Authors' response to reviewer 3 (Riccardo Riva)

We thank the reviewer for the constructive and insightful comments which helped us to improve the manuscript. Following the reviewer's comment on line 495 we also updated our analysis for GRACE ocean mass change (see below).

In the following we respond to the specific comments. (Reviewer comments are repeated in *italic.*)

Line 93: in the list of recent studies, I miss Frederikse et al. (Nature 584, 2020). Note that their numbers (Table 1) are very similar to those presented in the current manuscript.

We included reference to Frederikse et al. (2000). In Section 7.1 we discussed the good agreement between Frederikse et al. (2000) and our study.

Line 158: though it is common practice, I am still not convinced that it is correct to use the density of freshwater when computing sea level change induced by continental freshwater fluxes. Freshwater will probably mix rather quickly (surely when looking at global values over long times) and salt content will cause a reduction in water volume. It is a somehow small effect, hence below the uncertainty level, but it could at least be mentioned either here or in the later discussion of unaccounted error sources.

We mentioned the issue of different water densities used in the literature in Section 3.8. We now refer to this discussion right at the introduction of ρ_W at the place addressed by the reviewer.

Line 190: the explanation of the use of "GlobalOcean" vs. "Ocean" is rather unclear.

We agree that this formulation was unnecessarily complicated. We modified the notation and its explanation. It now reads:

"Based on such assumptions, $\Delta\mathrm{SL}_{\mathrm{source}}$ may be evaluated as

$$\Delta\mathrm{SL}_{\mathrm{source}} = \frac{1}{\rho_{\mathrm{W}}} \langle \Delta\kappa_{\mathrm{source}} \rangle_{\mathrm{Ocean65}}, \tag{8}$$

where $\langle\cdot\rangle_{\mathrm{Ocean65}}$ denotes the averaging over the ocean area between 65°N and 65°S. Here we assume $\langle \Delta\kappa_{\mathrm{source}} \rangle_{\mathrm{Ocean65}} = \langle \Delta\kappa_{\mathrm{source}} \rangle_{\mathrm{Ocean}}$."

Line 279: it would be nice to show the mentioned trend of 3.05 ± 0.24 mm/yr in Figure 1a (both the straight line and the shaded uncertainty). It would help visualizing the presence of non-linear changes.

Our idea is to present the time series and time-variable uncertainties for all budget elements in the same style (Fig. 1, 2, 4, 5, 6, 7, 8). Partly, it would complicate readability if we added the line / or lines of fitted models (potentially different lines for different time intervals). Therefore, we suggest to refrain from adding this line and to concentrate on showing the actual data.

Line 304: when mentioning the 1.65-sigma uncertainty, I would also add that it is equivalent to the 90% confidence margin. It might not be obvious to everybody.

Good point. We added an according phrase

Line 365: "for there to be …" could be changed into "for … to be present".

We made the sentence simpler. It now reads: "It is relatively common to have layers …"

Line 482: when I first read about the use of a scaling factor, I was worried it would introduce unknown biases. Only later on (lines 555 and 579), I was convinced that it is indeed appropriate to make use of such a *strategy. I suggest adding a comment, possibly with a caveat and reference to the appropriate section, that the effect of using of a scaling factor has been explicitly analysed.*

We made this point clear by referring to the underlying assumption right at the place which used to be line 482. It now reads: "This scaling is based on the assumption that the mean EWH change in the buffer equals the mean EWH change in the buffered ocean area. Effects of violations to this assumption are included in the uncertainty assessment (see further below)."

Line 495: it is somehow surprising that the authors did not make use of the most recent degree-one timeseries provided by NASA-JPL, as they did for C20. It requires some motivation for this choice. I would also recommend to perform a comparison about the timeseries used in this study and those available through podaac-tools.jpl.nasa.gov.

We agree that the standards on degree-one time-series and some other aspects of GRACE analysis have changed since the time when our GRACE-based OMC products were generated.

Given the pertinent comments by two reviewers (Reviewer 2 comments #9 and #10; Reviewer 3 comment on line 495) we decided to perform a major update of the GRACE-based OMC analysis according to more recent standards. The update concerns GIA corrections, Degree-one solutions, and C20 series. The update includes updates of the related uncertainty assessment. The update has resulted in an increase of the linear trends for GRACE Ocean Mass Change in the order of several tenths of millimetres per year. The updated standard uncertainties are also increased slightly.

This update entailed a complete re-work of the budget analysis and a change to many numbers. We believe that it was worth the effort and that the dataset and assessment thus provided may serve the community as a longer-lasting reference.

Some more details on the update are appended at the end of our reply to Reviewer 1. They include comparisons among many variants of handling GIA, degree-one and C20.

We have updated the pertinent text in Sect. 3.3 as well as all numbers and figures in Sections 4, 5, 7, and 8 that depend on the OMC products.

Line 577: it is unclear how degree-one and C20 uncertainties have been determined, considering that only a single product was used. "The same approach" seems to refer to the GIA uncertainty, which is based on three different models.

