Ocean Science Discussions

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Comment on os-2022-3

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Referee comment on "Attributing decadal climate variability in coastal sea-level trends" by Sam Royston et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2022-3-RC3, 2022

Summary: The manuscript by Royston et al., explores the mechanisms responsible for sea level variability at decadal time-scales using a combination of ocean general circulation model (NEMO), climate model predictions (CMIP6 ensemble), satellite altimetry observations and tide gauge observations. The authors attempt a reconstruction of sea level trend anomalies using a regression between climate indices and the correlated components of a decomposition in empirical orthogonal functions of the NEMO sea level outputs. These reconstructions are compared with tide gauge and satellite altimetry observations, to estimate how climate modes contribute to decadal sea level variations.

Recommendation: The thematic treatment is of great interest for the scientific community, as the internal sea level variability has been identified as a major source of uncertainties in climate models, especially in the near-future. It is therefore important to advance knowledge in the identification of the mechanisms responsible for sea level variations in a changing climate. The study brings some useful insights in this regard, and should be considered for publication after consideration of the following comments.

General comments:

- The description of the method lacks clarity in the manuscript. It is difficult to follow step by step what has been done with which data. It is unclear to me how the CMIP6 predictions are used. The reconstruction seems to be applied on the manometric, steric and GRD outputs of the NEMO predictions, but the method is still unclear. Have the eof decompositions been applied on the total, manometric, steric and GRD contributions individually? Then correlations are estimated between the PCs of the eof decompositions and the climate indices. Finally, a regression analysis is performed, though it is unclear how. A few equations would help to better understand this final stage. The text should be clarified and a flow chart would help to picture the steps of the analysis.
- There are a few methodological hindrances in the approach of the authors that have not been acknowledged. In particular, the authors calculate the correlations with climate

indices based on the results of an eof decomposition. The eof decomposition will pull apart physical signals and redistribute them into statistical models explaining less and less variance as you increase the order. As a consequence, the correlation between sea level changes and individual climate indices might be lost because it has been divided into several modes of variation. To avoid this issue, a multivariate regression is usually carried out directly on the variable of interest (here sea level changes). To deal with the issue of intercorrelated climate indices, a regularisation can be applied (see Pfeffer et al., 2018 and 2022). The multivariate regression also allows the identification of climate indices contributing to the sea level variations at each grid point, which is only possible with limitations with the author's approach. The authors should acknowledge these limitations to allow the reader to assess the relevance of the approach.

- The description of the data lacks clarity in the manuscript. In particular the description of processing applied on the altimetry and tide gauge measurements is imprecise. It is not clear that adequate corrections have been applied for the various datasets for GIA and GRD.
- The description of the results is clear and interesting. However, more precision would be appreciated. In particular, the authors restitute the performance of the sea level reconstruction based on climate modes by reporting the percentage of variance explained above a certain threshold. It would be much more informative to have a range of variance, with a minimum and maximum bounds for a given region. The authors also use several time expressions like “explain much of this” or “explain well”, it would be useful to have a metric, so that the reader can assess what “much” or “well” means.
- The conclusion is clear, but fails to compare the results with Pfeffer et al., (2018) and (2022) dealing with the attribution of climate modes contributions to steric and manometric sea level changes.

Detailed comments:

Abstract: Define GRD or use full words

L28-29: formulation not excessively clear

L33: “A proportion of regional variation in sea level rise”: change rather than rise. The full sentence is not clear.

L44: The two following references are lacking. Pfeffer et al., 2018 has shown the influence of the PDO, ENSO, AMO, NPGO and IOBM on steric sea level changes, with significant influence at pluri-decadal time scales. Pfeffer et al., (2022) has shown the influence of ENSO, PDO, AO, NAO and SAM modes in the barystatic component of sea level measured by GRACE.

L70: sentence not clear

L70-72: see general comment on eof decomposition

L78-81: reformulate to increase clarity

L102: not a huge fan of rolling pin, that will generate an aliasing of many different signals and modes of variability

L117: why not using the full altimetry period?

L110-122: verb missing. Reformulate the sentence for clarity

L113-115: Not clear reformulate.

L123-124: not clear why GRD correction is not applied. It does not rely on GPS measurements.

L133:typo "noting"

L157-158: This sentence is very confusing. GRD and GIA are observed by satellite radar altimetry and by tide gauges, but not in the same way since tide gauges are attached to the coast. The corrections applied on the various datasets must be consistent one with another. If you wish to remove these effects from altimetry, you need to remove the global mean correction if it has been applied if it has been applied (it depends on the product chosen, but usually gridded altimetry products are not corrected for GIA), and then apply an appropriate correction at each grid point. Maybe consider writing this paragraph after the description of the datasets. So it would be easier for the reader to understand what data processing is applied to which data.

