All major edits are listed here:

Abstract: The conclusions of this study should now be clearer highlighted in the abstract.

Introduction has been changed to better reflect what this study aims to do and why it is a novel approach compared to other similar studies.

Steric: A figure has been added that shows the data density of T/S in the Arctic used for computing the steric estimates. Furthermore, a computational error lead to a significant overestimate of the steric errors and are generally reduced by a factor 3.

The manometric section (3.4) has been rewritten and is hopefully now more coherent to read.

Results section: Throughout this section, results are compared to other studies when relevant. In particular to the recent studies by Carret, 2017 and Raj, 2020.

Residual figure has been changed to show the areas where ASL_A and ASL_r agree within the 68% and 95% confidence interval.

Conclusion has been changed with respect to the updated results.

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

Anonymous Referee #1
Author response in red

The authors thanks the referee for the thorough review and positive assessment. Parts of the manuscript has been altered completely, which makes some of the comments below irrelevant for the updated manuscript. All comments below have either been addressed and the manuscript subsequently changed, or the comments are no longer relevant due to substantial manuscript changes.

The authors have made substantial changes to the structure of the manuscript and some figures and it is now significantly clearer to read and understand. In particular, the motivation in the introduction is more coherent and I appreciate the care taken to clarify the naming of different terms. In general, my previous comments have been addressed and I think this is an interesting study worthy of publication. I have a few remaining questions/comments but these are generally very minor and I have outlined them by line number below.

Note that there are very minor typos in the manuscript (generally related to plurals) that I have not listed but should be picked up during the final proofing.

Line-by-line comments:
Line 5: “that is independent from any observed sea level change”. This is a little confusing, since the reconstruction is based on observations. What is independent from what? By the observed sea level change here, do you mean tide gauges? Some clarification would be welcome

Line 73: Why is GRACE listed as a satellite product that is used in the study?

Line 78: should there be a dot above ASL_A? In general, I recommend checking the variables from equations that appear in the main text for this. Also check equations 8 and 9

Line 130: I may be wrong, but I think the Armitage and Davidson (2014) reference here, and all subsequent references to that paper, should actually be referencing the Armitage et al (2016) paper in the reference list

Line 163-165: these sentences are confusing. Please reword, and refer to the subpanel of figure 4 that the terms discussed are related to

Line 173: this is the first time ECCO has been introduced. A bit more information about this product would be welcome, especially as further on the discrepancy between ECCO and the manually-calculated Mdot are discussed

Line 177: Figure 5 should be referred to here in relation to ASL_r

Line 179: Refer to figure 4f here

Line 192: has anyone else found such a difference before? Are there any references for the sort of magnitude difference you could expect from comparing two such datasets?

Line 207: this dataset is from 2003-2014. Here it should be explained to the reader why the altimetry-based dataset of Armitage et al (2016) does not suffer from the seasonal bias and ‘flattening’ you describe.

Lines 199-207: This paragraph focuses on key differences in the Beaufort Gyre region, but there is no mention of the paper by Giles et al (2012) who specifically focus on trends from satellites in the region. This paper should be discussed here.

Line 217: specify the interior Arctic Ocean, or it suggests TGs have no use in the study

Lines 225-226: which error estimate are you talking about here? You state the tide gauges, but it is ASL_A that has the lowest error estimates for the specified locations, not ASL_TG…

Line 233: please reference figure 3 here

Line 242: I am surprised that this section does not refer to Figure 2. For these four stations in particular, the timeseries in Figure 2 is variable and one could argue that at Izvestia Tsik leaving out the last year or two of the timeseries would significantly alter the linear trend, while Golomianyi has a lot of missing data towards the end which may skew it upwards. Some acknowledgement of this figure would be welcome here

Line 242-244: it may be worth reminding the reader why the manometric estimate from ECCO and the summed estimate could differ here

Line 294: there have been studies of

Line 297: what was the time period used for the previous correlations? Could that have affected the results? This should be stated here as it is important for the conclusion

Conclusion: Given it is the main motivation for the study, I feel that a sentence or two in the conclusion could be dedicated to emphasising the novelty of this study and how it does/does not improve on the GRACE-based results in the literature. At the moment, a sentence on an improved correlation is given but is then followed by a discussion of uncertainties, which dampens the message. Perhaps moving the first two sentences of the second paragraph of the conclusion to the end of the first would help

Figure 4: the caption says “pherical glaciers” which I think should be “peripheral glaciers”? Also, I am a bit confused by how c) and d) look so different but have a very similar yearly contribution

Figure 5: is there a reason that the colormap has a different scale to figure 3? Providing both figures on the same scale would make the relative contribution of halosteric sea level change easier to see

Technical corrections
Line 20: “sea ice floats” -> “sea ice floes”?

Line 112-113: “and therefore does not significantly affect the trend estimates with a seasonal bias” -> “and therefore the trend estimates are not significantly affected by a seasonal bias”

Line 125: “does all TGs show” -> “all TGs show”

Line 129: “300.000” -> “300,000”

Line 130: “spatial and temporal” -> “spatially and temporally”

Line 134: “which is cause to large uncertainties” -> “which results in large uncertainties”

Line 150: brackets are unclear. Do you mean “…changes in ice loading (equation 4, similar to the elastic VLM-component) is computed…”?

Line 181: “way smaller” -> “much smaller”

Line 184: “til” -> “still”; line 185: “a absolute” -> “an absolute”

Line 202: “i.e.” -> “e.g.”

