

Manuscript « Regional sea level budget over 2004-2022 »

Responses to the Reviewer Comments (*in italics*)

30 March 2025

I. Reviewer 1

The work under review is a relatively straightforward regional sea level budget analysis based on the Scripps Argo product for the steric component, several gravity-derived and reanalysis-based manometric sea level estimates, and the C3S altimeter dataset for sea level. I have a considerable number of issues that I wish the authors can address in a revision of the current draft. Major and minor comments are listed below, with reference to line numbers where relevant.

Major comments

The paper deals with linear trends in many of the figures but there is no discussion of whether the trends are statistically significant (i.e., statistically different from zero at some confidence level). This seems to be a major shortcoming of the presentation and can be simply corrected by stippling or not plotting insignificant values in figures 1 – 7. The discussion should then be focused on statistically significant non-zero trends.

Response

We agree with Reviewer 1 that this was a major gap in the initial version of the manuscript. We have now added trend uncertainty maps for the sea level and components and focus the discussion on regions where the trend signal in all terms of the budget and residuals is significant (i.e., above the noise).

In addition, all budget analyses need to carry at least a qualitative discussion of possible uncertainties in all the terms, in this case sea level, steric and manometric components derived from the different products. The authors use an ensemble of manometric estimates but they fail to provide a spread or some quantitative measure of uncertainty for their results.

Response

See above

And some features in their results are treated lightly or ignored. For example, there is a clear issue with the major Tohoku and Sumatra earthquakes affecting the trends in the mascon solutions, but that is never mentioned.

Response

We now have briefly discussed the signal related to the Sumatra and Tohoku earthquakes in the GRACE-based manometric map and residuals (based on GRACE)

Last but not least, I find the discussion of many figures loosely presented and at places not really jiving with what I could reasonably infer from the figures themselves. Here are several examples of this issue:

325-326/ I would disagree. For example, halosteric term is larger than thermosteric term in parts of the South Indian Ocean, northeast North Pacific, and manometric term is larger than thermosteric in parts of the Southern Ocean.

Response

The text has been modified

333-335/ I think the claimed similarity between manometric and thermosteric terms is hardly justified, and actually I can't think of any reason why we should expect them to be similar.

Response

We did not say that. However to be clearer, we rephrased this sentence.

336-339/ The text mentions strong residuals, but contrary to what it is claimed, in the eastern Atlantic there are relatively strong negative thermosteric trends, not fully compensated by positive halosteric trends, which seem to give rise to the large residuals (sea level trends are fairly weak);

Response

Text has been modified for clarity

419-420/ Again, I am having a hard time seeing this claim in the figure. Actually, I see opposite signs in spatial trends of the ensemble mean reanalyses and GRACE in the North Atlantic as well.

Response

We did not say that trends in GRACE and reanalyses-based manometric components have the same sign. They have indeed opposite sign in many regions, including in the North Atlantic (that part of the sentence 'except in the North Atlantic...' was an error. It is now corrected).

If qualitative claims are going to be made, the authors should make sure they are clearly evident in the figures. In addition, quantitative analyses such as determining pattern

correlation coefficients should be attempted when deriving inference about similarity between spatial trend patterns.

Response

Quantitative results are provided in Table 2 that gathers RMS residuals for each reanalysis and GRACE mascon cases and for each oceanic region.

Minor comments

51-52/ Note that “redistribution” can also lead to changes in steric component (you can redistribute density, not just mass).

Response

Text modified

66-60/ This sentence (or perhaps actually the full paragraph) almost equates global mean sea level budget with the issue of global mean sea level rise, but of course budget analyses can and should apply to all time scales, not just the linear trend, for both global mean and regional cases. A recent example of regional budgets for the seasonal cycle is found in <https://doi.org/10.1029/2024EA003978>.

Response

This study focuses on trends. Looking at the annual budget is beyond the scope of the present study

101/ Delete “already in the ocean”.

Response

Done

141-143/ *Sentence could be improved for clarity.*

Response

Done

157-158/ Statement is not strictly true, as changes in global mean pressure can come from changes in the mean water vapor content of the atmosphere and those can affect the barostatic term. Likely a small effect, however!

Response

The water vapor contribution essentially affects the global mean barystatic component, which is removed in the present analysis.

168/ “GRDs” sounds weird! “GRD effects” or similar would read better, here and elsewhere in the text.

Response

Corrected

171-172/ I think this statement pertains to “absolute” sea level. Please clarify the text. Similar issues may apply to other parts of the text.

Response

The word ‘absolute’ is added when needed

175/ Delete “sea level”; also “northeast coast of North America”.

