Response to reviewer 1 comment,: RC1, on "Attributing decadal climate variability in coastal sea-level trends" by Royston, Bingham and Bamber

Dear anonymous reviewer,

The authors thank the reviewer for their comprehensive review. We provide the following comments (in blue, italic) to the reviewer's major concerns (in black) below. Minor comments, typographical and figure errors are amended in the revised manuscript.

The study "Attributing decadal climate variability in coastal sea-level trends" aims to relate the variability in decadal sea-level trends in coastal regions to climate variability. Based on a high resolution ocean model, the authors reconstruct coastal, sea-level trends via linear relationships with climate indices.

Based on their reconstruction, the authors can confirm and quantify the dominance of manometric over steric sea-level trends at coastal locations and pinpoint locations where the GRD signal is of importance. They quantify the sea-level trend variance explained by climate variability and show that in one-third of all coastal locations almost half the variance can be explained by climate variability. Finally, their results suggest that climate variability has suppressed sea-level rise during the period 2008-2018.

The results are well presented, the paper is relevant to the scientific community and doesn't present any major flaws. I recommend the article to be accepted, after some minor revisions.

We thank the reviewer for their precis and recommendation.

General comments

1. The authors used a series of climate indices to establish the relation between sea-level trends and climate variability. It is my understanding that, except for the AMOC index, the indices are based on observations, or reanalysis data. Wouldn't it be more consistent to use indices that are derived from the model output or the atmospheric forcing data set to infer the relation between those indices and the modeled sea level? The reconstructed sea-level trends that are compared to observed trends should still be based on the observed indices, of course.

In terms of consistency, yes we agree that it is more consistent to use the model atmospheric forcing and model ocean variables to derive the climate indices and their decadal trends. In doing so, we should expect the relationship (regression coefficient) between the climate indices and sea-level trends to be stronger, in particular since the steric sea level change is directly related to the model sea surface temperature (SST). However, one aim of the work is to demonstrate the applicability of the derived relationship between sea-level trends and climate indices with observations. So in mind of the practicality of stakeholders using the relationship derived here, we felt it was more representative to use standard, publicly available derivation of the climate mode indices (CI). Although there are minor differences in the model atmospheric forcing and model SST compared with the standard CI, the calculated decadal trends are similar (Fig R1.1). Most of the acceleration or deceleration of the CI trends is captured in the NEMO model or its atmospheric forcing. The modes of variability, now discussed more in the revised manuscript, are dominated by relationships with ENSO, PDO, AMOC, with second or third order PCs additionally correlated with SAM or AO, and IOD with lower-order PCs. Whilst there are some differences such as the ENSO trends in the late 1970's, the SAM index in the early 1990's and the IOD index before 1985, that could affect the exact value of the coefficients determined by this method, the dominant modes of variability and the summed reconstructions would not be significantly affected if we were to use the NEMO-derived climate indices rather than the standard. Therefore, the conclusions of the work aren't affected.

