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Reply to the reviewers. Second round of reviews

"Variability of manometric sea level from reanalyses and observation-based products over the Arctic and North Atlantic Oceans and the Mediterranean Sea" by Andrea Storto, et al.

We thank the reviewers for the careful reading, the encouraging comments, and the many suggestions to improve the quality of the manuscript. We appreciate the very careful checks of the reviewers that helped us spot some inconsistencies in the text, introduced during the revision and not identified earlier. We report below a point-by-point answerto the Reviewer's comments (Reviewer in bold font, our reply in regular font).

Reply to Anonymous Reviewer 2

Main comments

The manuscript uses several observational and reanalysis datasets to describe and explain manometric sea-level variations in the Arctic Ocean, the North Atlantic Ocean, and the Mediterranean Sea. In addition, the manuscript reports strengths and limitations of the datasets used for the analysis. I would like to thank the authors for submitting this contribution. I found the manuscript interesting. However, I recommend a major revision of the manuscript before it is accepted for publication.

Thanks for the comments

Minor Issues

I would recommend the authors to check the text for:

• missing articles (e.g., "are applied to GRACE solutions" in L107)

• missing hyphens (e.g., "gravimetry based" in L63, or "in situ measurements" in L114/115)

• typos (e.g., "wet troposphere correction" instead of "wet tropospheric correction" in L122)

• missing words (e.g., "which covers from 1993 to 2019" in L145)

• repeated words (e.g., "changes" in L196)

These are just small mistakes, but they should be removed before the manuscript is accepted.

Thanks. We have corrected all the points above, plus a few more small changes while re-reading the manuscript to improve the language.

Abstract

The acronym GREP is used without it being previously defined in the abstract.

Thanks, GREP is now introduced with its acronymic meaning

Short summary

It is maybe better to find an alternative to "three different techniques". I am not sure we can refer to "reanalyses, gravimetry, and altimetry in combination with in-situ observations" as techniques.

We changed techniques to methods.

Data and methods

Section 2.4

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1) In L159/160, the authors state: "… where the interannual signal is the timeseries to which the monthly climatology has been subtracted, and the subannual the residual part." I am not sure I understood this approach correctly. It seems to me that this method does not allow the authors to extract the subannual and the interannual components of a timeseries. In fact, the residual is the monthly climatology and, as such, it should mostly correspond to the seasonal cycle. If this were the case, the authors should avoid referring to the residual variability as 'subannual' because this term can be misleading.

We thank the reviewer for pointing out this sentence; indeed, we used subannual basically as a synonym of seasonal, but we recognize it could be misleading and substituted all occurrences of "subannual" with "seasonal".

2) There seems to be an inconsistency between what written in Section 2.4 and what written in the caption of Table 2 in relation to how the seasonal amplitude is computed. In L169, the authors write that "Seasonal amplitude is defined by fitting the monthly data to a sinusoidal curve". However, the caption of Table 2 states that "Seasonal amplitude stems from fitting the detrended timeseries to a sinusoidal line". Did the authors fit the sinusoidal curve to the detrended timeseries or to the original ones?

Thanks for pointing this out, indeed was a typo between the two versions of the manuscript, as we re-did the calculation. We fit into a line with a trend term and a sinusoidal term and modified the text for clarity. For better clarity, we now stress that the fitting curve has a trend a seasonal components. To avoid redundancy, we refer to section 2.4 in Table 2's caption.

3) In L169/170, the authors write that the "interannual variability is the standard deviation of the detrended and de-seasonalized timeseries."

However, by doing so, they also include the contribution of the subannual variability.

This is partly true, but we assume that most of the subannual signal is seasonal and remove the seasonal term; we have added a comment on this in the text.

4) How did the authors account for the presence of sea ice in the Arctic Ocean? Does the satellite altimetry dataset provide sea-level observations in the regions covered by sea ice? If not, has this region been masked in the other datasets to ensure that the datasets return consistent results? In any case, I suggest that the include additional information on how the producers of the satellite altimetry data handle the presence of sea ice.

