Response to the major comments

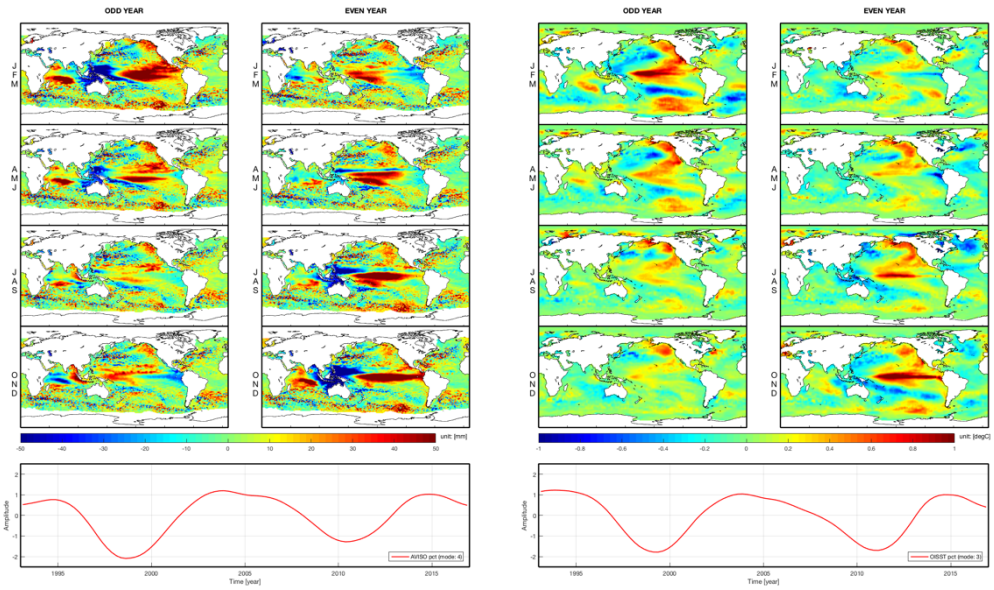
We thank the reviewer for the comments and insight into the paper. We have made many adjustments to the paper and have added clarifications where necessary. We think the paper is much improved as a result.

1. Title and abstract: sea level projections seem to be an important aspect in this paper. Unfortunately, there are no results related to 'projections'.
The reviewer is correct – we have removed 'projection' from the title.

2. 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.
We do not necessarily believe that a reconstruction of SST will yield better results than a reconstruction using tide gauges. If we had a large number of high quality (long) TG records, the reconstruction using TG data would likely be the best. Unfortunately, TG data around the KP is less than 10 prior to mid 1960s. As a result, we have used SST data instead of TG data for the basis of our reconstruction. The question the reviewer raises is a difficult one to address in any reconstruction attempt – specifically, how accurate is the reconstruction in the past. To address this question, we have used several different cases, using different datasets and different areas of study. Through these test cases, we attempt to validate the reconstruction back through time, but certainly expect the reconstruction to be of lower quality towards the beginning of the record. We have tried to make this point clearer throughout the paper.

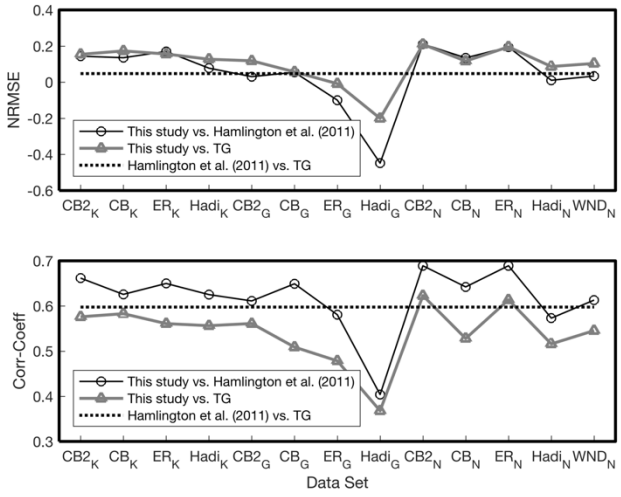
I still believe the wind stress and local surface currents are dynamically important for sea level variations, like many studies have shown. There is no direct link between coastal sea level and SST in open ocean. How possible to include other dynamical factors?