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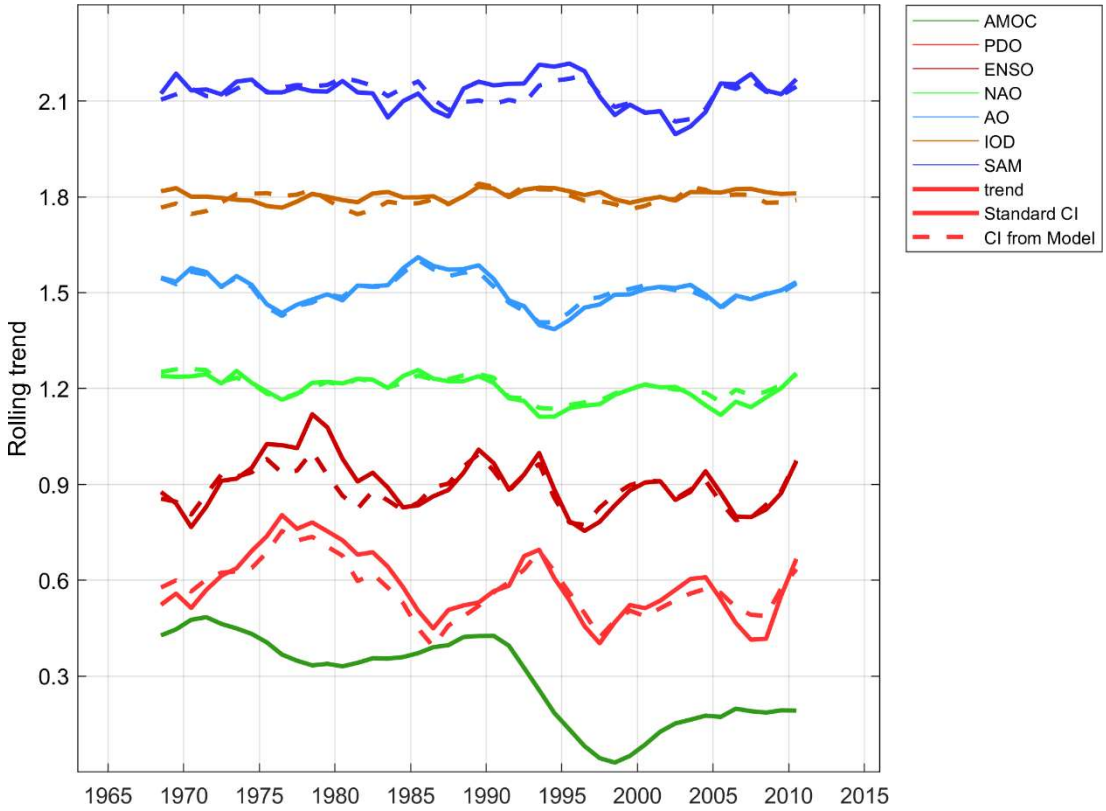

Fig R1.1 Comparison of decadal trends in climate mode indices (CI) by publicly-available standard derivation (Solid lines) and recalculated from the NEMO model atmospheric forcing and modelled SST (dashed lines)

2. The authors limit their analysis to a coastal region within 25 km of the coastline. As they point out the translation of steric to manometric sea-level anomalies depends on the water depth. I am wondering if a criterion based on water depth to identify coastal region would be more appropriate?

The authors did evaluate three different ways to define 'coastal':

1. *the standard altimetry community's definition of bathymetry less than 125 m, which represents 4.5% of the NEMO grid points;*
2. *the shallow bathymetry definition of Penduff et al (2019) of less than 25 m depth, which represents 1.3% of the NEMO grid points; and,*
3. *the Penduff et al (2019) distance definition of within 25 km of any low (coarse) resolution GHSSH coastal polyline, which covers 2.5% of the NEMO grid points.*

Option 2, the shallow bathymetry definition of Penduff et al (2019) didn't provide adequate coverage of coastal locations across the globe in the NEMO grid so we neglected it. The option 3 distance definition gave a good coverage of valid grid locations in the NEMO grid. The additional grid points covered by the option 1 altimetry community definition of shallow water (125 m) gave additional coverage away from the coastline itself and due to the nature of sea-level variability, it did not provide additional information in the principal component analysis of the sea-level trends. The authors felt the distance from coastline definition is sufficiently representative of the coastal region, given we use a principal component analysis to characterise the sea-level trend variability. Fig R1.2 presents the SSH leading EOF in each basin, based on options 1 (a) and 3 (b), demonstrating the limited difference the choice makes to the analysis against CI.

