North Atlantic Sea level budget revisited (Song et al.)

Responses to the Reviewers

In bold italics

(the modified text is also shown in underlined bold italics between "…")

I. Reviewer 4's comments and responses

The revised manuscript entitled "North Atlantic sea level budget revisited" has been significantly improved. In particular, the globally averaged trend was consistently removed throughout the analysis. I therefore recommend minor revisions. My detailed comments are provided below.

We thank Reviewer 4 for these minor comments.

1. Line 37, "This was observed but not in this study, …" should be "In this study, …"

Corrected

2. Line 48, " With manometric, the budget is closed within data uncertainties." I found this sentence is not clear. It may be revised to be "The budget can be closed within the data uncertainties when CIGAR manometric and full steric sea levels are included."

Corrected

3. Line 131, change "withdraw" to "remove"

Corrected

4. Figure 2. Panels e and f are not discussed. The main difference shown in panels e and f is in subtropical gyre between 20-40 degree.

A sentence has been added

5. Line 198-199. "Regarding the trend uncertainty, it is expressed as the standard error of the least-squares fit." Since a three-month moving-average filter was applied to the time series, the effective number of degrees of freedom is reduced when estimating the standard error of the least-squares fit. Was this effect taken into account? If not, the trend uncertainties may be underestimated. In that case, the uncertainty estimates reported in Table 1 should be revised, and the associated errors may be larger than those currently reported.

The trend uncertainties were indeed computed before smoothing. Therefore, the uncertainty values follow the reviewer's requirement.

6. Line 366. "… allows to almost close regional sea level budget." Are you referring to Table 1? If so, please explicitly cited the table.

Reference to Table 1 has been added

7. Line 372 "We can thus conclude that the closure of the sea level budget holds within the data uncertainties in this case." This statement is not consistent with previous statement in Line 366. It may be revised to be "We therefore conclude that, although a small residual remains, the sea-level budget closure is achieved within the data uncertainties for this case."

Corrected

II. Reviewer 5's Comments and Responses

The manuscript examines the North Atlantic sea level budget for 2004-2022 time span, comparing estimates of manometric and steric contributions from GRACE, Argo floats and ocean reanalysis with satellite altimetry data. The authors find that using the manometric component from the CIGAR reanalysis instead of GRACE helps with closure of the North Atlantic sea level budget and that using deep ocean steric contribution reduces the North Atlantic sea level budget residuals by 30. This is the third round of review overall, and the second time I have reviewed the manuscript.

The manuscript could potentially have scientific significance and its quality has improved substantially since the previous round of review, but there are still several issues that were not addressed properly or at all, as well as some new ones I discovered. If this was in an earlier stage of the review process, I would recommend another revision, however, as it is already the third review and the reviewers are specifically asked to assess whether the manuscript is acceptable for final publication, I have to say that no, it is definitely not.

A major problem is that the authors have not adequately addressed the reviewers' comments in the revision and rebuttal. Except for one comment, the 'final author reply to the editor' does not quote the relevant revised or newly added text in the manuscript, despite the OS's instructions for submission of revised manuscript clearly stating that it should. Having to search the manuscript for changes corresponding to each comment makes it unnecessarily difficult and time-consuming to assess whether the comments have been adequately addressed. Nonetheless, there are multiple instances of the authors responding only to part of a question, responding to a question in the rebuttal but not in the manuscript or claiming that they modified the manuscript to address it, but there are no such changes in the manuscript. These are not necessarily all issues present in the manuscript, as the structure of the rebuttal does not allow me to systematically verify all changes within the available review time, but they are sufficient for me to make a recommendation. Below is the list of all issues from the last review that the authors did not address properly, as well as of some new issues I detected. I recommend the authors to take their time to fully address them before re-submitting their manuscript.

We respond below to each comment of Reviewer 5 and show how the manuscript has been modified.

