Response to Reviewer 1

Comment 1

Review of "North Atlantic sea level budget revisited" by Zhe Song et al. This paper reviews various contributions to sea level trend inferred over the period 2004 – 2022. The paper revisits issues of sea level components in the North Atlantic that (almost) the same authors discussed in a recently published paper. In fact, with this new paper the authors contradict them self or in other words, try to correct the previous conclusion they drew for the Noth Atlantic. In that sense I find this paper really to be a correction of the previous paper and suggest publishing it as such, not as a new science paper. Moreover, I find the revised paper not convincing in the argumentation. Many arguments remain at a "hand or arm waiving" level, and no really back-upped results and conclusions are presented. While reading the comments from previous referees I realized the same concerns, but do not find the answers satisfactory. Personally, I cannot find any new real insight, find many figures nit necessary and miss a clear convincing conclusion. My recommendation is therefore to reject the paper and encourage for either to submit it as an addendum and correction for the previous paper or invest more energy to reach clear conclusions that address all the concerns the last round of reviews raised.

Answer:

The comment made by Reviewer 1 is really surprising. This new study is by no means a correction to the Bouih et l. (2025)'s paper.

In Bouih et al., we investigated the regional sea level budget using altimetry, Argo and GRACE data, plus ocean reanalyses. We found that the basin-scale budget was almost closed in all oceans except in the North Atlantic where strong residuals were observed. But we did not provide any explanation for the North Atlantic non closure of the budget.

In the present manuscript, we focus on the North Atlantic with the objective of identifying which component (s) of the sea level budget might explain the observed strong residuals. We come up to the conclusion that: (1) GRACE-based manometric is likely in error in the Northeast Atlantic, and (2) that the North Atlantic residuals can be significantly reduced when using a reanalysis-based manometric component plus accounting for deep ocean warming (below 2000m).

It is worth to mentioned that a new study by colleagues from the USA, recently developed a new GIA model with 3-D mantle viscosity and showed that the classical GIA correction applied to GRACE (assuming radially symmetric mantle viscosity) is likely wrong in the Northeast Atlantic. Our results based on a sea level budget approach supports this statement.

That being said, we agree with Reviewer 1 that the initial version of our manuscript had too many (unnecessary) figures and in the revised version, we deleted many of them and significantly simplified the presentation.

Response to Reviewer 2

Comment 1

Line 61-63, the sentence may be rephased to be :"At the global scale, the two main contributors to the global mean sea level rise are ocean mass (known as barystatic sea level, Gregory et al., 2019) and thermosteric change."

Answer: We have revised this sentence as suggested.

Comment 2

Line 96, typo. Should be "Section 4".

Answer: Corrected. The manuscript now refers to the Discussion as section 4.

Comment 3

Section 2.1.4. Bouih et al. (2025) evaluated six ocean reanalysis products. In this study, only CIGAR and ORAS5 were selected. Please provide a clearer justification for choosing ORAS5 over the other products considered by Bouih et al. (2025), such as GLORYS, C-GLORS, SODA, and FOAM. Can you summarize the major differences between CIGAR and ORAS5 in terms of the vertical physics and data-assimilation schemes since you indicated that they may have important implications for the results presented in Figure 3.

Answer: In the revised manuscript we now only focus on the CIGAR reanalysis. There are two reasons: (1) The other reanalysis products used in Bouih et al. (2025) end in 2019, whereas the present study extends the analysis to 2022. Updating and harmonizing all these reanalysis products to 2022 would have required substantial additional processing that could not be completed within the timeframe allowed by the editor for revising our manuscript. But this will be considered in a future work. (2) Bouih et al. (2025) showed that the ensemble mean of five ocean reanalyses and the CIGAR manometric component are highly similar over 2004–2019. We therefore use CIGAR only, while extending the analysis to 2022. This is now stated in the Data section.

Comment 4

Section 2.2.1. You derived CIGAR manometric sea level. Why did not you compute ORAS5 manometric sea level? Please include some explanation.

Answer: See our response to Comment 3

Comment 5

Line 200. "this least-squares fit" should be "the linear least-squares fit".

Answer: This has been corrected

Comment 6

Line 210, "the law of error propagation, considering all terms of the sea level budget". Can you define the residuals and explain explicitly how the error is calculated? For example ,the residual = altimeter sea level - manometric sea level -steric sea level. Assuming the uncertainties of these three terms are independent, the uncertainty of the residual is calculated by adding the individual uncertainties in quadrature.