We have included this information in Sections 2.1 and 2.2, thank you. Indeed, the data is flagged in the presence of sea ice in the C3S product and masked out. There is no data at very high latitude, because of the recurrence of sea ice.

Results

General comment on this section

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1) The text does not specify the period of the analysis. This should be clearly stated in the text.

We have added this info at the beginning of section 3. Thanks

Figure 1 suggests that the analysis spans the period between January 2003 and December 2019. However, it seems that all the datasets are available from April 2002 to December 2019. The authors should state why they did not perform the analysis over this longer period.
Thanks. We excluded a few months (second half of 2002) over the 17 years to have a homogenous period covering full years, for all the analyses presented. We prefer not to include this information in the text, as this is probably not crucial for most readers.

2) Section 3.3 examines the relationship between large-scale atmospheric/oceanic patterns and manometric sea-level variations in the three regions. However, it lacks a thorough description of the results. More details are needed to show that the statistical method provides results that are physically sound.
We have added more information and interpretation of the results, and a few more notes in section 2.4. Note however that this fingerprinting technique is based on a statistical method (LASSO regression) that by construction minimizes collinearities.

Section 3.1
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1) L194/195: There is typo either in the main text or in Table 2. The main text states that, in the Arctic Ocean, GRACE returns a manometric sea-level trend of 2.45±0.44 mm/year, whereas GREP of 3.45±0.57 mm/year. However, Table 2 shows the opposite.

2) L201: There is typo either in the main text or in Table 2. The main text states that the interannual variability in the North Atlantic Ocean ranges between 6.6 to 8.6 mm. However, Table 2 shows values between 6.0 and 6.6 mm.

4) L205: There is typo either in the main text or in Table 2. The main text says that the interannual variability of manometric sea level in the Mediterranean Sea is more than 25 mm for all datasets. However, Table 2 shows that the interannual variability from GREP has an amplitude of 20 mm.

Thanks a lot for carefully checking the consistency of the results. The previous three points are due to some typos due to the recomputation between the original and revised versions of the manuscript. By mistake, we revised and upgraded the table but not all the texts. Now we have adjusted the main text and rechecked that all values in the Table are correct.

3) L203: The authors write that, as expected, the North Atlantic manometric sea-level variability resembles the global signal. This statement needs to be supported by one or more references.
Thanks for pointing this out; it was our subjective expectation, but as we did not find any clear indication of this in past literature, we removed "as expected" from the sentence.

5) L211/212: I suggest the authors provide an explanation of why GREP tends to underestimate the maxima in manometric sea level in the Mediterranean Sea in 2006, 2010, 2011, and 2018.
Added: "likely due to the use of climatological discharge from rivers in the reanalyses, and the low resolution at Gibraltar strait affecting the representation of the Mediterranean inflow".

Section 3.2

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1) L225: The authors write that "… SLB might capture the year-to-year variations better than the reanalyses". I do not think the authors can make this statement as the correlation between GRACE and SLB is not statistically different from the correlation between GRACE and GREP.

2) L228: I recommend that the authors rephrase the sentence in which they argue that they have greater confidence in GRACE and GREP than in SLB regarding the seasonal cycle in the Mediterranean Sea. This seems an important conclusion, but it is not very well expressed.

3) L231-238: The authors state that the SLB approach returns poor results in the Arctic Ocean. This is an interesting conclusion. However, as the manuscript aims to compare the quality of the different datasets and approaches, it seems important to investigate this point further and try to understand whether the poor performance with the SLB approach results from problems in the sea-level anomaly or in the temperature and salinity profiles. For example, how does the sea-level anomaly from satellite altimetry compare to that provided by the reanalyses? Or how does the steric sea level from observations compare to that provided by the reanalysis?
We thank the reviewer for this suggestion, and we compared, in terms of correlation, the reanalyses total and steric separately with the altimetry and steric dataset used within the SLB approach. What we found (partly expected due to the assimilation of altimetry data in reanalyses) is that the total sea level between the two is quite well correlated (e.g. 0.69 for the interannual signal) while the steric is weakly correlated (0.35), resulting in an overall small correlation as shown in the figure. We cannot add much to this in terms of figures, etc., but we added a sentence in section 3.2 to report this finding.

