

Manuscript « Regional sea level budget over 2004-2022 »

Responses to the Reviewer 1 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 barostatic 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?

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