Answer: We have revised the uncertainty description. The manuscript now clearly states that residual uncertainty is estimated by quadratically summing the error of each term of the budget equation.

Comment 7

Line 228. "with the most significant difference observed near Greenland." To me, the most significant difference in Figure 1c is in the northeast Atlantic near Spain and France.

Answer: This sentence has been corrected.

Comment 8

Line 236 and 241, 251. Typo. "Figure 3a-c" should be Figure 2a-c.

Answer: Corrected through the revised figure structure. The old figure numbering and the incorrect references have been removed.

Comment 9

Line 275-276. "triggered by vertical physics and, to a lesser extent, vertical error correlations embedded in the 3D-Var scheme". Could you elaborate more on this?

Answer: This sentence was in the original version. It has been corrected.

Comment 10

Line306-307, can you cite some literatures to backup your suspicion "Given the unrealistically stationary thermosteric signal observed in ORAS5 below 2000m". What is the current observational consensus regarding deep-ocean warming below 2000 m depth? Please provide a brief summary and relevant references.

Answer: The ORAS5 reanalysis has been removed from the current manuscript. See our response to Comment 3.

Comment 11

Line 336. "Figure 6a, b shows the scatter plots between SIO gridpoint steric trend" This statement is inconsistent with the caption of Figure 6. Please double check and clarify if Figure 6 shows trends or normalized trends.

Answer: The previous scatter-plot analysis has been removed. In the current revision, we are only comparing maps.

Comment 12

Lines 339,344, 345, 350. The correlation needs to be quantified using correlation coefficients and tests of statistical significance.

Answer: The correlation-based scatter-plot interpretation has been removed. The current manuscript no longer uses this correlation argument.

Comment 13

Figure 6. Please clarify what you plotted in Figure 6. What is the unit of x and y axis? Do you normalize the trend? If so, how do you normalize the trend?

Answer: The old Figure 6 has been replaced in the manuscript.

Comment 14

Line 617-618. Maybe rephrase the sentence to be "What is the impact of using EN4 (without applying a salinity drift correction) in the assimilation procedure on the CIGAR steric component estimates?"

Answer: Corrected. The final paragraph now is "What is the impact on the CIGAR steric component estimates of the use of EN4 (with no salinity drift correction applied) in the assimilation procedure?"

Response to Reviewer 3

Comment 1

The conclusion that GRACE errors are solely responsible for the difference between GRACE and CIGAR-based manometric component and the reason why North Atlantic sea level budget cannot be closed with GRACE data might be true, but the results presented in the manuscript are not enough to support it. The authors mainly base it on the comparison between the total sea level from altimetry, steric sea level from Argo (SIO dataset, upper 2000 m only), and the manometric component from GRACE and CIGAR reanalysis. They treat the sum of the manometric and upper ocean steric component as equivalent to altimetry and conclude that the manometric component that provides a better match is better. However, they also show that the deep ocean steric component is important in North Atlantic and cannot be ignored, which makes this comparison and their conclusion flawed. Furthermore, the CIGAR-manometric component is based on a reanalysis that assimilates Argo data (from EN4), and the authors themselves demonstrate that SIO and EN4 are very similar, so CIGAR and SIO cannot be considered entirely independent. The authors should account for the deep ocean contribution into this comparison and add discussions about other possible causes for the differences to strengthen their conclusion.

Answer: We fully agree with Reviewer 3's comment. In the original version we just mentioned that the 'Altimetry minus Argo-based steric sea level' represent the sum of the manometric plus deep ocean contributions but further did not discuss any potential deep ocean contribution when comparing with GRACE manometric. In the current revision, the comparison clearly states that the deep ocean component contributes to the 'Altimetry minus Argo-based steric sea level'. We now consider both SIO hybrid case, in which SIO upper-ocean steric change is combined with CIGAR deep-ocean steric change, and CIGAR full-depth case. The residual maps explicitly show the effect of including the deep-ocean steric contribution. We also added a discussion of a possible error in the GIA correction applied to GRACE as a potential explanation for the discrepancy with CIGAR manometric.

Comment 2

Some references to datasets are missing from the manuscript. What is the satellite altimetry dataset the authors use? I can guess which one it is, but that dataset has a name (DT2021 is just the version) and a DOI, which the authors do not provide. The link the authors provide is not to the dataset but to the data store landing page. GRACE datasets also do not have references. All datasets included in this study should be clearly named and referenced.

Answer: We have corrected the "Data section" to provide clear references for all dataset and product used in this study.