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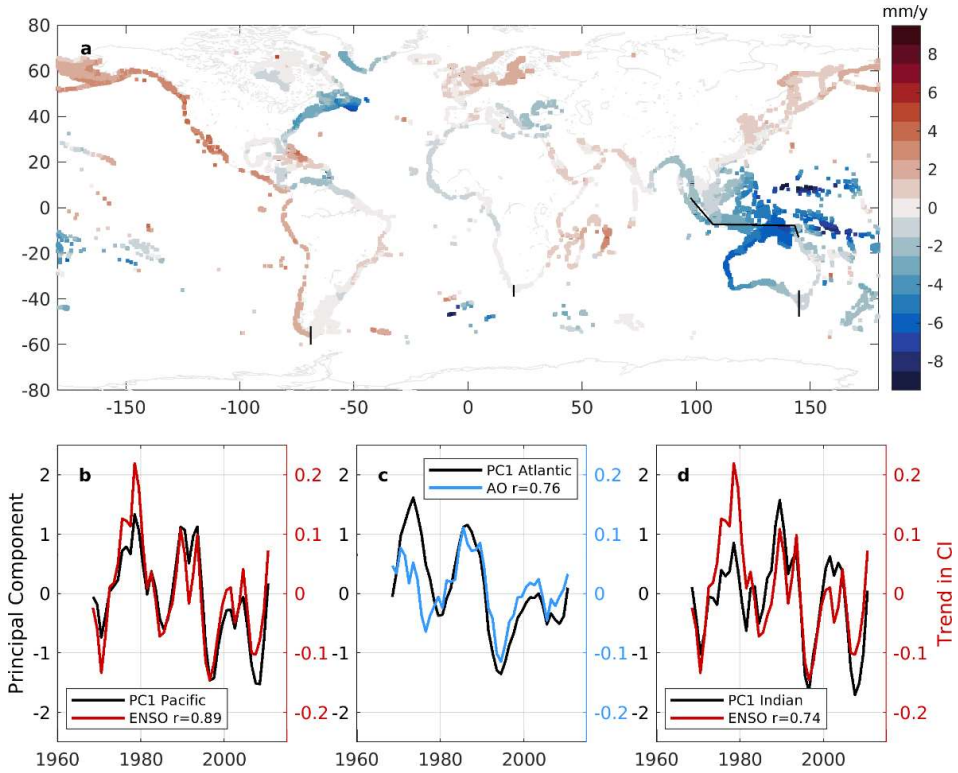
(a) Coastal definition option 1: by bathymetry (<125 m)



(b) Coastal definition option 3: by distance from coast (within 25 km)

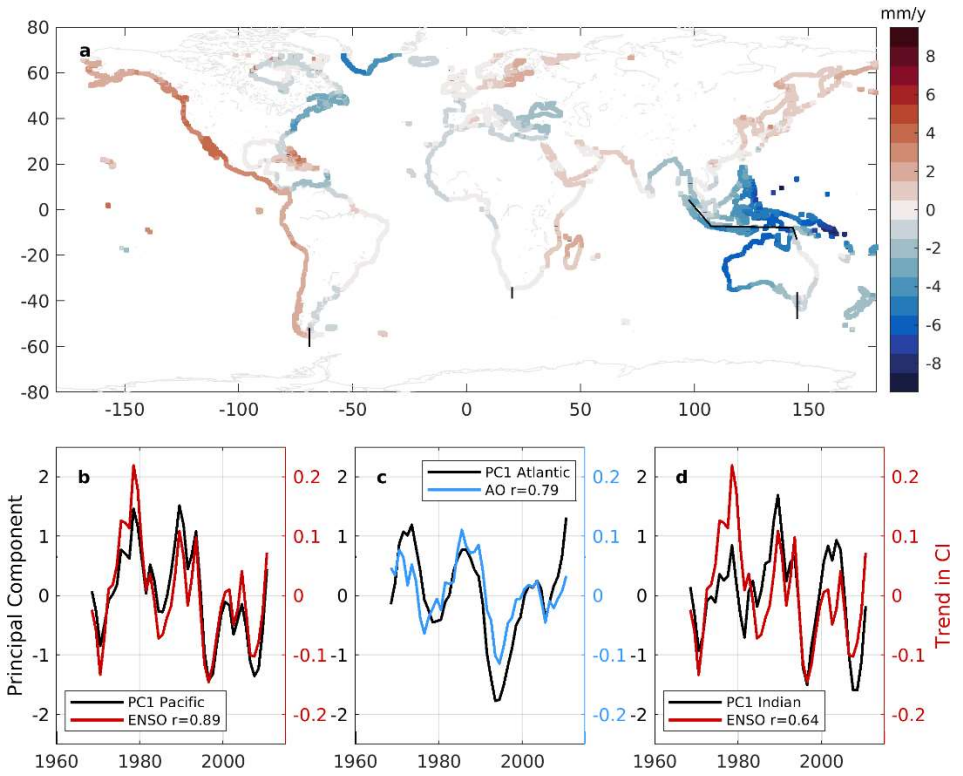


Fig R1.2 Comparison of the leading EOF mode of sea-level trend in each basin, using a coastal definition from option 1, 125 m shallow bathymetry (a) and from option 3, 25 km distance from the coast (b).