L169: "Absolute sea level is defined from a multi-mission" ☐ Absolute sea level is defined from **the ESA SLCCI v2** multi-mission

L171-173: it is not clear that appropriate correction has been applied for gia. As stated earlier,altimetry-based gridded SLA products do not usually (check specific product) correct for GIA. The GIA correction is usually only applied on the GMSL. Please reference the altimetry product in greater detail (exact product name, version and doi) and explain what GIA correction has been applied in it. Then, state what specific correction you applied, so that it is consistent with other datasets.

L186: This approach has flaws. An eof decomposition will pull apart the physical signal into a suite of statistical modes. As a consequence, coherent physical signals will be separated into several modes. If the sea level is influenced by one or several climate modes at one location, the part of variance explained by climate indices is likely to be separated into several modes as well. Therefore, you will not be able to retrieve a strong correlation with a single PC, but are more likely to get partial correlations with a lot of different PCs. This is why multivariate regression is preferred. To deal with the issue of correlated indices a regularisation might be applied. Alternatively, statistical tests have also been applied to determine the robustness of a correlation between two time series.

L192: Why are tide gauges not corrected for GRD? Admittedly there are other sources of deformation that cannot be easily modelled and require GPS observations that are very sparse and usually very limited in time, but non-linear GRD effects can be estimated with models.

L200: it would be interesting to see the differences between the NEMO run and the CMIP6 mean prediction. It would be easier to compare to the spread, in order to assess if both approaches are consistent within uncertainties.

Section 4.1: clear and interesting

L231-246: The results might be inflated to some extent in this section. The proportions of variance explained are credible and exhibit similar order of magnitudes than previous studies. It is perfectly fine to report an explained variance of 20 or 30%. It is still significant when compared with the accuracy of model and observations, but also with other physical signals present in sea level observations, predictions and reconstructions. It would probably be better to give a range of explained variance for a given region, rather than a minimum explained variance. The regions where the percentage of explained variance is small (~ <30%) cover most of the coastal areas of the world (orange areas in Fig. 3b). It is important to state that in most coastal areas of the world climate modes explain a small but significant part of the variance. Similarly in Table 1, it would be better to report the percentage of locations with a variance in the first (0-25%), second (25-50%), third (50-75%) and fourth (75-100%) quartiles. That way, the reader would have a better picture of the statistical distribution of the results.

L265: name the regions where coastally trapped wave are expected

L272: Some precisions would help here. Which region are you referring to? What constitutes large magnitude variability? Is it above a certain threshold of RMS? Which one? What constitutes "much of that decadal signal" (proportion?)?

L278-279: the reconstructed trend anomaly seems to capture the pattern well but not the amplitude. It should be said. A figure of the difference would help. For regions where the observed trends anomalies are large (e;g. tropical Pacific, west coast of North and South America etc.) it would be good to estimate the ratio between the reconstructed and observed trend anomaly. Also be careful about the vocabulary, it is a trend anomaly not a trend.

L311-313: this has also been shown by Pfeffer et al., 2018 for the steric component, with in particular the influence of AMO emerging in ocean reanalyses, only with a sufficient time coverage (~ 50 years). Other modes such as ENSO, PDO, NPGO (North Pacific Gyre Oscillation), IOD and IOBM (Indian Ocean Basin Mode) have been shown to have a strong influence on the interannual-variability of steric sea levels over a 57 time period. The NPGO is not often considered, though it has been shown to have a very large influence on SSH (see articles by Di Lorenzo including but not limited to Di Lorenzo et al., 2008) and on the manometric component (Pfeffer et al., 2022).

Section 5 Conclusion: please provide metrics in your conclusion to support the soundness of your approach

References:

Pfeffer, J., Tregoning, P., Purcell, A., & Sambridge, M. (2018). Multitechnique assessment of the interannual to multidecadal variability in steric sea levels: A comparative analysis of climate mode fingerprints. *Journal of Climate*, *31*(18), 7583-7597. https://doi.org/10.1175/JCLI-D-17-0679.1

Pfeffer, J., Cazenave, A., & Barnoud, A. (2021). Analysis of the interannual variability in satellite gravity solutions: detection of climate modes fingerprints in water mass displacements across continents and oceans. *Climate Dynamics*, 1-20. https://doi.org/10.1007/s00382-021-05953-z

Di Lorenzo, E., Schneider, N., Cobb, K. M., Franks, P. J. S., Chhak, K., Miller, A. J., ... & Rivière, P. (2008). North Pacific Gyre Oscillation links ocean climate and ecosystem change. *Geophysical Research Letters*, *35*(8).