Comment 3

In the Data section the authors present three Argo datasets (SIO, EN4 and JAMSTEC) and two reanalysis products (CIGAR and ORAS5), but then throughout the manuscript they pick and choose which of them are used for different comparisons without giving reasons why. They also use GRACE mascon solutions and not spherical harmonics without explaining why. Previous reviewers already asked about some of these choices and the authors responded in the rebuttal but none of these answers

were included into the revised version of the manuscript. Apart from the question already asked in the first review, I would like to know why do authors use only the SIO dataset for the steric component in Sect. 3.1 and 3.3? They explain why they exclude JAMSTEC in Sect. 3.2 (which is also not enough as it should be explained before the first time they exclude datasets), but they never explain why they do not use EN4. It often makes sense to focus on specific datasets, but then that decision needs to be explained, otherwise readers might wonder what would the result be with the other dataset and why it was not shown.

Answer: We have revised the manuscript to explain the product choices. ORAS5 has been removed from the core analysis, and the manuscript now focuses on CIGAR because Bouih et al. (2025) showed that CIGAR and the ensemble mean of five ocean reanalyses give highly similar manometric components over 2004–2019. In addition, the other reanalysis products used in Bouih et al. (2025) end in 2019, whereas the present study extends the analysis to 2022. Updating and harmonizing all these reanalysis products to 2022 would have required substantial additional processing that could not be completed within the timeframe allowed by the editor for revising our manuscript. But this will be considered in future work. For the Argo products, the revised Results section first compares SIO, JAMSTEC, EN4, and CIGAR in the upper 2000 m before selecting the product used in the subsequent budget analysis. We now explain that JAMSTEC is not retained because its halosteric time series shows a spurious (unexplained) positive anomaly during 2008–2012, while EN4 is not retained because (unlike SIO) it does not apply the salinity drift correction that likely causes the observed strong halosteric decrease after 2016. SIO is therefore retained for the following analysis because it applies a correction for the Argo salinity drift. In addition, Figure 3 has been revised relative to the previous version to better show how the different data sets are affected by large-scale/global signals and how the salinity drift in EN4 and JAMSTEC can affect the regional North Atlantic halosteric component.

Comment 4

Sect. 3.5, which is the most important in the manuscript, as it is showing attempts to close the sea level budget using different datasets, is not well organized and it contains too much irrelevant information, while not showing important information in a clear way. Four figures (Figs. 9, 10, 12, 13) show a side by side comparison of the sea level budget with the steric component from SIO and CIGAR, which has very minor differences. At the same time, the difference with and without the inclusion of the deep ocean, and the difference between GRACE and CIGAR manometric, which are relevant for the main conclusions of the manuscript, can only be analysed by comparing multiple figures, which is rather hard. I would strongly advise the authors to reduce the number of figures in this section by merging Figs. 9 and 10, as well as Figs. 12 and 13, after picking one of the steric components. Figures with the other steric component can be shown in the appendix for completeness.

Answer: We fully agree with Reviewer 3 that this section was not clear enough. In particular we presented sea level budget curves with the global mean trends included which was outside the objective of the present study (ie, trying to close the regional budget with the global mean trends removed). All corresponding figures have been removed ad this section has been greatly simplified. The revised section now focuses only on the budget time series using CIGAR manometric sea level combined with the SIO-hybrid and CIGAR full-depth steric configurations (global mean trends removed).

Comment 5

The structure of the manuscript is overall not bad but it could definitely use some improvements. Originally, the authors first presented individual components (Sect. 3.1-3.2), and then compared their combinations (Sect. 3.3-3.5), which is a good structure, but in the revised manuscript they added a comparison with altimetry minus steric component into Sect. 3.1, which does not make sense. I stronly advise to remove this comparison and go back to the original structure. As the conclusions about GRACE vs. CIGAR manometric component cannot be drawn by comparing them to altimetry minus SIO dataset, the authors should either add the deep ocean steric component in Sect. 3.3 and include multiple steric products, as they do elsewhere, or completely remove this section and move their discussion about the benefits of CIGAR manometric over GRACE to (currently) Sect. 3.4 and 3.5. It has more impact if shown that CIGAR manometric works better when combined with all steric component products than if that conclusion is based on only one product.

Answer: We agree and have revised the structure. The current "Results section" firstly presents the upper ocean steric comparison, then the full-depth steric comparison, then the manometric comparison, and finally the residual maps and time series. The altimetry minus steric comparison now uses full-depth steric estimates and is placed in the manometric comparison part of the Results.