1. Part of comment 1: "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."

Response:

Any product based on in-situ data will contain some structural cross-dependence, because the raw measurements at the origin are obviously the same. The manuscript shows (e.g., Figure 2) that large-scale patterns are similar, but many small-scale patterns and the quantitative assessment may differ between CIGAR and SIO, or between SIO and EN4. This is because, intrinsically, the products use different quality control, observational sub-sampling, and mapping/interpolation (horizontal and vertical correlation) procedures; additionally, in the reanalyses, other factors connected to the model physics and atmospheric forcing further shape the time evolution of the ocean state. Disentangling all these effects is prohibitive and beyond the scope of the present study; however, we added a discussion of this at the end of section 2.1.4 (lines 170-177):

"Note that, while the CIGAR reanalysis ingests the same T/S measurements as SIO and EN4 (specifically using the EN4 raw in-situ profile dataset), the reanalysis adopts different quality control, observational sub-sampling, mapping (horizontal correlation) methods, and vertical propagation; additionally, in the reanalysis, other factors connected to the model physics (e.g., vertical mixing) and atmospheric and river forcing further shape the time evolution of the ocean state. The reanalysis thus provides a generally different realization of the ocean state evolution than the other products. Vertical covariances and dynamical processes contribute to the evolution of the deep ocean, unlike observation-only products."

2. 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.

Response:

The gridded altimetry-based sea level data from Copernicus are NOT associated with a reference to authors. Link to the Copernicus web site (https://cds.climate.copernicus.eu/) quoted in the previous version was perfectly ok because it leads (from the Search window) directly to the gridded C3S sea level data set. However, in this revised version, we now quote the following reference in section 2.1.1 (lines 94-95):

"(Copernicus Climate Change Service, Climate Data Store, 2018; Accessed on 15-11-2025)".

And added the following reference in the reference list:

"Copernicus Climate Change Service, Climate Data Store, Sea level gridded data from satellite observations for the global ocean from 1993 to present. Copernicus Climate Change Service (C3S) Climate Data Store (CDS). DOI: 10.24381/cds.4c328c78, 2018."

3. GRACE datasets also do not have references.

Response:

We disagree with such a comment. For each GRACE mascon data set a reference was indeed quoted in the previous version, with the DOI given inside the reference →

"…the Center for Space Research (CSR, Save et al., 2016), the Jet Propulsion Laboratory (JPL, Watkins et al., 2015), and the Goddard Space Flight Center (GSFC, Loomis et al., 2019)".

In this revised version (lines 470-483), we have added a 'Data Availability Section' in which web links are given:

"Data Availability Statement

All data sets used in this study are publicly available and can be accessed from the following sources. The DT2021 gridded altimetry data set is available at Copernicus Climate Change Service, Climate Data Store (2018).

The Argo-based temperature data used to compute the thermosteric time series come from different sources: (a) SIO, data available from https://argo.ucsd.edu/data/argo-data-products, (b) JAMSTEC, data available from http://www.jamstec.go.jp/ARGO/J ARGOe.html., (c) EN4, data available from https://www.metoffice.gov.uk/hadobs/en4/

The GRACE, GRACE FO mascon data are available from Save et al. (2016) for CSR (http://www2.csr.utexas. edu/grace), Watkins et al. (2015) for JPL (http://grace.jpl.nasa.gov/) and Loomis et al. (2019) for GSFC (https://ea rth.gsfc.nasa.gov/geo/data/grace-mascons)

Description of the CIGAR data set, instructions, and data sets are available through the website https://cigar.isma r.cnr.it »

4. All datasets included in this study should be clearly named and referenced." - Authors claim in the rebuttal that they modified the Data section to provide clear references for all datasets and products used in their study, but they did not. In fact, there are no changes in Sect. 2.1.1 and 2.1.3 of the revised manuscript nor the track changes file, which describe satellite altimetry and Argo datasets. There is also, e.g., no DOI 10.1007/s00190-019-01252-y, which belongs to the article that accompanies the monthly GRACE GSFC mascon solutions.

Response:

This comment is similar to the above comment 3.

In the previous version, we indeed provided references to the three mascon solutions and we would like to remind that the DOIs are given in these references. For example:

Loomis, B.D., Luthcke, S.B. & Sabaka, T.J. (2019) Regularization and error characterization of GRACE mascons. J Geod 93, 1381–1398. https://doi.org/10.1007/s00190-019-01252-y

As mentioned in our response to comment 3, we now have added a "Data Availability Section at the end of the text (lines 470-483):

"Data Availability Statement

All data sets used in this study are publicly available and can be accessed from the following sources. The DT2021 gridded altimetry data set is available at Copernicus Climate Change Service, Climate Data Store (2018).