Response

Done

205/ “which leads to”

Response

Corrected

179/ “based on observations”

Response

Corrected

213-214/ I think you are referring to the drift in the global mean, but statement needs to be clarified.

Response

It seems that it is was we wrote

217-218/ We need some estimate of these deep steric changes at the local scales of interest to this paper.

Response

We agree with the reviewer’s comment. We added the following sentence to emphasize our statement : “Based on deep Argo profiles, Lele and Purkey (2024) estimated the

deep ocean steric sea level rise (temperature and salinity contribution) being 0.13 ± 0.16 mm yr⁻¹ in the south Pacific Ocean over 2014-2023”.

246/ “two filtering levels”? Please clarify this text.

Response

We removed this part of the sentence because this concerns to detailed technical parts of the data processing.

254-255/ The GRACE and GRACE-FO records have many gaps. Please clarify in the text how those are handled in the analyses.

Response

We added a sentence explaining how gaps are taken into account

263-264/ Unclear sentence.

We have rewritten the sentence for clarity.

283-285/ Can the authors discuss, at least qualitatively, what sort of errors this may imply in the derived manometric trends?

Response

Considering a linear extrapolation for the fingerprints at the end of the record may have negligible impact on the reanalyses-based manometric component considering that this contribution is very small

287-293/ I am confused by the treatment of atmospheric loading corrections for GRACE. Of course, GRACE does not “see” atmospheric loading effects, if one has an inverse barometer behavior. Effects of atmospheric loading would only be apparent in the global mean bottom pressure, but those are apparently removed in the present analyses. I think this needs to be clarified, to make sure corrections are appropriately applied.

Response

Text has been clarified

329-333/ You start by calling out large residuals in the North Atlantic but those are in the eastern part of the basin. You go on to discuss the western part and south of Greenland, where residuals actually seem relatively small. This is somewhat confusing.

Response

Text has been modified

340-345/ It would be useful to include an extra panel with residuals calculated on the basis of steric trends only. Do they look better? Actually including such panel in figure 2 might justify that figure better. Otherwise, figure 2 is not needed, as those same two panels can be readily examined in figure 1.

Response

The steric trend map is now added

374/ “everywhere” is an overstatement given results in figure 3c,d.

Response

Corrected

375/ Very unclear what inferences are being made in this text, given the previous discussion of results in figure 3. Please rewrite for clarity.

Response

Text has been modified

399/ Delete either “ ’s” or “the” before Camargo.

Response

Corrected

434-435/ “CIGAR also does not assimilate”

Response

Corrected

447/ “If we exclude FOAM”

Response

Corrected

459/ “assimilation in Figure 4 (e.g.,”

Response

Corrected

458-463/ You reference figure 6 but the text seems to be comparing CIGAR and C-GLORS results in figure 5?

Response

Corrected

461/ The largest differences are actually west of the Drake Passage.

Response

A sentence has been added

463-465/ I don't follow the corollary.

Response

Text has been corrected

475/ Actually all reanalyses, not just CIGAR show positive residuals.

Response

Text has been corrected

494/ "stripe"

Response

Corrected

495-497/ Very unclear what this sentence means. Please rewrite.

Response

Text has been corrected

Figure 9/ This figure is hardly justified. It does not bring anything new to what is already discernible from Figure 8a. I don't think the global EOF adds any relevant information to the discussion.

Response

Figure 9 has been deleted

511-513/ I think the residual for "all but North Atlantic" case is also significant?

Response

The sum of components agrees reasonably well with the sea level curve. Even if the residual curve is not perfectly flat, it does not show the strong decrease as in the North Atlantic case.

514/ The halosteric decrease is evident after 2016, not 2013-2014?

Response

Yes this is true. Corrected

II. Reviewer 2

Review for: “Regional sea level budget over 2004-2022” by Marie Bouih, Anne Barnoud, Chunxue Yang, Andrea Storto, Alejandro Blazquez, William Llovel, Robin Fraudeau and Anny Cazenave (<https://doi.org/10.5194/egusphere-2024-3945>).

The authors investigate the regional closure of 20-year sea level trend budget based on altimetry (total), ARGO (steric), GRACE/Reanalysis (manometric) sea level datasets and GRD fingerprints. The paper focuses on the differences between various manometric datasets. It examines the influence of the GRACE processing on the regional sea level budget and emphasises the need to apply the geocenter correction. The largest regional trend discrepancies occur in the North Atlantic, and the authors suggest that a spurious drift in the salinity measurements may be responsible.