3. The authors should make sure to use the terms "intrinsic variability", "internal variability" and "climate variability" consistently, and properly introduce them in order to avoid confusion. For the most part of the manuscript, the authors use "intrinsic variability" whenever they refer to variability intrinsic to the ocean, i.e. not directly forced by the atmosphere and "climate variability" when they refer to variability intrinsic to the climate system, i.e. not related to long term (anthropogenic) change. I second this choice but suggest to make sure the terms are used consistently throughout the manuscript and avoid other terms like "internal variability" for example.

We thank the reviewer for this comment and have added definitions in the Introduction for clarity. We have endeavoured to ensure consistency in the revised manuscript.

4. The authors should make sure the term "sea level" is hyphenated when used as an adjective.

Amended in the revised manuscript.

Specific comments

L38-39: Please add a reference. *Added.*

L50-54: This seems to motivate the choice of the climate indices used for the reconstruction. The Arctic Oscillation is also listed in Table S1 but not mentioned here. It would be nice to see a list of all climate indices that were under consideration and not only the ones that were used in the end. Either here or as a supplement. *We did only consider a limited number of climate mode indices in the analysis, as stated in the Method text and Table S1. This choice was made from review of the literature on sea-level variability and in attempting to identify independent modes for the least squares analysis. We acknowledge this approach isn't comprehensive.*

L52: Frankcombe et al., 2015 does not distinguish between interannual and decadal variability in case of the IOD. So the reported impact of the IOD is likely due to interannual variability. Are there any publications that shows an impact of the IOD in the Pacific on longer timescales? *Whilst there are several works discussing interannual IOD variability with sea level, I agree the volume of work on decadal scale variability is much smaller. The relationship between climate variability and sea level in the Indo-Pacific Ocean is complex, because the atmospheric teleconnection and Indonesian Throughflow affect and modulate the climate modes in each basin on different time scales. Whilst the Nidheesh et al (2019) work focuses on sea level variability in the Indian Ocean, their attempt to explain sea-level variability at the decadal scale from CMIP5 runs discusses the two-way interaction of the decadal IOD and ENSO modulations in both the Indian and Pacific Ocean basins. I have added some text to the manuscript.*

L57-59: The statement is true in general and not only for intrinsic sea-level variability. *True, I have amended the sentence.*

L87: I'm not sure what is meant by "real" atmospheric forcing. Apologies for the lack of clarity. *Replaced with 'observed' i.e. the atmospheric forcing includes anthropogenic change, which we attempt to account for by removing the global-mean sea-level at each time step.*

L112: What exactly is meant by a non-significant PC? *I have amended this text in the revised manuscript. We use all EOF modes that describe at least 5% of the sea-level trend variance. We determine the*

correlation coefficient between each PC and CI, test the statistical significance of the correlation, and chose the highest correlated CI to represent each PC in turn.

L117: Is there a reason for this particular period 2008-2018? Altimeter data would allow for a longer period or an additional period of similar length. *The coefficients of determination from the linear regression on climate index trends are defined for a decadal period. A representative period from the altimetry era is used simply as an example of reconstructing a decadal sea-level trend from observed climate mode indices. It is the case that this assessment could be made on any decadal period from 1993 onwards.*

L122-124: The data is not corrected for VLM of any kind other than GIA, right? Not just GRD-induced VLM. *Correct, I have amended the text.*

L127: The authors refer to Marzocchi et al. 2015 and Moat et al. 2016 for a detailed model description. I understand that this is very subjective matter, but I suggest to include a few more details of the model setup that are relevant for this specific study on sea-level variability. I am thinking of issues like a fresh water budget correction or restoring of temperature or salinity to a climatology which are commonly used in ocean models but have the potential to affect sea level variability. *Text added.*

L130: "The NEMO Working Group (2019)" refers to NEMO4.0 but from the year of publication of the other two reference a assume the experiment is based on NEMO3.6 or older. *This is correct, the model configuration is described better by the Moat et al and Marzocchi et al citations.*