C20 uncertainties and degree-one uncertainties were assessed based on ensembles, too. We added about 10 lines of text to explain this assessment in more detail.

Even more background information is appended at the end of our response to Reviewer 2.

Line 664: "Figure 5c" should be "Figure 5b".

Thanks for spotting this typo. We corrected it.

Line 732: the reference to Simonsen et al (2021) could be removed, since it is already mentioned two lines later (or the fact that the approach follows Simonsen could be mentioned earlier).

We shifted the sentence "This approach follows that of Simonsen et al., 2021." To the place where the description of the approach starts. We deleted the second, redundant reference to Simonsen et al., 2021

Line 736: what approach was used to exclude the peripheral glaciers from the grid?

We added more detail and a reference. It now reads: "The peripheral glaciers (connectivity level 0 and 1 according to Rastner et al. 2012) were excluded from the grid.

"Line 751: probably "reduced" (or something similar) is more appropriate than "circumvented".

Yes. We replaced "circumvented" by "reduced"

Line 801: I find it a bit of a pity that Ivins et al. (2013) has been used here instead of Caron et al. (2018) as elsewhere in the manuscript. Admittedly, in the discussion section the inconsistent treatment of GIA is explicitly mentioned as a limitation of this study. I can imagine that there were practical reasons for this choice, but it does require a short motivation.

The practical reason is that the study used CCI products where possible. Therefore, we used the AIS CCI Gravimetric Mass Balance products which use the Ivins et al. (2016) GIA model. We made this clearer by changing the wording. The question which GIA model is closest to the truth for Antarctica is open, of course. We now explicitly refer to the pertinent discussion in Section 7.2. So this part in Section 3.6.1 now reads: "The GIA correction adopted by these products was based on the regional model by Ivins et al. (2013). In Sect. 7.2 we address the trade-off between using global or regional GIA models for Antarctica."

Line 850: the whole paragraph about Figure 7a would better fit after line 824, before the uncertainty assessment.

We followed the suggestion and shifted the discussion of results to the place before the uncertainty assessment. We reworded some sentences to make them better flow in their new context.

Line 939: it is nice that the other contributions are explicitly discussed, but it is unclear why they have not been added to the final budget.

We agree that the study is not conclusive about the contributions and issues discussed in Section 3.8. Previous assessments of these contributions and issues (shortly reviewed in Sect. 3.8) are partly in conflict, refer to different time intervals, and do not always comprise time series.

Our principle has been to exercise our analysis based on datasets to which we have thorough insights, as outlined in Section 1. (Admittedly, we were not puristic about this principle when adopting a deep ocean steric estimate.) Therefore, we suggest to leave a more comprehensive inclusion of the mentioned contributions to future work.

Back-of-the-envelope calculations indicate that accounting for the missing contributions would not change the conclusions of the study on budget closure. We added this as a comment in the discussion section.

Table 3: please add a label, and a short explanation in the caption, about the difference between the two rightmost columns. Since some components are used in both columns, their meaning is not evident.
Table 4: same comment as for Table 3, this time about the three rightmost columns.

Thanks for pointing to this potential source of confusion. We largely extended the Table captions for Table 3 and 4 and added footnotes to explain the meaning of the entries in the different columns.

Line 1040: the proof of the Gaussian error distribution assumption is very nice, maybe a comment about it could be anticipated in the Methods section.

Thank you, this is a good hint. We added a pertinent explanation after Eq. (7) in Section 2.1

Line 1121: the fact that percentage of misclosures does not follow a Gaussian distribution is worth a comment.

We added a comment that the distribution is again (as in the previous paragraph) narrower than allowed for by the assessed uncertainties. We do no elaborate on whether the distribution is non-Gaussian or just Gaussian with a smaller width.

Line 1141: same as previous comment.

Again, we added a respective comment.

Line 1198: again, the same numbers for the LWS contribution also found by Frederikse et al. (2020).

Yes. We now added reference to, and comparison with, Frederikse et al. (2000) in Section 7.1

Line 1228: same comment as about line 939.

See our response to the comment about line 939. As a compromise, we added a sentence here saying: "Coarse estimates based on the literature review of Sect. 3.8 indicate that considering the discussed effects does not change the overall conclusions of our study."

Line 1237: the fact that the budget excludes polar areas is explained in the beginning of the manuscript, but it could also be repeated in the Table captions. This because some people will pull and use numbers from the tables without actually reading the full manuscript.

The original manuscript already stated in the Caption of Table 4: "The estimates of total sea level, the steric contribution, and the GRACE-based OMC refer to the ocean between 65°N and 65°S". We now added: "thereby excluding polar and subpolar oceans in the Arctic and the Southern Ocean."

Line 1239: the section about data availability would make more sense after the conclusions.

The manuscript composition suggested by the ESSD journal (https://www.earth-system-science-data.net/submission.html) has the "Data availability" section prior to the "Conclusions" section, thus qualifying the "Data availability" as being a generic part of the manuscript rather than a kind of addendum. We suggest to keep this order.