and published: 16 July 2012

The present paper analyses the mean sea level variations in the Singapore strait (SS) on seasonal to interannual time scales over the period 1994–2007 with tide gauge data. Although the authors claim they are also using wind data from the NCEP reanalysis and sea level data from the Topex/Poseidon Altimeter, we could not find any use of these datasets through out the paper. To estimate the SS mean sea level, the authors use a single tide gauge record: the record of the Tanjong Pagar (TP) station. They mention 3 other tide gauge records in the “data and methodology” section but these records are not used to estimate the SS mean sea level. The 3 extra records are compared to the TP record over the year 1998 to check if the TP record is contaminated by vertical movements.

This paper is dealing with an important issue: the estimation of the sea level variations

C711

in the Singapore Strait and more generally in the Indonesian region that links the tropical Pacific Ocean to the tropical Indian Ocean. The topography and bathymetry of this region are complicated and make it difficult to estimate the local mean sea level with in-situ or remote sensing techniques. But this is an important task to do because it provides some information on the ocean dynamics in this region and on the exchanges between the Indian and the Pacific Oceans. For this reason the present article is addressing an interesting problem which is also original because very few studies have analysed the sea level variations (at least at interannual time scales) in the Singapore Strait before. Unfortunately, the estimation of the SS mean sea level provided here presents two major flaws that make the conclusions of the study questionable and unreliable.

The first major flaw is that the authors used only one tide gauge record (the Tanjong Pagar tide gauge) to estimate the SS mean sea level between 1984 and 2007 while there are many other sources of sea level data in the SS during this period. We have found in the PSMSL, 6 tide gauge stations located in the SS. Some of them, like the “Raffles Light House” or the “Sultan Shoal” have records that cover the whole period 1984–2007 and could help to compute a more consistent SS mean sea level or to validate the TP record. Others like the “West coast” record and the “Jurong” record cover several years among the 1984–2007 period and provide also interesting information to validate the other records or to test the estimation of the SS mean sea level estimation. Some altimetry data is also available in the SS strait: 2 tracks of Envisat and 2 tracks of the series Topex Jason1/2 (track n° 77 and 242 for Jason 2 for example) are crossing the SS and provide validated sea level data in the SS. All this additional SS strait sea level data should help to construct and validate a reliable SS mean sea level. Instead, the use of a single tide gauge record to estimate the SS mean sea level (like in the present study) casts doubts on the estimation: what if the record is contaminated by shifts due to seismicity or instrumental changes? What about the motion of the ground on which the TP tide gauge is fixed? How can we be sure that the TP record is not representing some local and coastal phenomena that are not representative of the SS mean sea

C712

level variations (like wind-forced coastal trapped waves, local flooding events. . .). The only way to overcome this problem is to compare the TP record to other sources of sea level data over the whole period 1984-2007. This is done in the article only over the year 1998 which is definitely not sufficient. Much more should be done here (at least a comparison at seasonal to interannual time scales over the whole period 1984-2007 with the “Raffle light house” record and the “Sulan Shoal” record which cover the whole period).

The second major flaw of this paper is the treatment of the TP record for gaps and any type of contamination. The method used to fill in the gaps of the TP record is not clear: how do the authors expect to reconstruct the TP record at interannual time scales with a method based on tidal harmonics? The method used to correct the TP record for vertical movements is incomplete. The comparison of different tide gauge records over a given year can show the presence of any outlier or contamination from seismicity in this year but what about the other years? Moreover this method based on a comparison over a single year does not allow for the detection of any slow subsidence of the tide gauge station due to natural effects (tectonics or sediment compaction for example) or anthropogenic activity (fluid pumping for example). Consequently the TP record, as it is computed here, could hold signal coming from many sources other than the sea level variations (ground motion signal related to local subsidence for example). It makes all the conclusions unwarranted. A good amount of work is needed here to improve and consolidate the treatment of the tide gauge record.

Although the subject addressed by this paper is interesting, the 2 major flaws found in the method developed by the authors make me think that the conclusions of this study are not reliable and can not be published in this form. I think that a pretty big amount of work is still needed to improve and consolidate the method developed by the authors (in particular in terms of comparison with other sea level data and improvement of the treatment of the tide gauge time series). For this reason I would recommend to reject the paper. Nevertheless I think that this work is interesting and is a good start. I

C713

encourage the authors to improve their method and to resubmit when it will be mature.

Hereafter I have gathered the detailed comments on the paper. Before getting into these details, I would like to draw the authors' attention to the references. A paper submitted to high standards journals such as Ocean Science should not present such an incomplete reference section. Many references cited in the text are lacking in the reference section (Michael and Andrew 2008, Pavel et al. 2011, Pavel et al. 2012 . . .) This is not acceptable.