Section 3.3

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1) L254/L255: The authors write that: "AMO is known to modulate the sea-ice interannual variations and the Arctic amplification (Li et al., 2018; Fang et al., 2022), which are both important contributors to the sea level manometric fluctuations." The authors should provide evidence that sea-ice interannual variations and the Arctic Amplification significantly affect the manometric sea-level variability in the Arctic.
Over the past few decades, the Arctic Ocean has been experiencing rapid warming, a phenomenon known as Arctic Amplification. Fast warming has been leading to a widespread shrinkage of the cryosphere, including sea ice, but also glaciers, and ice sheets. These rapid changes are affecting the atmospheric and ocean circulations in the Arctic, which in turn impact the climate variability at both regional and global scales. The increased melting of land ice and disturbances in atmospheric and ocean circulation influence the variability of manometric sea levels through the input of freshwater and its redistribution in and out of the Arctic basin. These significant and interconnected changes in the Arctic climate are caused by multiple factors, including human-induced greenhouse gas emissions, as well as internal climate variability such as the AMO.
We cannot detail all these arguments, but we added a reference (Previdi et al., 2021) that addresses these issues in detail.
Previdi, M., Smith, K. L., & Polvani, L. M. (2021). Arctic amplification of climate change: a review of underlying mechanisms. Environmental Research Letters, 16(9), 093003. doi:10.1088/1748-9326/ac1c29

2) I recommend that the authors show the spatial patterns of the climate modes, and the timeseries of the respective climate indices. Journal restrictions might prevent the

authors from doing it. In this case, could the authors add this piece of information in the supplementary material?
The time series and spatial patterns of climate modes are shown in Fig. 1, 3, and 4 in Pfeffer et al., 2022, and are not reported in the paper to avoid redundancy and for the sake of brevity as required by the journal policy; the link with the previous paper is made now more evident in section 2.4.

3) I would also suggest that the authors better explain their results in section 3.3. As an example, why does the manometric sea level in the North Atlantic appears to be more related to the NPGO than to the NAO, the AO, and AMO? The authors provide little information on the reasons behind this relationship. They argue that: "(L260-261) While NPGO well explains variations in the eastern North Pacific Ocean (Di Lorenzo et al., 2008), its impact on the North Atlantic manometric sea level likely depends on the global barystatic signal and teleconnections (Iglesias et al., 2018)." Which teleconnections in Iglesias et al. can explain this relationship? Furthermore, do Iglesias et al., 2018, focus on the entire North Atlantic Ocean or only on the eastern North Atlantic Ocean?
The NGPO index is defined as the second mode of sea surface height variability in the Northeast Pacific (180°W–110°W; 25°N–62°N). However, The nature of PDO and NPGO is changing at multidecadal time scales in a warming climate. The spatial structure of such modes is non-stationary in time, and the relationship between the physical, environmental, and biological variables and the climate indices evolves. The correlation between the PDO and NPGO has increased in the last decades. In recent decades, the NPGO showed increasing association with the first mode of climate variability rather than the second (Litzow et al., 2020). The NGPO may therefore be a relevant index to understand climate variability at the global scale, which influences manometric sea level changes in the Atlantic. It may be noted that Pfeffer et al., (2022), also found a widespread correlation of the NGPO with the water mass redistribution observed with the GRACE and GRACE-FO satellites, although this association was found to be more robust on land than on the ocean. To avoid confusion and subjective interpretations, we now refer only to the published work showing the wide impact of the index on the sea level and have rephrased the sentence.

