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Interactive comment on "Assessment of 21 years of Arctic Ocean Absolute Sea Level Trends (1995–2015)" *by* Carsten Ankjær Ludwigsen et al.

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This study uses tide gauges from 12 Arctic coastal locations along with an altimetry-based absolute sea level product to infer the contributions to absolute sea level trends over 1995-2015 at those sites. The novelty of the study comes from using an approach to determine the mass component of the sea level without using GRACE, meaning that the timeseries can be extended and is not reliant on selecting one of the GRACE products, which can vary greatly. After introducing the various datasets and initial breakdown into steric and mass components, the authors further compute the steric (halo- and thermo-), and mass (sources from different deglaciation sites, atmospheric loading, and dynamic mass contribution) components at each location, and discuss possible reasons for the trends there.

This paper presents an interesting approach to investigate the varying causes of sea level trends at each of the Arctic tide gauge sites in the study. However, I feel that the manuscript is unclear in quite a number of places, and the main results section (section 6) reads more as a list of what happens at each location, with little effort to point to the main notable findings and what we should infer from them. Therefore, by the end of the conclusion I was not completely sure what the main new results were, other than that uncertainty is large. I do think there are some interesting results in this manuscript but, as it stands, they are somewhat buried in the text. Below I have listed some key things which I feel need to be addressed to make the paper more accessible to the reader. Note that there are also some grammar issues which should be fixed during the review.

The authors thanks reviewer #1 for the thorough and constructive comment on the manuscript. It is clear that the manuscript needed substantial edits and that in particular the structure of the paper was unclear. We hope the reviewer will find that the edits has improved the paper significantly and made it more accessible and interesting. A pdf with all changes highlighted is attached as supplement and the specific comments are addressed below:

Main comments 1) The structure of the paper needs to be clearer - it is hard to know where the sections are divided, and whether we are reading about background, methods or results. In particular, the introduction seems to be made up of two introduction-style paragraphs with the rest as methods. I would suggest re-ordering so that you introduce the ASL formula before you discuss the mass component, and adding some more motivation about why it is important to study this before having a sentence or two to explain the layout of the rest of the paper. This would help to clarify what the reader can expect, and may result in less confusion. Introducing each subsequent section/subsection by saying what will be discussed will also help.

The introduction is completely altered and the method and data section is separated in a more coherent way.

2a) As part of my issue with clarity, it seems that section 6 is a large amount of information with no real synthesis apart from what appears in the conclusion. At the very least, a summary such as that in the conclusion should appear before the end of section 6, as otherwise the manuscript jumps from a pure description of the figures to the conclusion without all of the individual results being brought together in a coherent way. What actually are the main results of the analysis? How do they fit into the current literature? I also feel the last two paragraphs of section 6 - a very welcome discussion on uncertainties - should be given their own subsection and figure 8 referred to before the conclusion (which should not be introducing new results!).

The results section (now section 4) is divided into a subsection comparing the reconstructed sea level trend with altimetry and that compares ASL-trends at TG-locations. Section 5 contains an assessment of the observed and reconstructed ASL-trends with respect to the observed uncertainties (figure 7 and 8).

References to existing literature has been added when relevant – in particular where the observations disagree with the reconstructed sea level.

2b) In this part, it is important to discuss Table 1 and the large error bars at some of the sites - how much of this error is due to uncertainties in the methods compared to drawing a linear trend over strong inter annual variability (which is evident in some timeseries in Figure 2)? If they cannot easily be separated quantitatively, this should at least be stated.

We have added a paragraph on this (line 280-282). Furthermore a computational error was found when calculating the errors of TG, which made them too low.

3a) I feel as though the reader is left to infer for themselves what some of the figures show. In particular, Figure 4 is clearly important, as it shows the contribution from each of the mass components (the notable difference between this study and those that use GRACE-based products), but it is never given any real explanation or interpretation in the text. Despite this, results from two of the sub-panels are referred to in the abstract

(if my understanding is correct), which is bad practice at the very least.

In the results section several paragraphs describing figure 4 has now been added.

3b) Each figure should have at least a few lines dedicated to what it is showing scientifically, and should be referred to adequately when it is being described. For example, lines 152-153 discuss a positive halosteric trend along Siberia – I would have assumed that this is referring to Figure 3, but in that figure much of the Siberian coast has a negative trend. Unless it is referring to a particular station in Figure 6? Being specific about which part of each figure is being referred to would greatly help the reader in section 6 where multiple figures from multiple datasets seem to be being discussed.

In this particular case, we discuss a a positive halosteric trend in western Russian Arctic between Kara and Barents Sea (Amderma TG). This has now been specified (line 231-235). Generally it is specified when describing changes at specific locations.