Comment 6

49 "by a factor of 2" This has been corrected in the conclusion in the last revision, but not here.

Answer: Corrected. We have revised the wording to express the reduction as a percentage. The manuscript now states that including the deep-ocean steric contribution reduces the residual by about 30% when using the GRACE manometric component with SIO upper-2000 m steric sea level, and by about 70% when using the CIGAR manometric component together with the CIGAR full-depth steric component.

Comment 7

122-125 "Additionally, the GAD product derived from AOD1B models (Dobslaw et al., 2017; Flechtner et al., 2014), which represents non-tidal atmospheric and oceanic mass redistribution, is added back over the ocean areas to restore the ocean bottom pressure, which combines the effects of ocean mass and atmospheric loading." Why use AOD1B RL06, when there is a newer version released in 2022: https://doi.org/10.1029/2022JB024360? Especially for a paper that in the title includes word "revisited" it is important to use more recent information than the previous papers covering the same topic. If there is a reason for not using the newest release, e.g., this part of processing is already included into the mascon solution, that should be noted in the text. You should also specify which parts of the processing was performed by you and which is part of the dataset.

Answer: We used the GAD fields distributed as auxiliary products with the mascon solutions to keep post-processing consistent with the mascon products. Therefore, we did not independently replace the GAD correction with another AOD1B release. In the mascon products used here, the GAD-related correction is already part of the product processing. What we performed in this study was the additional adjustment needed to make the GRACE/GRACE-FO ocean mass data consistent with altimetry-based sea level data, which are corrected for the inverse-barometer

response. This processing step is already described in the manuscript. This additional processing was performed by us.

Comment 8

129 "In this study, we use the ensemble mean of the three mascon solutions." How different are the mascon solutions and would perhaps individual solutions achieve a better result? It would strengthen the conclusions if you can demonstrate that you considered differences in GRACE products.

Answer: We do not show results for individual mascon solutions because as shown previously in Bouih et al. (2025), all three mascon solutions give very similar results.

Comment 9

Sect. 2.2.1 Computation of the manometric component: The explanation of the term "manometric" should come before explaining the methods how to obtain it.

Answer: We have moved the explanation of the term "manometric sea level" to the beginning of Section 2.2.1, before describing the GRACE/GRACE-FO-based and CIGAR-based methods used to estimate this component.

Comment 10

Sect. 2.2.2 Post processing of the data: It is not entirely clear whether post-processing of the data, particularly the interpolation to a 1-degree grid, was performed before or after computing the manometric component. Presumably before, to be able to combine different datasets, but then it would also make more sense to have this section before the previous one.

Answer: We have revised the post-processing section to clarify the order of operations. The interpolation step has been moved to the end of the workflow description, after the component-specific processing steps.

Comment 11

Figs. 1 and 5: They are essentially the same, use the same products and the only difference is whether you subtract manometric or steric component from altimetry. It is redundant to show both.

Answer: The redundant comparison has been removed. The revised manuscript now presents a more focused full depth manometric comparison in Figure 4 and residual maps in Figure 5.

Comment 12

Sect 3.2 Just a detail, but since Sect 3.2 is split into 3 parts (Argo products, reanalysis products and timeseries for both) why do reanalysis products and time series get their own sections (3.2.1 and 3.2.2), and Argo products do not? Perhaps there should be a Sect 3.2.3 here.

Answer: The section structure has been revised. The current Results section separately presents upper-ocean steric products, full-depth steric products, and upper-ocean steric time series from Argo products and reanalysis products.

Comment 13

Fig. 3: It would be nice if it was written on the figure that the two left columns are the upper 2000 m and the below 2000 m contributions. Same for other figures with many subplots, it is hard to analyse them while looking at very long lists of letters in the caption describing what each subplot represents.

Answer: The figure organization has been revised. The current revision separates the upper-ocean steric maps and the full-depth steric configurations into different figures, and the captions now explicitly state the depth ranges.

Comment 14

328-332 "From Figure 5, we clearly observe that the spatial trend pattern of SIO is more consistent with the "Altimetry minus CIGAR manometric" estimate, as both exhibit positive signals in the western North Atlantic. In contrast, the "Altimetry minus GRACE manometric" estimate displays a prominent positive signal in the eastern North Atlantic which is absent in the SIO data." Altimetry - GRACE also has a positive signal in the western North Atlantic, that is not what makes it differ from SIO, it is its positive signal in the east.