4) The authors try to identify the large-scale atmospheric and oceanic patterns that are responsible for manometric sea-level variations in the North Atlantic. However, this region is wide as it extends from 0° to 67°N. So, different areas of the North Atlantic might be affected by different large-scale patterns. I suggest that the authors focus on different sub-regions of the North Atlantic. For example, they could consider the tropical North Atlantic, the mid-latitude western North Atlantic, and the mid-latitude eastern North Atlantic separately.
The purpose of this article is not to provide a detailed explanation of the factors affecting the variability of sea levels in the Atlantic Ocean and sub-basins. Instead, we aim to describe the changes in manometric sea level as accurately as possible using various datasets and evaluate the significance of the different signal contents in comparison to known modes of variability. Subdividing the NA in sub-basins would result in inconsistency with the analyses performed in Figures 1, and 2, and we prefer to keep the analysis as it is.

5) The authors should also explain why the manometric sea-level variability in the Mediterranean Sea seems to be largely affected by the AO, but not by the NAO. In this respect, the authors consider the AO and the NAO as two distinct climate modes, but the AO and NAO indices might not be independent. On the contrary, they might be

highly correlated (e.g., Ambaum et al., 2001). How do the authors handle this statistical dependence? How much do the results change if either the NAO index or the AO index is excluded from their analysis?
The LASSO will by construction select the indices in the multivariate regression that will minimize the cost function, expressed as the sum of least-squares residuals and absolute values on the coefficients of the regression. If two indices are largely correlated, the LASSO will pick the one minimizing the cost function. Shall we remove the AO from our analysis, NAO will be selected as a relevant index. The AO and NAO should not be considered as two independent modes, but rather as two relatively similar ways of describing the climate variability in the Arctic and North Atlantic regions. We added a sentence on that (in section 2.4 and while commenting on the results).

6) The authors use the full manometric sea-level signal to study the relationship between manometric sea-level variations and climate modes. However, splitting the signal into interannual and subannual variability is needed as the two might be forced by different climate modes.
The strength of a multivariate LASSO regression is to select all relevant modes of variability simultaneously. The method can select multiple indices, explaining either short-term or longer-term variations. The method however does not account for delays (e.g. out-of-phase signals) or seasonal dependence (e.g. summer or winter influences of a specific mode may differ) between climate indices and manometric sea level changes. Consequently, it is not very meaningful to filter the input signal given to the LASSO regression.

7) The authors should explain how they derive the climate modes and the climate indices used in the manuscript. For example, which datasets and which techniques do they use?
The time series and spatial patterns of climate modes are shown in Fig. 1, 3, and 4 in Pfeffer et al., 2022, to which we refer for the data sources, etc. (please see the answer and modification related to previous point 2).

Table 1

I would recommend the authors to rearrange the order to the products in Table 1. I would rearrange them in such a way that they follow the same order of appearance as in the "Data and Methods" section.

Else, the authors could remove Table 1. The piece of information in Table 1 could be included in the main text. I would favor this solution if it allowed for the inclusion of a new figure (e.g., a figure showing the spatial patterns of the climate modes used in Section 3.3 and of their respective indices).
Thanks. We have changed the order of the products in the Table as requested; however, we prefer to keep the Table with full information, as this could be interesting for future follow-up studies and intercomparison (reading a few numbers within the text won't ease their use in future studies).

Figure 2

The colorbar does not help understand the heatmaps. I recommend the authors to test alternative colorbars. For an example, how would the viridis colorbar perform (https://matplotlib.org/stable/users/explain/colors/colormaps.html)? The authors could also try reducing the range of the colorbar as the lower correlations are higher than 0.2

or 0.3. Maybe, this would help make the figure clearer. Using a discrete colorbar and adding the actual number within each cell of the heatmaps could also help.
The authors could also reduce the size of the white spaces in between the subplots.
Thanks for the suggestions, which have all been implemented in the revised figure.

Figure 3

The histograms contain vertical bars to show the standard errors of the regression coefficients. However, there is no vertical bar associated with the contribution of the NPGO to the manometric sea-level variability in the North Atlantic. Also, the limits of the y axes in Figure 3 should be modified because this same contribution is out of scale.
Replotted as suggested; we also show the mean of the relative importance across the dataset, to provide a more “global” measure of impact.