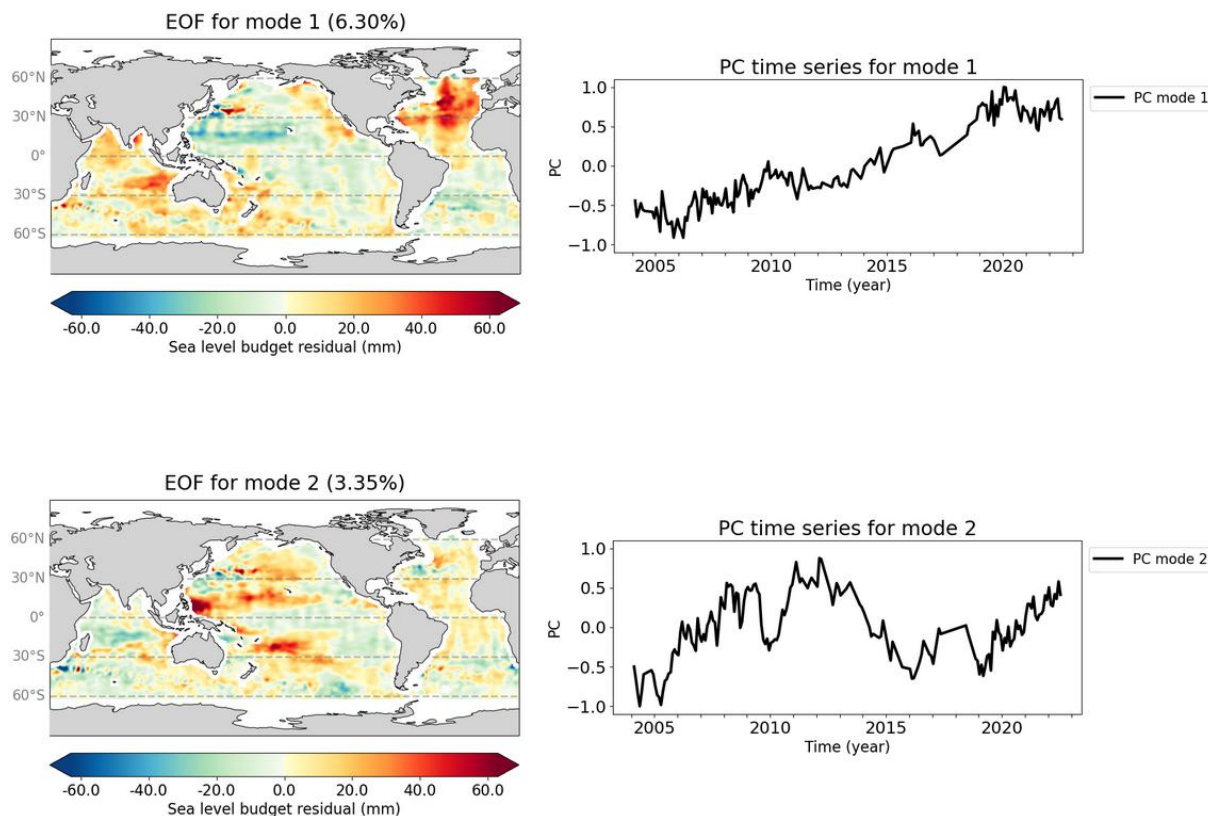
The paper deals with a relevant topic and is well written. The introduction provides a good overview of the topic and cites the relevant literature. Equations could help to make it easier to find out which components were taken into account for which data sets to calculate the residual trends. The figures are clear and informative, however, some of them seem to be in the paper twice. If possible, the datasets and the corresponding versions should be cited unambiguously (e.g. doi for altimetry).

Unfortunately, none of the manometric components studies provides a sufficient closure of the regional trend budget. Possible causes for the differences between the datasets should be discussed in more detail. A discussion of the uncertainties of the trend estimates is missing for all data sets. With regard to the North Atlantic, the explanation for not closing the trend budget should be better justified. The figures suggest rather decadal variability between the gyres than consistent long-term drifts in the entire area.

Response

We have added maps of trend uncertainties for the sea level data, components and residuals, and now focus on the regions where the residual signal is above the noise.

We also performed an EOF analysis of the gridded residual time series (the corresponding figure has been added in the SI). It is reproduced below (using GRACE mascons for the manometric component):



Mode 1 is dominated by a strong residual trend in the North Atlantic. Its spatial map is very similar to the residual map. Mode 2 shows a low frequency oscillation of period around 11 years on which are superimposed shorter fluctuations related to ENSO.