It is true that other variables could be used to reconstruct sea level. We have relied on SST in this case due to the availability of a long record and reasonably consistent measurements back through time. It should also be noted that we are not requiring a strong physical relationship between SST and sea level. Instead, we are requiring that there is a strong statistical relationship between the CSEOF modes of the two variables. This strong relationship has been demonstrated when we analyze both the SST and sea level data through CSEOF analysis (Hamlington et. al., 2011, 2012a). Actually, many CSEOF modes shows great agreement, e.g., ENSO and PDO (see, below figures. LHS and RHS are ENSO modes of AVISO and OISST, respectively; c.f. we just introduce these to show the example of their relationship)



As explained in 2.2.1 and 2.2.2, we tried to find the lagged relationship between PCts (SST and sea level), and indeed we could likely only physically interpret a few modes. Even if we cannot explain the exact physical background of each modes and their relationship in SST and sea level, the reconstruction results are still valuable. On a basic level, the only goal is to reproduce the PCts back through time. As long as the statistical relationship we have established between SST and sea level holds, the two can be used in tandem to reconstruction sea level.

We have attempted a reconstruction case using wind data in the place of SST, but the result is not good when compared to SST (see below figure). Other variables could be used, but it is beyond the scope of the current paper.

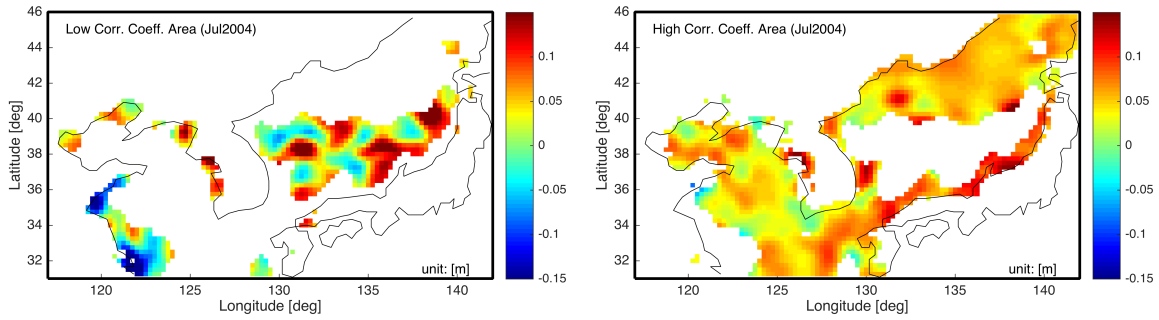


3. Section 2: this part reads loosen and tediously long, and many parts are unnecessarily mentioned with many times. I would suggest shortening this section with concise contents to avoid readers losing interests.
We have trimmed this section and removed repeating text.

4. Section 3: This section again is not properly presented.
We have modified this section.

4.1 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)?
We agree with your comment. There are no studies that explain the relation between the sea level rise trend and ocean current (or river discharge). To explain the relationship, we need more research that is beyond this paper's boundary. This was largely speculative and as a result, we have simply decided to remove this part of the paper.

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.
This study's basis assumption is the SLA-KP can be represent with GMSL as the baseline and our reconstruction as the variability about that long-term trend. And we wanted to know the influence of the extreme trend zones to the mean sea level anomalies in KP. If the extreme zones had significantly different MSLA with the other zone showing normal SLR trends, we needed to conduct reconstructions separately, as shown below figure.