The Argo-based temperature data used to compute the thermosteric time series come from different sources: (a) SIO, data available from https://argo.ucsd.edu/data/argo-data-products, (b) JAMSTEC, data available from http://www.jamstec.go.jp/ARGO/J ARGOe.html., (c) EN4, data available from https://www.metoffice.gov.uk/hadobs/en4/

The GRACE, GRACE FO mascon data are available from Save et al. (2016) for CSR (http://www2.csr.utexas. edu/grace), Watkins et al. (2015) for JPL (http://grace.jpl.nasa.gov/) and Loomis et al. (2019) for GSFC (https://ea rth.gsfc.nasa.gov/geo/data/grace-mascons)

Description of the CIGAR data set, instructions, and data sets are available through the website https://cigar.isma r.cnr.it »

5. Part of comment 3: "They also use GRACE mascon solutions and not spherical harmonics without explaining why." - The authors explain this in another comment, but completely ignore it here. More importantly, they do not implement this answer into the manuscript.

Response:

Most articles that use GRACE mascons do not justify why they use the official GRACE project products from PODAAC rather than alternative solutions based on spherical harmonics (SH), including some developed by independent reserach groups. However, to satisfy Reviewer 5, we added the following sentence in the text (section 2.1.2, lines 108-113):

« In Bouih et al. (2025), we used, in addition to the mascon solutions, an ensemble of 60 SH solutions (update of Blazquez et al., 2018). The sea level budget residuals based on this ensemble mean SH solution showed a strong north–south signal in the mid-North Atlantic, due to north–south stripe noise affecting SH solutions (see Blazquez et al., 2018 for a discussion). For that reason, we focus here on the mascon solutions. »

6. Comment 7: "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." - The authors explain the processing steps in the rebuttal and claim that the additional processing step they performed is already described in the manuscript, but the

manuscript does not differentiate which processing steps are a part of the downloaded mascon solution and which are performed by the authors.

Response :

The atmospheric and oceanic de-aliasing model (AOD1B RL06) is natively applied by the data centers during the Level-1B to Level-2 orbital data inversion stage. As end users of distributed Level-3 mascon products, it is technically incoherent to replace AOD1B RL06 with the newer AOD1B RL07 version retrospectively, as this would require the raw satellite range-rate measurements to be reprocessed in full. AOD1B RL07 has not yet been integrated into the actual mascon releases by the processing centers.

We have updated Section 2.1.2 to clarify both this technical constraint and the exact division of processing steps between the data providers and our study. The new text is copied below (lines 124-131):

« However, to align GRACE data with altimetry-based sea level (which is corrected for the inverse barometer effect), we convert ocean bottom pressure into manometric ocean mass by subtracting the monthly global ocean-mean atmospheric mass from every grid cell, using the spatial mean of the AOD1B RL06-based GAD product corresponding to each mascon solution (Chen et al., 2019). Note that using the AOD1B RL06 GAD product rather than any updated version is mandatory to maintain strict internal consistency with the background de-aliasing model applied during the RL06 mascon processing. »

7. Comment 8: "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." - The authors reply in the rebuttal, but do not implement the answer into the manuscript. If a decision is made based on the results of another published study, that should be explicitly written in the manuscript, including a reference to that study.

Response:

In the Supplementary Information, we have added figures of the GRACE-based manometric trends for each individual mascon solution. All three individual solution show rather similar large-scale patterns, in particular negative manometric trends in the northeast Atlantic.

8. 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." - The authors claim in the rebuttal that they revised the post-processing section to clarify the order of operations, but there are no such changes in said section in the revised manuscript, nor in the track changes file.

Response:

We added the following sentence in section 2.2.1 (lines 135-136)

« The CSR, JPL and GSFC products are provided with 0.25°, 0.5° and 0.5° resolution, respectively. They are further interpolated on 1°x1° grids before computing the ensemble mean »

In section 'Post-processing of the data' (now section 2.2.3), we added (lines 225-226):

« Post-processing was performed on such 1°x1° monthly grids. »

9. Part of comment 18: "It is also hardly surprising when most components are from the same dataset and dependent on each other." - The authors respond to the rest of this comment, but ignore this particular statement.

