

Ocean Sci. Discuss., referee comment RC1  
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## Comment on os-2022-3

Anonymous Referee #1

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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-RC1>, 2022

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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.

### 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.

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?

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.

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

### **Specific comments**

L38-39: Please add a reference.

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.

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?

L57-59: The statement is true in general and not only for intrinsic sea-level variability.

L87: I'm not sure what is meant by "real" atmospheric forcing.

L112: What exactly is meant by a non-significant PC?

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.

L122-124: The data is not corrected for VLM of any kind other than GIA, right? Not just GRD-induced VLM.

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.

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.

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.

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?

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

L203: The following sentence lacks a reference: *This effect in climate models is typical in semi-enclosed seas.*

L232-234: For the benefit of the reader, please clarify with which timeseries the

reconstruction has been correlated.

L247-253: This is in large parts a repetition of what has been said in the paragraph starting at line 57.

L247-L259: Please clarify why this paragraph is necessary here. Did you use ARGO Data to compute steric sea level?

L261-262: Can you speculate as to why the addition of contributions from each component improves the result? It's not obvious to me.

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

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?

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.

Figure 4: What does the envelope around the gray line show?

Figure 4: What is shown in panel f? The caption says global mean but the title says Helsinki.

Figure S1: I guess the reference to AVISO is incorrect?

## **Technical comments**

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.

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.