

Interactive comment on “Reconstruction and Projection of Sea Level Around the Korean Peninsula Using Cyclostationary Empirical Orthogonal Functions” by Se-Hyeon Cheon et al.

Anonymous Referee #2

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This MS is trying to present a sea level reconstruction method targeting on the marginal seas around Korean Peninsula. Understanding the regional variations and therefore attempting to reconstruct/project are interesting research areas in sea level science. Unfortunately, there are many key issues in this version of ms. The paper is tedious to read with loosen focus, and in some places it is hard to understand. Ultimately, I think the presentation is needed to improve. A major revision is suggested before getting this paper published in OS.

Some key questions are provided first, followed with some minor comments.

Title and abstract: sea level projections seem to be an important aspect in this paper.

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Unfortunately, there are no results related to ‘projections’.

Introduction: authors believe that the sea level reconstruction using SST provides better results than the conventional methods (using TGs). However, SST was also sparsely observed in early years including ICOADS. How well do the SST methods cope with this common concern? Clarification is needed. I still believe the wind stress and local surface currents are dynamically important for sea level variations, like many studies have shown. There is no direct link between coastal sea level and SST in open ocean. How possible to include other dynamical factors?

Section 2: this part reads loosen and tediously long, and many parts are unnecessarily mentioned with many times. I would suggest shortening this section with concise contents to avoid readers losing interests.

Section 3: This section again is not properly presented. Essential questions: 3.1: I do not think the following key question is answered. ‘To reason whether the extreme trends patterns was related to the local mass distribution caused by various sources such as vortex and river discharge or was an independent...’ The extreme trends on China coasts in Fig 2 are proposed as a result of increasing river discharge by the authors. However, there is no convincing evidence supporting this. (one would not expect that river discharge can cause sea level increase on north Chinese coasts, because it is drying over recent years in this region). Same for the ocean current impacts. Can authors provide evidences supporting this (P10 Lines 28-31)?

Also, I cannot see any point of separating the regions with local correlations $</>0.5$. Because the two regions are both located in Yellow Sea and Japan Sea, the regional averages are supposed to not contain local information, and they instead reflect the large-scale variations. This might be reason why the two series in Fig5 are always highly correlated.

For the correlation map e.g. Fig4 (and Fig 6), is the annual cycle removed? Removing the seasonal cycle is critical. Otherwise, they are always statistically correlated but it

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does not make any sense. Need to clarify.

How can the sea level records between TG and AVISO be correlated e.g. Fig 6 when also having linear trends? If linear trends exist, they are always correlated. Correlation is for assessing the similarity between detrended variability/anomalies but cannot be used for assessing the trends. The basic concept I think is wrong. Please clarify.

Fig 6 & 7: how far are the AVISO sites from TG stations? Fig8 & 9: I cannot see there is a trend in the PC series of Fig9. What are the trend value and its significance level? Does it agree with the values based on the local estimation i.e. Fig 3. Because there is no annual cycle signal in Fig9, there is no need of presenting it with 4 seasons.

Section 3.2: what are the reasons for COBESST2-NWP having best correlations with sea level? Do author have interpretations? Why does not the local SST do better job than others? Also, the short names e.g ReSLA-NWP are not used in figures, which however use the long name. Authors need to be careful for the presentation throughout the whole paper.

Again in Fig 14 & 15, are the linear trend and annual cycle both removed before calculating correlation? Are the trends in Fig 14b statistically significant? 'these detailed fluctuations are closer to the actual sea level variability': what is the actually sea level variability?

Authors seem to insist that the SST-based reconstruction shows better results. What are the reasons for that? In the marginal seas of NWP, many studies have shown that the local ocean surface currents and wind stress determine the sea level, and the open ocean in far-field has less impacts. However, this paper finds the (far-field) NWP SST can 'statistically' better capture the sea level in marginal seas of NWP i.e. KP. What is the science behind it? Please keep in mind that the sea level variations between the two sides of western boundary currents (Kuroshio/Oyashio) are very differently forced e.g. by the thermalsteric height and open ocean currents via geostrophic balance and by local wind/surface currents.

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More essentially, this paper is focusing on 'reconstruction capability', but it spent a lot space in section 3.1 comparing TG and AVISO. Authors should work properly to make the presentation and structure of this paper concise and focused.

Conclusion: What is the linear trend map of reconstructed SLA-KP over satellite era? Are they comparable with Fig 2? How are the SST variations looking like over this region/NWP? Does SST follow the sea level changes very well?

Minor comments. For example: P2, lines 6-7: do not understand. What does bias mean? Also, references are needed to support this statement. P2, lines 29-31: references? P2 lines: 31-32: do not understand. P2 line 33: this needs to reword P3 lines 14-15: what reconstructions? P3 lines 11-22: references? P3 lines 11-27: the focus/motivations are loosen and not concise. P4 lines 9-10: do not understand Figure 1 is not readable Figure 1 seems to have 3 TGs on China coasts, while there is only one appearing in Figure 3. Any flags applied?

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