Specific comments:

Lines 78-92:

The usage of the expressions regional, basin-scale, sub-basin scale and local scale is confusing and could even be inconsistent

Response

These terms were introduced to distinguish between the different spatial scales

Lines 141-143: Shouldn't the barystatic component distribute according to the GRD fingerprints?

Response

The barystatic term is spatially quasi uniform. It was removed for the regional budget assessment.

Line 180: Could you provide numbers for the small regional GIA & contemporary GRD trends?

Response

Added

Line 190-200: Please specify the data version (doi?). Some of the given information is abundant since it is not used (Topex side A drift, total sea level uncertainties)

Response

Added

Lines 217: Could you provide numbers for the regional deep steric contributions?

Response

Added

Lines 265-: Could you specify the differences between the manometric data from ocean reanalysis? What is the uncertainty and why did you choose these models?

Response

Added

Line 271: Are all reanalyses based on ARGO data? Is it known how the salinity drift is handled in the individual reanalyses?

Response

This is indeed a key issue. We added the following text:

“One may wonder whether the salinity drift observed in some Argo floats as of 2015 is impacting the CIGAR reanalysis since, unlike altimetry data, T/S data are assimilated during the reanalysis integration, thus non-linearly interacting with dynamical processes. The treatment of the salinity drift simply consisted in rejecting data that Argo had flagged for rejection in the delayed mode. But this may not fully guarantee that all bad salinity data have been discarded. However, to compute the reanalysis-based manometric component, the local steric contribution is removed. Thus any effect of the Argo salinity drift should be minimized.”

Lines 304-305: Figure 1 suggests that the spatial filtering of altimetry and mascon datasets is not consistent.

Response

The same filtering is indeed applied. However due to the lower resolution of GRACE data compared to altimetry, the GRACE-based manometric map looks smoother.

Lines 306-308: Are there systematic differences between the trends for these two periods?

Response

No there are no significant trend differences. But adding 3 years is interesting since it shows that the trends do not change, hence do not reflect short term fluctuations.

Figure 2: Is there a difference to figure 1b and 1e?

Response

Figure 2 has been deleted since it was redundant with Figure 1

Line 340-345, Couldn't there be problems with the degree 2 terms of the GRACE-processing as well?

Response

There is a consensus within the GRACE community to use such values.

Lines 370-390: I would suspect that the ARGO-data, as well as the ocean reanalysis, are referenced to the centre of figure. Even though the altimeter orbits were calculated relative to the centre of mass they may have been transferred to the centre of figure somewhere on their way to the level 4 Copernicus sea level grids.

Response

Normally, all data are expressed in the center of figure reference frame but for the altimetry data, the way the geocenter correction is performed may is still an issue (Alexandre Couhert, personal communication)

Figure 4: Trends differ quite a lot. What is the uncertainty of individual trend estimates?

Response

There is no information on individual reanalysis uncertainty. Only dispersion around the ensemble mean can be estimated.

Line 458: You decide to focus on the CIGAR model, but the results for CIGAR are not included in Table 2.

Response

This has been added

Lines 511-515: The halosteric component seems to be on the decline after 2015/2016 and to be anticorrelated with the thermosteric component. You might want to consider the budget for the subtropical and the subpolar gyre separately.

Response

We leave this for a future work that will focus on the North Atlantic

Why should the effects of a spurious drift in salinity measurements only be effective in the North Atlantic?

Response

Because it is in this region that most instrumental drifts have been detected

Figure 9: If you suspect that the PC1/EOF1 of the halosteric signal is dominated by the North Atlantic signal, why do you perform a global EOF-analysis?

Response

The figure has been deleted

III. Comment posted by Carsten Ludwigsen

This paper addresses a critical aspect of the regional sea level budget. Beyond the reviewers' comments, **I believe it's essential to discuss how reanalysis models assimilate Argo data and how potential issues in the Argo dataset might propagate into the reanalysis outputs.** Given this, I find the suggestion that GRACE may not accurately observe manometric sea level is currently insufficiently supported. The authors should provide a more detailed elaboration on these points to strengthen their analysis.

Response

This is indeed a key issue. We added the following text:

“One may wonder whether the salinity drift observed in some Argo floats as of 2015 is impacting the CIGAR reanalysis since, unlike altimetry data, T/S data are assimilated

during the reanalysis integration, thus non-linearly interacting with dynamical processes. The treatment of the salinity drift simply consisted in rejecting data that Argo had flagged for rejection in the delayed mode. But this may not fully guarantee that all bad salinity data have been discarded. However, to compute the reanalysis-based manometric component, the local steric contribution is removed. Thus any effect of the Argo salinity drift should be minimized.”