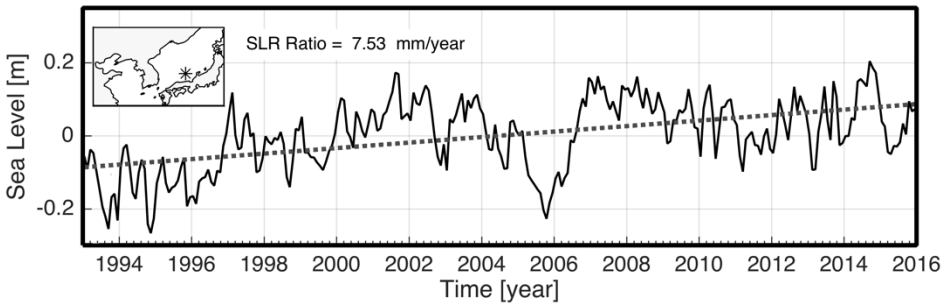
So, to separate out the extreme trend zone, we estimated the averaged correlation coefficient maps. As the reviewer said if the large-scale variations reflect the two separated zones' MSLA. That means we don't need to worry about the different trend zones.

In summary, we conducted these process to estimate the extreme trend zones influence to the MSLA-KP.

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 does not make any sense. Need to clarify.
Yes, we removed the annual cycle. We have clarified it on the figure captions.

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.

We removed linear trends during the calculation of correlation coefficient. In this case, however, the linear trend represents very small variance compared to the fluctuations, so the trend has litte effect on the correlation coefficient values (see below figure). Regardless, the trend has been removed.



The trends and correlation coefficients in Fig. 6, actually, calculated separately, there were several figures before. To reduce the figures, we combined the information and put the information in one figure. See the below figures.

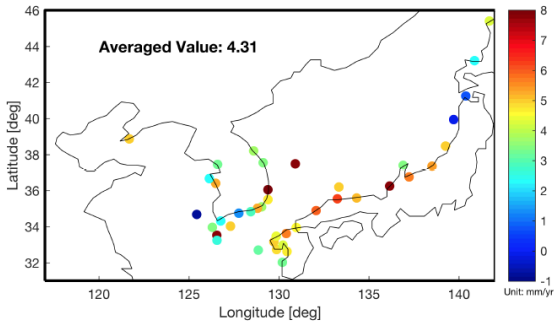


Figure 3. SLA linear trends over 1993-2013 using TGs_KP

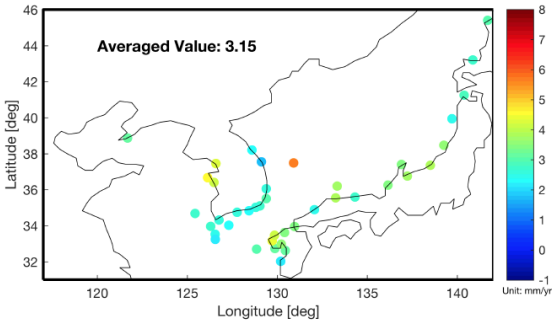


Figure 4. SLA linear trends over 1993-2013 using AVISO_sla

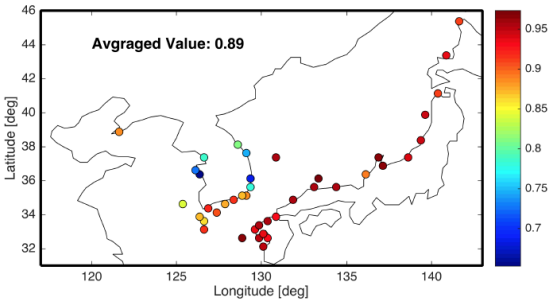


Figure 6. Correlation Coefficients between TGs_KP and AVISO_sla points with seasonal signals

Please clarify. Fig 6 & 7: how far are the AVISO sites from TG stations?
I clarified the maximum distance (about 12 km) at the caption.
In Fig. 7, but, to calculate MSLA of AVISO, I just used entire area not the closest point.

Fig8 & 9: I cannot see there is a trend in the PC series of Fig9.
We have changed the Fig.8 and 9 to show bigger PCT. We think the trend is now apparent.

What are the trend value and its significance level?

To help readers' understanding, we have added one more figure. Fig 12 shows the linear trend values of each mode and their confidence intervals.

Does it agree with the values based on the local estimation i.e. Fig 3.
Fig 3 doesn't have annual signal. If you see Fig 11, the wiggled signals arise from the summation of each CSEOF modes. So, it does agree with MSLA-KP in terms of low-frequency signal.

