EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-716-RC2, 2022

Comment on egusphere-2022-716

Anonymous Referee #2

Referee comment on "Revisiting the global mean ocean mass budget over 2005–2020" by Anne Barnoud et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-716-RC2, 2022

I have read the paper by Barmoud dealing with closure of the global ocean mass budget for the period 2005-2020. I agree with the other author that such study is crucial to our understanding of observed climate change and identifying potential problems and limittions in observing capability and/or data processing. In general, the manuscript is well-written, but there are a number of issues and clarifications that need to be dealt with. I also agree with the other reviewer to enc encourage more discussion of the non-budget-closure with respect to the various hypotheses presented in the paper.

One initial concern deals with the datasets. They have various coverage, and as such this needs to be accounted for in the comparison. One example is the altimetric ocean dataset. Apparently, this dataset is limited to 60N where as previous investigations have been limited to 66N. What is the reason for this limitation? It can not be the 200 km distance to shoreline but some other argument?

If the investigation is limited to 66S-6ON then all major contributors to mass changes are outside the ocean mask. Hence the authors NEED to revise the manuscript and compute the sea level fingerprints as all of the contributing datasets (GIS and AIS in particular) is completely outside this limitation. In my view, this is a requirement to perform this before the manuscript is published, as it might have a significant impact on the results.

Figure 6. I agree with the other reviewer that there is something wrong with the residuals. I also noticed that the computations/comparisons in this figure, unfortunately, ends

sometimes in 2018 which really calls for an update to the time series before publication.

Equation (3): I agree with the other reviewer that there is something wrong with this equation.

Section 3.3. The paper claims that Glaciers in Greenland is left out because they are already a part of the Greenlandic estimates. This is, to my knowledge, incorrect. At least they are not a part of the estimates in Simonsen 2021 and Mankoff, 2021). This needs further clarification.

The authors devote large parts to the discussion on the contribution of a possible trend in the Jason-3 MWR/WTC being responsible of up to 40% of the differences. This is a critical point in the paper as it is referred to unpublished material by Bernaud, 2022 as a lot of the following discussion is related to Jason-3 issue.

A closer look at their own figure 6 brings me seriously doubt about this explanation that Jason is really the problem. Particularly the lower part of Figure 7 indicates that the difference between altimetry and other mass-contribution clearly diverged from late 2014/early 2015. This is more than a year before the launch of Jason-3 in 2016 delivering reliable data from March/April 2016 Wouldn't this means that a more intuitive explanation would be that the older Jason-2 started drifting during its old age and the problem being that the tandem mission correction of the MWR between Jason-2->3 was in error?.

In my view the authors explanation of a drift in the Jason-3 radiometer is very vague. Particularly as the authros discuss the significant trend in the 2015-2018 period. During this period Jason-3 was only present 65% of the time (2016-04-2018.12) . If Jason-3 is responsible for 40% of the trend in this period the apparent trend in Jason-3 during its presence (in 65% of the time series) much consequently have been much larger. This should also be addressed in more detail.

Similarly to reviewer 1 I have an issue with the physical plausibility of the very sharp drops in the datasets seen in 2017? Please explain this. Could this be related to the missing GRACE-GRACE_FO during this period?.

When it comes to the discussion points in line 238-242 that potential evolution below 2000 m depth, permafrost thawing, and atmospheric water vapor, but In line 190 the authors already investigated and corrected for the deep ocean contribution which ranges up of 0.1 mm/year. Again this magnitude is very small compared to the difference seen, so I do not follow this argumentation.

All in all I find the issue on revising the global ocean mass budget extremely important but the paper and findings are presently not adequately convincing for publication.

Without computing the full fingerprints of the contribution to deal with the limited ocean mask I do not think that the paper presents substantial clear and new information. Particularly as many results are only presented up to 2018.

I suggest the authors to revisit the data perform the correct computation and extend the timeseries as much as possible so the paper and the conclusions could really represent the 2005-2020 period.