Note that the lines and the pages indicated below refer to the friendly printed version of the article

-page 2256 line 22: the assertion that “1.6 mm/yr” over the period 1984-2007 matches the global sea level should be referenced. Moreover I do not agree with this assertion and I would be interested in reading a reference that gives such a value for the period 1984-2007. If we compute the global mean sea level trend over 1984-2007 from the Church et al. 2006, 2011 data for example, we find 2.2mm/yr which is significantly higher than 1.6 mm/yr

-page 2256 line 25: Sea level variations are also caused by changes in the temperature of the ocean under-surface and deep layers.

-page 2257 line 1: which several studies? References are lacking here

-page 2257 line 8: the reference Michael and Andrew 2008 does not appear in the reference section

-page 2257 line 21: there are much more recent studies that estimate long term sea level trends: Church et al. 2011, Ray and Douglas 2011, Hamlington et al. 2011, Meyssignac et al. 2012. . .

-page 2257 line 28-29: I do not agree at all with this assertion. Chambers et al 2002 and Church et al. 2004, 2006 never claim that there are some links between the large inter-annual variability in the Tropical Pacific and Indian Ocean region and the rising

C714

trend of the global mean sea level. Chambers et al. 2002 does not even compute the trend of the global mean sea level in his study. This sentence should be clarified or removed.

-page 2258 line 1: the reference Pavel et al. 2011 does not appear in the reference section. I could not find it on ISI web of science either. It is regrettable because this study is supposed to be the ground basis for the present article.

-page 2258 line 26: a reference is lacking here

-page 2259 line 2: the authors indicate that there is 8 tide gauge stations in the SS but then they use only a single one to compute the SS mean sea level. Why? At least the records should be cross-checked on the longest period possible to find any outliers, shifts or contamination in the data and to estimate the shared variability.

-page 2259 line 5-7: Why does the TP record provided here ends in 2007? On the PSMSL website the TP record goes until 2011 and presents an interesting interannual variability between 2007 and 2011. The authors should justify why they use a short record which ends in 2007.

-page 2259 line 7: the authors say they fill in the gaps of the TP record using tidal harmonics. Looking at the TP record on the PSMSL website it appears that in 1995 some data are lacking for at least 6 month. How do the authors expect to reconstruct the sea level signal at seasonal to interannual timescales with tidal harmonics? They should clarify their method and explain it in details because it seems a priori impossible. I recommend as well that the authors show a plot with the raw data (monthly time series for example) to enable the reader to make his own opinion on the gaps and the variability contained in the raw record.

-page 2259 line 10: Why introducing the NCEP data and the Topex data since it is not used in this study? Additionally, when introducing datasets, the sources should be mentioned: which NCEP reanalysis do you want to use? Where does the Topex data

C715

come from, AVISO?

-page 2259 line 12: Why performing a harmonic analysis? Table 2 is never commented in the text or compared to other results. It does not lead to any conclusion.

-page 2259 line 14: the tide gauge data could be contaminated by vertical movements which come from other sources than the one mentioned here: seismicity, tectonics, instrumental changes (the TP tide gauge has been changed in 1997), ground water pumping, sediment compaction etc. . .

-page 2259 line 20: the fact that there is no abnormal value in the 4 tide gauge record during the year 1998 does not mean that it could be the case for the other years. The comparison should be done for the whole period. Moreover this comparison technique only allows for the detection of shifts in the records. It can not detect spurious trends that come from ground subsidence for example. This point should be investigated.

-page 2259 line 24: the reference Pavel et al. 2011 does not appear in the reference section

-page 2260 line 1-7: I understand that the wind dataset has been used in the previous study (Pavel et al. 2011). But what has been done here with this dataset?

-page 2260 line 15: the reference Pavel et al. 2012 does not appear in the reference section

-page 2260 line 24: what do you mean with "oceanic features". Please describe and justify

-page 2261 line 1: I understand that Figure 3 is based on the TP record only. Some work is needed to justify that TP gives a reliable estimate of the SS mean sea level (see the beginning of this review)

-page 2261 line 11: to justify the impact of El Niño, you could compare the TP record with a Niño index

C716

-page 2261 line 17-18: this sentence is inconsistent

-page 2261 line 21: You should justify with a computation or a reference that the 1.6 mm/yr matches with the global sea level rise. I do not agree with this assertion. If you compute the global sea level rise over 1984-2007 with Church et al.2006, 2011 data (available on the web) you will find 2.2 mm/yr which is significantly higher than 1.6 mm/yr.

-page 2262 line1-3: I do not understand this sentence

-page 2262 line 26: the use of a single tide gauge record does not allow to estimate the time-evolution of the MSL variability of the SS. You should use more data to compute your estimation or validate it.

Interactive comment on Ocean Sci. Discuss., 9, 2255, 2012.