Because there is no annual cycle signal in Fig9, there is no need of presenting it with 4 seasons.
The spatial pattern of one CSEOF analysis is not a single map, so they need to represent through their nested period. Actually, most of the CSEOF mode do not have similar spatial patterns though the time evolution. The reason why we can determine the 2nd mode as the trend mode, is these spatial patterns are pretty similar though the nested period. In other words, the non-periodic nature of the trend is represented by the CSEOFs as a spatially constant map.

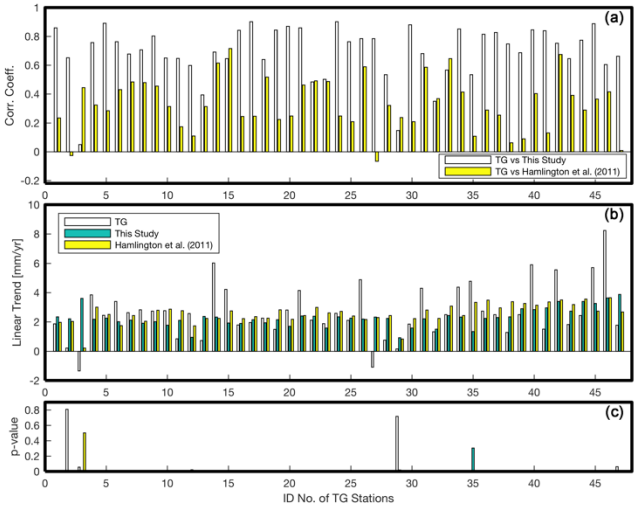
4. 2 Section 3.2: what are the reasons for COBESST2-NWP having best correlations with sea level? Do author have interpretations?
We speculate that since Japanese researchers made the COBESST-2 data, it is possible that there is improved data input or calibration for the region. As a result, this dataset appears to have more accurate results for NWP area. However, we have no studies to support this speculation, and merely state that it is the best case.

Why does not the local SST do better job than others?
We thought that SLA-KP is influenced larger scale variability that is also expressed outside of the immediate region of the KP. The best reconstruction will likely result when considering the domain that most nearly captures the physical processes impacting sea level around the KP. It is found that this domain is larger than the immediate area around the KP but smaller than a global domain.

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.
We have checked the shortened variables and corrected them.

Again in Fig 14 & 15, are the linear trend and annual cycle both removed before calculating correlation?
Yes, we removed trend before calculating correlation coefficient.

Are the trends in Fig 14b statistically significant?
Most of the trends are statistically significant. But some of them are not. We have added the p-test result.



‘these detailed fluctuations are closer to the actual sea level variability’: what is the actually sea level variability?

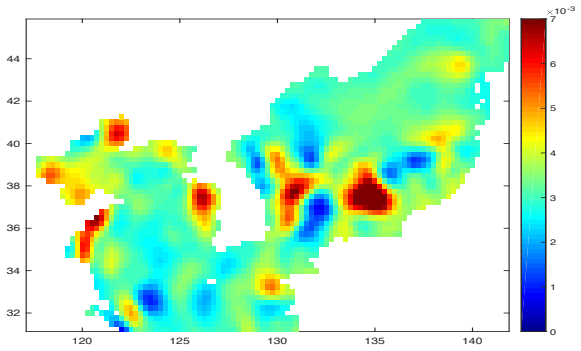
In this case, ‘the actual sea level variability’ means AVISO-KP – basically the sea level coming from actual observations. We have clarified this in the manuscript.

Authors seem to insist that the SST-based reconstruction shows better results. What are the reasons for that?
The main reasons that this reconstruction shows better results are 1) the use of CSEOF, 2) the incorporation of SST, and 3) the domain consideration. As we mentioned in the paper, the other reconstructions were not specifically focused on the SLA-KP. In a global reconstruction, there is a high possibility of omitting some important modes in certain local scale reconstruction. Using the CSEOFs, we are also attempting to account for periodic behavior that is difficult to capture with EOFs, and while also trying to represent potential lagged relationships between each basis functions.