Response:

This comment is similar to Comment 1.

While the manometric and steric sea level in the reanalysis cannot be considered fully independent due to their dynamical interactions within the ocean general circulation model, the only constraint in the reanalysis comes from in-situ T/S data (no altimetry is assimilated) and indirectly from atmospheric forcing and model physics, meaning that the reanalysis is free to adjust the partitioning of sea level variations into steric and manometric components according to the ocean physics and the assimilation framework.

Additionally, please note that the effect of in-situ data assimilation on the manometric sea level variations is very indirect: the manometric component receives no direct observational constraint, and is affected by data assimilation only through the compensating adjustment of the free surface to density increments, so between CIGAR-manometric and SIO there exists factors of independence associated with the different processing, sampling and spread of in-situ observation information, and with the different use of them (either in direct steric estimates, as in SIO, or implied indirect adjustments, as in CIGAR manometric).

We have added a comment on that in section 2.1.4 (lines 170-177)

« Note that, while the CIGAR reanalysis ingests the same T/S measurements as SIO and EN4 (specifically using the EN4 raw in-situ profile dataset), the reanalysis adopts different quality control, observational sub-sampling, mapping (horizontal correlation) methods, and vertical propagation. Additionally, in the reanalysis, other factors connected to the model physics (e.g., vertical mixing) and atmospheric and river forcing further shape the time evolution of the ocean state. The reanalysis thus provides a generally different realization of the ocean state evolution than the other products. Vertical covariances and dynamical processes contribute to the evolution of the deep ocean, unlike observation-only products. »

And in section 2.2.2 (lines 211-220) :

« As mentioned above, the CIGAR reanalysis assimilates in-situ T/S profiles. This would mean that the steric and manometric sea levels are not fully independent within the reanalysis due to their dynamical interaction. However, the CIGAR-based manometric component is estimated from the sterodynamic variable that includes both ocean mass redistribution estimated by the model and steric contribution (the latter being further subtracted from the sterodynamic variable). Because, the reanalysis is free to partition sea level variations into steric and manometric based on model dynamics, without altimetry ingestion, the manometric component constitutes a quasi-independent realization. »

10. Comment 22: "Isn't that at least partially a consequence of the way reanalysis is produced? It is supposed to be consistent with itself." - (1) The answer in the rebuttal does not answer my concern. What does "may be" independent mean? Are they confirmed to be independent, do you not know, do you assume they are? Do you not compute the manometric component by subtracting steric from sterodynamic? How are they possibly independent? (2) This should be discussed in the manuscript, not just in the rebuttal. Generally, the answer to reviewers' questions should be implemented into the manuscript, or the authors should explain in the rebuttal why they do not consider it appropriate to include it. Simply answering the question in the rebuttal is not sufficient; if a reviewer wanted to know the answer, so will some readers.

Response:

Part of comment 10.1 'Are the components independent?***' is the same as comment 1 and comment 9. We already responded in detail to this issue and do not reproduce our response here (see response to comment 9).***

About comment 10.1 asking the question 'Do you not compute the manometric component by subtracting steric from sterodynamic?': ***A detailed response is provided in comment 13 below.***

'

11. Some sentences do not make sense in the revised version, perhaps parts of the text were lost while editing. E.g., " This was observed but not in this study, we revisit the North Atlantic sea level budget, using satellite altimetry, GRACE..." (L37-38); "With manometric, the budget is closed within data uncertainties." (L48); "... the EN4 curve (and to a little extent also SIO and JAMSTEC) shows a strong decrease as of 2016, possibly linked to the salinity drift in EN4 and JAMSTEC data sets, to together with a potential physical salinity increase." (L251-253). The authors should check the revised manuscript carefully to avoid such issues.

Response:

This has been corrected.