4) The naming of each component/combination of components needs to be more consistent. For example, line 146 mentions 'derived product' but this seems to be used interchangeably between the one that uses mass from ECCO and mass from the REAR model. In the conclusion it is then described as 'derived steric estimate and in the mass product'. Similarly, for figure 6, the caption describes steric+geoid+dyn, but on the figure it's steric+geoid+dym [sic] + IB. As there are various different acronyms and sums of components throughout the study, explicitly naming these and then referring to them by one term only from then on would aid keeping track of what is being described.

Beginning with the method section and throughout the manuscript, the naming has been completely changed is hopefully now more intuitive to follow. The term 'manometric sea level' is introduced and we use 'reconstructed sea level' instead of 'derived'. ECCO OBP has been removed from the results and most of the analysis since it looked like ECCO was the 'true' sea level. We define the manometric (incl. IB) +steric as the reconstructed ASL (ASLr) which is compared to ASL from altimetry (ASL_A) and tide-gauges (ASL_TG).

5) With regards to Figure 5, I think more effort should be made to demonstrate that the cause of the discrepancy is the hypothesised overestimation of ASL in the altimetry data pre-2011. Can you somehow compare the final years where Cryosat-2 is available to see if the ASL (if not the trend itself) is comparable? What are the implications of the discrepancy on the later analysis? This should be stated in the text.

We have added a timeseries of the reconstructed ASL and altimetric ASL in figure 5 for two regions which identifies the drop in altimetry over the Beaufort Gyre in 2010.

6) Figure 8 is not well explained, either in what the subplots show or what they mean for the comparison of the different products. It should appear before the conclusion, and the spatial patterns described explicitly, at the very least where there are notable differences. Lines 200-203 state "Without the use of ECCO, the derived product agrees with altimetry at 98% of the area, while only 5 out of 12 of the TG-data agree with derived product. For the steric+mass(ECCO) product, the products agree at 99% of the area and at 11 out of 12 TG's." Why do we see no visible difference in the steric+mass versus steric+mass(ECCO) in Figure 5, but a big difference in agreement with tide gauges by this calculation? The interpretation of this figure and its consequence should be made more explicit.

The difference between ECCO/non-ECCO was small on +/- 10 mm/y scale shown in figure 8. As said, is the ECCO estimate removed from most of the analysis and also from figure 5 and 8.

Line by line comments Abstract: lines 6-7: This was never explicitly stated in the paper Lines 8-9: Again, this is never stated in the paper, and not discussed. I believe it is being taken from the bottom right panel of figure 4? This should at least be stated somewhere in the main text. Last line: What is the conclusion/importance of this work?

The abstract has been completely changed and the conclusions are now specifically stated in the last paragraph.

Line 45: IB has not yet been introduced.

Ok.

Lines 46-47: Referencing of figure 5 is out of order, which may be against journal rules.

This is referenced to avoid double-showing the altimetric trend. The text has been altered and a parenthesis is added around the reference. Hope this is ok.

Line 50: GNSS has not been explicitly introduced as an acronym

Ok.

Line 60: How many months are missing? Could you provide some idea of this and how it may affect the results? Is the missing data predominantly in one season?

Missing months are now indicated in figure 2 and mentioned in text (line 113-114).

Lines 61-64: Be clear that you are talking about relative sea level here and not absolute. Is there a reason you choose to discuss this rather than the absolute sea level?

Text has been improved, so that it is more clear that RSL is discussed and how it changes when VLM is added.

Lines 121-125: This description of the middle panel of figure 5 and its caption are not clear; which terms are used? Is it just the ECCO Ocean Bottom Pressure (bottom left of figure 4) that is added to the steric data, as may be implied by line 125 and the caption? Or is the dynamic mass contribution from ECCO included in the figure, as line 122 might suggest? Referencing the relevant panel on figure 4 would help to clarify this.

This has been changed according to the other associated changes made.

Line 129: Dooming -> doming Corrected

Lines 195-197: These figures show that the steric uncertainty can reach 10 mm/yr, which is the maximum halosteric contribution to the trend in Figure 3 (and will exceed the sum of the two steric components in places). How much impact does this have on the interpretation of the results?

More discussion on uncertainty has been added.

Figure 1b: I would suggest displaying the same latitude limit on this figure as the other maps in the paper. Having the more southerly latitudes does not add anything to the story as far as I can tell, and zooming in on the northerly latitudes would make the squared markers easier to see

You are right. We have changed it to display the same area as the other maps.

Figure 2: It would be easier to read this figure if the y axes were all the same. Why are the trend lines centred on 0? It was initially confusing to see some trends (for example, at Ny-Alesund) being offset from the data. Some of the trends are very small and having the offset makes it hard to see. I see the error in each is noted in Table 1, but why do the yellow lines on figure 2 have a different trend to the corresponding column in table 6?

Figure 2 had several computational issues in the first place. These have alle been fixed, and figure 2, figure 6 and table 1 should all agree now.

Figure 8: It is hard to distinguish between dark blue and black on lower panels - it may be clearer to change them to a different colour

Different colorscale is selected for figure 8.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-87/os-2020-87-AC1-supplement.pdf

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