In the marginal seas of NWP, many studies have shown that the local ocean surface currents and wind tress 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.
This reconstruction was conducted simply by extending the PCTs of AVISO-KP CSEOF, using SST as a statistical proxy. As mentioned above, we cannot explain most of the CSEOF modes in a physical manner. Indeed, other reconstructions have the same problem. At beginning stage, we tried to understand the background mechanism and the relation between the factors which you mentioned above. This remains a challenge, particularly given the complicated dynamics that the reviewer correctly points out. We have attempted to convey that SLA-KP is related with many factors: ocean currents, thermal expansion, global sea level rise, wind and so on, and it is expected SST-KP cannot cover each these factors. By ensuring that this variability is in some way captured by the CSEOF modes we use (even if we cannot say in which mode it resides), we should still be able to represent it in our resulting reconstruction.

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.
We have omitted unnecessary parts.

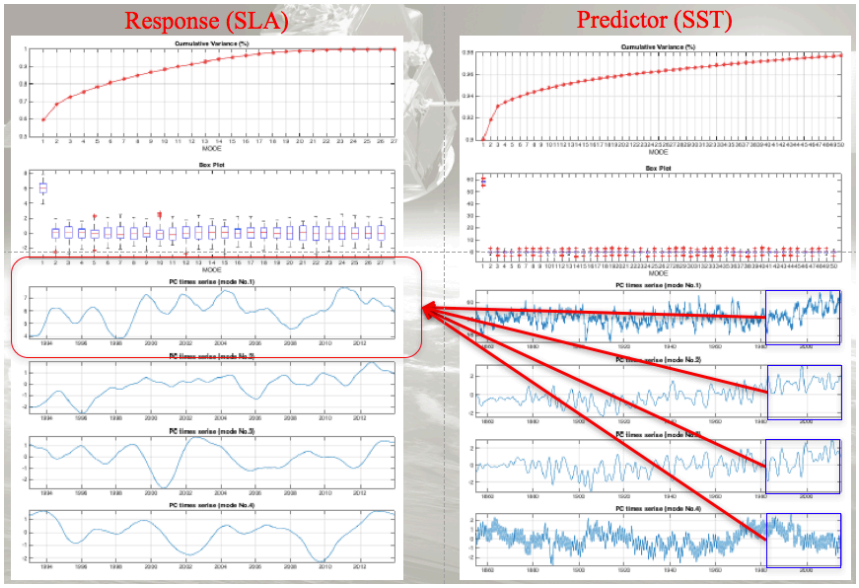
Conclusion: What is the linear trend map of reconstructed SLA-KP over satellite era? Are they comparable with Fig 2?



We calculated the linear trend map of ReSLA-KP over the satellite era (1993-2014). Even a time period is not exactly same but the result agrees with the AVISO-KP's trend map.

How are the SST variations looking like over this region/NWP? Does SST follow the sea level changes very well?

We are unsure what the reviewer is exactly looking for with regards to an explanation of the SST variations of SST-NWP. But, as I showed you above, the relationship between SST and SLA is strong in terms of the CSEOF description. The figure below attempts to explain this process.



Minor comments. For example:

P2, lines 6-7: do not understand. What does bias mean?
It means that most of the TG stations are located on the Northern Hemisphere. We have clarified this.

Also, references are needed to support this statement. P2, lines 29-31: references?
Instead of reference, we have added global linear trend map.

P2 lines: 31-32: do not understand. P2 line 33: this needs to reword
We have rewritten this part.

Original:
Properly planning for future sea level change requires an assessment of sea level on local or regional levels, as future sea level for one location could be quite different than future sea level in another location. Rather than using a global reconstruction, several studies have instead focused on regional reconstructions of sea level, targeting a specific area of focus.

Modified
Therefore, it is necessary to assess the sea level changes in local or regional level for planning for future sea level accurately, as local sea level changes can have significant differences with the global sea level rise. Several studies have focused on regional reconstructions targeting a particular area of interest.

P3 lines 14-15: what reconstructions?
Global reconstructions by Hamlington et al (2011) and Church and White (2011)
We have rewrite this part.

P3 lines 11-22: references?
We have put the reference.

P3 lines 11-27: the focus/motivations are loosen and not concise.
We have rewritten this part.

P4 lines 9-10: do not understand
We have erased this sentence.

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?
The two TG's time spans did not cover 1993-2014.