About the JAMSTEC curve, we now write (section 3.2, lines 312-315):

« The EN4 curve (and to a little extent the other two datasets) shows a decrease as of 2016, possibly linked to the salinity drift reported in some Argo floats (and not corrected in EN4 and JAMSTEC data) , although a potential physical salinity increase cannot be excluded. »

12. Since both GRACE mascons and the satellite altimetry dataset are anomalies relative to a specific time period (2004.0-2009.999 for JPL mascons, possibly 1993-2012 for altimetry, if I am guessing correctly which dataset is used) how are different reference periods handled in this study when comparing products? This needs to be included in the manuscript.

Response :

We thank the reviewer for pointing out this missing detail in the text. Indeed, raw input datasets are distributed with different native baseline reference periods (e.g., 1993–2012 for C3S altimetry, 2004–2009 for JPL mascons).

To ensure direct physical comparability across all datasets, every time series and gridded fields were systematically re-referenced to a single, common baseline period (2004–2009) in the post-processing phase. Concretely, for each dataset, the temporal mean calculated over 2004–2009 was subtracted from each grid point.

We have now explicitly described this step in Section 2.2.3 (Post-processing of the data; lines 234-236):

"To allow consistent comparison and budget combination across datasets, all gridded anomaly fields and time series were systematically re-referenced to a common baseline period (2004–2009) by subtracting their respective temporal mean over this timeframe."

13. Sect. 2.2.1 (1) The explanation of how the manometric component is computed from the reanalysis is not clear enough. You explain subtracting local steric effects but do not explain how you obtain these local steric effects. You should include that part. (2) Using a manometric component directly from a GRACE dataset is not really a method of computing the manometric component, so this section should be limited to specifically calculation of the manometric component from other components. (3) Unrelated to the manometric component, you should include some text about the calculation of the steric component in general, not just in the Argo data section. Is the steric component from the reanalysis calculated in the same way?

Response:

Comment 13.1: We now explain in detail how we compute the steric component in the CIGAR reanalysis and how it is subtracted from the sterodynamic variable to obtain the manometric component.

Section 2.2.1 is now dedicated to the steric computation method, for both Argo and CIGAR. The computation of the manometric component is described in the following section 2.2.2.

The new text is about the computation of the steric component (section 2.2.1, lines 180-188) is :

"The thermosteric, halosteric, and total steric sea level time series are computed from the gridded temperature and salinity products (both reanalyses and gridded observation products) using the Gibbs SeaWater (GSW) Oceanographic Toolbox (McDougall et al., 2011), which implements the Thermodynamic Equation Of Seawater (TEOS-10) standard. Specifically, 3D density anomalies are vertically integrated to obtain the total steric sea level; thermo- and halo- steric sea levels are obtained similarly, using density anomalies at constant salinity or temperature, respectively. See https://link.springer.com/article/10.1007/s00382-015-2554-9 for further details."

In section 2.2.2 (computation of the manometric component), we added the following text (lines 200-202)

« To isolate the manometric component, at every grid mesh, we subtract the local steric value from the sterodynamic variable, both being computed at the same grid mesh. »

Comment 13.2 : We disagree with Reviewer 5 who suggests to remove the few lines about GRACE manometric. We think it is important for the reader to be informed on the two sources of manometric component estimates.

Comment 13.3 : As explained in our response to comment 13.1, we detail how the steric component is computed for both Argo data and CIGAR (addition of a new section, now 2.2.1).

14. L193-195 "For the GRACE-based manometric component, uncertainties can be derived from the dispersion of the data sets with respect to the ensemble mean." Those are not the only uncertainties of GRACE data. Do mascon solutions not have uncertainty estimates? I am pretty sure they do. Why do you not use them?

Response :

Individual mascon products are indeed not provided formal error estimates related to their own specific processing (inversion constraints, interpolation technic, etc.). However, we chose to derive uncertainties from the ensemble dispersion across the three processing centers (JPL, CSR, GSFC) for three main reasons:

