



Comment on **essd-2021-137**

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Referee comment on "Global sea-level budget and ocean-mass budget, with focus on advanced data products and uncertainty characterisation" by Martin Horwath et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-137-RC3>, 2021

This work presents the results of an extended study about global mean sea level and ocean mass change, carried on in the framework of the ESA's SLBC_cci initiative. The authors analyse both sea level observations (sea surface height, volume and mass changes) as well as observations and models of all the main contributors to ocean mass changes (landwater, Antarctica, Greenland, glaciers). The analysis is carried out for two time periods: the altimeter era and the GRACE era. Since budget closure is particularly sensitive to the size of the uncertainties, particular attention is devoted to propagating them through the whole chain in a consistent manner. I have particularly appreciated the thorough discussion of the different error sources.

This is an excellent piece of work, well structured, very well written and presenting results highly relevant to the ongoing discussion about sea level change and the accuracy of estimates of the various freshwater sources. I recommend its publication, without major changes.

Below, a list of somehow minor comments, in the order as they appear in the manuscript, mostly meant to clarify a few details.

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.

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.

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

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.

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.

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

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.

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.

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.

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

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).

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

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

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.

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

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.

Table 3: please add a label, and a short explanation in the caption, about the difference between the two leftmost columns. Since some components are used in both columns, their meaning is not evident.

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.

Table 4: same comment as for Table 3, this time about the three leftmost columns.

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

Line 1141: same as previous comment.

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

Line 1228: same comment as about line 939.

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.

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