Answer: The previous Figure 5 and the associated statement have been removed. In the current revision, the comparison is based on altimetry minus full-depth steric sea level, and the text now focuses on the eastern North Atlantic difference.

Comment 15

379-381 "As shown in Figure 7a,e, the resulting residuals implicitly include a deep ocean steric contribution, which is not accounted for since the steric data are limited here to the upper 2000 m." This sentence is not clear. 1. How is that seen from the figure? Is it specifically in a and e, and not in others? Why?

Answer: We have rewritten this part. The current text directly states that the upper-ocean residuals implicitly include the deep-ocean steric contribution, and then compares these residuals with the full-depth cases.

Comment 16

418 "For all cases except the ORAS5 deep ocean case," It might be worth explicitly stating that ORAS5 does not have much signal in the deep ocean, as shown earlier.

Answer: The ORAS5 data set has been removed from the current manuscript. See our above response.

Comment 17

425-426 " when using CIGAR for both the steric (full depth and manometric component)." This is phrased wrong, manometric is not part of steric contribution, which this sentence implies. Ending bracket is probably in the wrong place.

Answer: We agree with the Reviewer. We have revised this sentence to clearly separate the CIGAR full-depth steric component from the CIGAR manometric component. The revised

sentence states that the residual is reduced by about 70% when using the CIGAR manometric component together with the CIGAR full-depth steric component.

Comment 18

429-431 "Notably, the sea level budget is effectively nearly closed within error bars when combining CIGAR manometric and CIGAR steric data." This may be true, but cannot really be seen from the discussed Fig. 8, as it does not include error bars. It is also hardly surprising when most components are from the same dataset and dependent on each other.

Answer: We have revised the relevant figure and caption. The current Figure 6 shows the residual time series with shaded uncertainty ranges, and the text states that the CIGAR full-depth case is closed within data uncertainties.

Comment 19

455-458 "Since the CIGAR manometric component does not fully account for the external sea water mass addition to the ocean due to land ice melt and terrestrial waters, we adopt GRACE data to represent the total ocean mass manometric sea level change (i.e., global mean trend accounted for)." I do not understand this. What is different here compared to using GRACE vs CIGAR manometric component in previous sections?

Answer: This confusing global mean trend included section has been removed. The current time-series analysis focuses on global mean trend removed components.

Comment 20

476-478 "For SIO, a slight halosteric decrease is observed over 2012-2015, with a linear trend of -1.10±0.94 mm/yr during the such period, then it increases to -3.20±0.9 mm/yr over 2016-2022." It seems rather stable in the 2012-2015 time span, same as CIGAR, and they both have a negative trend from 2016. The 2015-2015 "trend" is probably just a result of the peak in 2013 that is more prominent in SIO than in CIGAR, but calculation of trend over 3 years is not very meaningful.

Answer: This part has been removed from the revised manuscript. The previous time-series budget figure and discussion with global mean trends included have been deleted. Therefore, the statement about the 2012–2015 SIO halosteric trend is no longer present. The revised time-series budget section now focuses only on the global-mean-removed case.

Comment 21

606-608 "(3) As of 2012, a negative trend is observed in the SIO halosteric component which suggests a physically real increase in salinity of the North Atlantic. Again, this needs further investigation." It really only starts in 2016, in all Argo datasets.

Answer: Corrected. The manuscript now describes that EN4 and, to a lesser extent, SIO and JAMSTEC show a strong decrease as of 2016, possibly linked to salinity drift together with a potential physical salinity increase.

Comment 22

625-626 "The North Atlantic sea level budget is closed within data uncertainties when using the CIGAR reanalysis for both the manometric and steric components." Isn't that at least partially a consequence of the way reanalysis is produced? It is supposed to be consistent with itself.

Answer: Indeed we cannot exclude this. However, in the reanalysis, the ocean mass redistribution (manometric component) and steric component may be two independent parameters.

Comment 23

635-638 "Why GRACE manometric data give less good results in the Northeast Atlantic than the CIGAR-based manometric sea level? Which inaccurate correction applied to GRACE is involved? Is it related to the GIA or the geocenter corrections applied to GRACE?" At least some of these questions could be answered in this paper by comparing the results of different GRACE products.

Answer: In the revised version, we quote a new study by colleagues from the USA, who recently developed a new GIA model with 3-D mantle viscosity and showed that the classical GIA correction applied to GRACE (assuming radially symmetric mantle viscosity) is likely wrong in the Northeast Atlantic. Our results based on a sea level budget approach supports this statement.