1. ***Formal errors quantify only inversion noise: The formal errors provided in mascon files capture instrumental noise and inversion constraints. They do not account for structural/systematic processing choices (e.g., degree-1/geocenter, GIA, background models), which dominate the overall uncertainty budget (Blazquez et al., 2018).***
2. ***Underestimation due to missing error covariances: Mascon files do not provide full spatial and temporal error covariance matrices. Propagating grid-point formal errors assuming spatial independence severely underestimates the uncertainty over a large basin like the North Atlantic, yielding unrealistically small trend uncertainties (< 0.1 mm/yr).***
3. ***Consistency with our ensemble approach: As end-users, we have no objective basis to privilege one mascon processing strategy over another. Adopting the ensemble mean of JPL, CSR, and GSFC is therefore the most robust and neutral strategy. Consequently, using the ensemble spread naturally reflects the uncertainty of our central estimate by capturing the dispersion among these equally plausible solutions.***

Using the ensemble spread is the standard recommendation in sea level budget assessments (e.g., Blazquez et al., 2018; WCRP Sea Level Budget Group, 2018; Barnoud et al., 2021), as it encompasses the structural processing uncertainty inherent to GRACE processing choices.

We have revised the text to explain this rationale clearly to the reader.

Revised text in Section 2.2.4 (lines 241-250):

"While formal error estimates are provided with individual mascon solutions (e.g., Watkins et al., 2015), they primarily quantify inversion noise and lack spatial error covariance information, leading to overly optimistic basin-wide uncertainties (< 0.1 mm/yr). Furthermore, as end-users, we have no a priori ground to consider one mascon solution superior to the others. We therefore adopt the ensemble mean of the JPL, CSR, and GSFC solutions as our main manometric dataset. In accordance with established community standards for sea level budget assessments (Blazquez et al., 2018; WCRP, 2018; Barnoud et al., 2021), we estimate the manometric uncertainty from the ensemble dispersion across the three processing centers, which naturally captures the structural processing uncertainty among these equally plausible products."

15. L271-272 "These maps are supposed to represent the manometric component, plus the deep ocean contribution." Is the deep ocean contribution not removed? The previous sentence states it is "altimetry minus steric full depth". Also, "supposed to represent" is a strange and too informal phrasing. Do they represent or are they supposed to represent but actually do not?

Response: Thanks for the reviewer to point out thi error. This is a typing error; the correct statement is "These maps are supposed to represent the manometric component."

We have corrected the error.

16. Section titles are generally too long, they should be labels to distinguish the sections, not summaries. Also, Sect. 3.4.1 should not exist, as there is no Sect. 3.4.2; sub-levels should only be used if the level is split into multiple parts.

Response:

We do not find that section titles are too long.

We have deleted subsection 3.4.1, only keep section 3.4

17. Introduction could use some improvements. The context of introducing the Bouih et al. (2025) study should precede the details given about it, and it should be written more clearly what are the differences between it and your study and why.

Response:

We do not understand at all this comment. The first 9 lines of the introduction only summarize the main contributions of the global and regional sea level budgets. No result based on Bouih et al. (2025) are mentioned in this first few lines.

Besides, the difference between the Bouih et al (2025) study and our study is clearly mentioned. The strong North Atlantic residuals are noticed but not explained. Investigating the origin of non closure of the sea level budget in the North Atlantic was the objective of our study. In the previous version (and in this revised version as well), it is written (lines 81-84):

« Our objective is to evaluate the impact of each product on the currently reported non-closure of the North Atlantic sea level budget. An important addition, compared to the Bouih et al. (2025)'s study, consists of accounting for the deep ocean warming below 2000m (not sampled by Argo) using estimates from the ocean reanalysis. »

18. L378-387 This long list of questions at the end of the paper is very unusual for a scientific research article. It would be more appropriate to synthesize the main implications and limitations of the study, rather than presenting a large number of questions that are left unanswered. In particular, the questions pertaining to something that has been at least partially addressed in the manuscript (GIA correction) should not be repeated as fully open questions, while some other should also be discussed earlier.

Response:

The list of questions has been removed. We replace it by the following sentences (lines 446-452) :

"Nevertheless, some issues still merit deeper investigation. This is particularly the case for the manometric component which shows opposite trends in the Northeast Atlantic when GRACE or CIGAR are considered. Another important issue concerns the exact contribution of the deep ocean. Use of CIGAR (full depth) steric data improves closure of the sea level budget. But whether deep ocean warming still plays a significant role needs to be confirmed using Deep Argo data. Clearly, future studies should be devoted to try answering these remaining questions."