L136-138: I suggest to avoid the term "correction" in this case. My understanding of "correcting for the Boussineq approximation" is to diagnose the global mean steric sea level by considering the mass budget (see Greatbatch 1994, Madec and NEMO System Team, 2016), but the authors merely subtract the spurious global mean trend, which is of course sufficient in this case. *Yes this is a fair comment and the text has been amended accordingly.*

L146-147: I am not sure what is meant here. "Internal ocean variability" in this case refers to variability of oceanic parameters intrinsic to the climate system, which is diagnosed from the ensemble spread, correct? The "internal/intrinsic/chaotic" variability of the ocean model represents variability intrinsic to the ocean, i.e. not directly forced by the atmosphere. Something very different and impossible to diagnose from a climate model. So what exactly is going to be compared? *This statement wasn't clear and has been amended. We use the CMIP6 model ensemble to confirm the NEMO model sea-level trend variability is reasonable and within the expected envelope of variability, with similar atmospheric (historical) forcing.*

L150: I suggest to use the term "monthly means" rather than *"monthly time stamps"*. *Amended*

L203: The following sentence lacks a reference: *This effect in climate models is typical in semi-enclosed seas*. *This statement has been clarified and references added.*

L232-234: For the benefit of the reader, please clarify with which timeseries the reconstruction has been correlated. *Amended*

L247-253: This is in large parts a repetition of what has been said in the paragraph starting at line 57. *Yes we accept this is somewhat a repetition, and this paragraph is amended to discuss only the results from separating the sea-level components.*

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L247-L259: Please clarify why this paragraph is necessary here. Did you use ARGO Data to compute steric sea level? *Yes this is predominantly a discussion point around the separation of sea-level to its components and is removed.*

L261-262: Can you speculate as to why the addition of contributions from each component improves the result? It's not obvious to me. *As shown in Fig 2, the variance in the trends are dominated by different components in different regions of the coast. Applying the reconstruction method to total SSH gives the EOF with highest power that derives from different physical processes in different areas. Splitting the variance into component parts allows the EOF analysis to determine more specific spatial patterns for each component part.*

L261: Please refer to a specific table or figure, like Table S1 for example, rather to "Supplementary Information" in general. *Amended.*

L281-282: *"The variance explained at coastal grid cell locations in the Atlantic and Indian Oceans is increased, although the variance is decreased by the reconstruction in the Pacific."* Is this shown somewhere? *This statement is made specifically for the one example period compared against observations by satellite altimetry. There are some regions where the reconstructed trend from climate indices is of opposite direction (Fig 5b) to the observed trend (Fig 5a), the Gulf of Mexico coast being a clear example. The text has been amended*.

L300-301: I expected some results after this sentence and I actually find the results shown in Table S1 worth to be mentioned in the manuscript. I would not have expected such a strong influence of the AMOC index on the Indian Ocean for example. *Together with the comment in RC3, the PC1 relationships rom Table S1 are added to the manuscript and a discussion is added here and in the Conclusions to compare our results against previous works.*

Figure 4: What does the envelope around the gray line show? *Caption amended, the grey shading is 1σ trend error estimate assuming an AR(1) noise model for the observed tide gauge time series.*

Figure 4: What is shown in panel f? The caption says global mean but the title says Helsinki. *It's Helsinki, corrected.*

Figure S1: I guess the reference to AVISO is incorrect? *Yes apologies, the altimetry data used throughout the work is ESA SLCCI v2.*

Technical comments – *amended in the revised manuscript. With thanks.*

L12: Introduce GRD

L18: "sea level variability"

L22: "sea level change"

L33: "sea level rise"

L33: The sentence is very long and could be read as if locations of fronts and ML/thermocline depth induce variations to the GRD equipotential.

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L35: "sea level change"

L57: "sea level trends"

L58: "sea level changes"

L53: "sea level variability"

L69: "sea level variability"

L86: "sea-level"

L108: "sea level trend"

L133: Please check sentence

L203: "In contract"

L226: "sea level variability"

L243: "areally"

L302: "sea level variability"

L306: "sea level variability"

L352: Correct sentence

Figure 5: The label of panel c overlaps with the labels of the x-axis.

Figures S4 – S17: Panels lack labels

Figures S5,S8,S12,S13: The ignored pattern are still shown here. *For transparency, we are showing the pattern and PC that is missing from the reconstruction because there isn't a significant correlation with a climate index, which introduces error into the reconstruction.*