Line 203: erroneous bracket to be removed

Line 239: “Left map” -> “The left map”

Line 319: “thus can the individual contributions” -> “thus the individual contributions can”

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

Anonymous Referee #3
Author response in red

The authors thanks reviewer #3 for the comments on the manuscript, which lead to substantial edits of the manuscript.

This revised version has neen significantly improved compared to the original manuscript. However it still needs substantial improvement before being considered as publishable. My main comments concern : (1) the writing, sometimes poor and unclear, with several vague or not argued statements (see below), (2) the lack of discussion on the data uncertainties, in particular on the steric component, and (3) the lack of acknowledgement to similar previously published studies on arctic sea level (just quoted in the reference list but without discussion/comparison) (see below).

- The abstract is vague and non informative. Nowhere in the abstract is explained what the computed reconstructed sea level consists of. The abstract says : ’as the first study…’, a wrong statement in view of the many previously published studies. In addition, it claims that NOT using GRACE is a progress, but why ? GRACE has a nearly global coverage over the Arctic and provides unique information about tha mass component of the sea level budget in the Actic. Why oppose GRACE to the approach considred here ? The only acceptable argument is the shorter time series of GRACE data. I recommend to rewrite the abstract and clearly explain the approach considered in this study and presensing the main outcomes (and eventually the novel results compared to previous studies). More detailed comments below :

The abstract has been altered. The main reason for not using GRACE is the disagreement between available GRACE-products. This has been highlighted throughout the manuscript.

- Line 31 : what is the meaning of ‘difficult’ in the sentence ‘Previous attemps in reconstructing sea level in the Arctic have show to be difficult’ ? Explain. In the same sentence, the author write : … because satellite and in situ observations are less consistent'. Less consisent than what ? Curious claim considering that the authors ALSO use satellite and in situ observations…

- Line 35 : Clarify what you want to say by : 'the 10 mm/yr discrepancies between GRACE solutions'. In absolute value, this has no meaning. It should be compared to the amplitude of the signal. It also depends where.
- Line 37 : I disagree with the sentence ' some authors tend to choose the GRACE solution that closes the sea level budget. This not true. Please remove this sentence.

Introduction has been rewritten which should solve the issues raised in the three comments above.

- -Method section, lines 63-64 and Equation 7 : What about local VLMs , unrelated to GIA and ongoing mass redistribution ? These could be important at some TGs
A paragraph on this has been added, despite little information on VLMs from non-glacial change.

- Eqution 6 : What about the GIA component for altimetry (the component of -0.3 mm/yr removed to altimetry data in terms of global mean)? It seems to be ignored here.
Not entirely sure what is meant here, but the altimetric product is without any GIA-correction.

- Line 90. Fig. 1 should be quoted first.
OK

- Section 3.1, Altimetry. What is the accuracy of the altimetry data set in this region partly covered by ice. The authors disgard GRACE because of its 'poor' accuracy, but what about altimetry ?
It has been highlighted that altimetry is challenged over sea ice and that the associated uncertainties are larger. Also disagreements with Armitage et al 2016 has been described in the results section.

- Section 3.1, VLMs. The authors choose the option of using theroretical VLMs due to GIA and ongoing mass redistributions rather than GNSS data. Ok but explain why using the model is best and at least compare where possible the model VLMs with GNSS.
For detailed VLM/GNSS comparisons in the Arctic from 2003-2015 the reader is referred to Ludwigsen et al, 2020 (GRL). GNSS-data does often not extend to prior 2002/3, which is why the VLM-model is used, even in regions like the Norwegian Coast where GNSS is located close to the TG. Ny-Ålesund and Reykjavik are exceptions.

- 3.3, steric sea level. The interesting part of this study is the use of situ T/S profiles over the Arctic. However, we have no idea of the data coverage nor on the accuracy of this data set. What is the integration depth H ? I strongly recommend the authors provide information on the T/S data set, a crucial aspect of the study.
The description of the steric estimate and the used T/S-data has been extended.

- Section 3.4, ocean mass component. This section is hard to follow, with several unclear sentences, e.g., lines 163-165.
Section has been rewritten is hopefully now more coherent to follow.

- Results section. I strongly recommend to put into perspective the results of this study with the published literature (e.g., the works by Henry et al, Armitage et al., Carret et al., Raj et al). What is novel ? Wht is different ? etc.
Results from other studies have been added where relevant throughout the results-section.

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

Anonymous Referee #2
Author response in red

The points raised by the reviewer has been considered in the updated manuscript and the reviewer will hopefully find the manuscript improved.

When I reviewed the first submission of this manuscript, I recommended rejection. I cannot even remember if I have ever before made this recommendation and I have reviewed a great many manuscripts over the years. My assessment then was this: "this work reads like a technical report that has not properly matured into a scientific study, and I do not think that it merits publication". I now repeat this recommendation.

I started reading this second, revised, version, and I observe that the Introduction is very short (LL 16-43) and makes some (referenced) assertions about "uncertainties" (L 24) and "discrepancies" (LL 34-5), mentions that Arctic sea level reconstruction is "difficult" (L 31) and says that "satellite observations and in-situ observations are less consistent than in low and mid-latitudes"; but none of this provides a clear and explicit rationale and context for the work that is described in the rest of the manuscript.

The requirement on the author of any manuscript is to state in the Introduction (or Background, or Rationale) the following: (1) what is the "big issue", (2) what do we know, (3) what do we not know, (4) the reason for the importance of our ignorance, and (5) what we propose to do about it. If I remember right, the best-known (most accurately measured) quantity in physics is the electron magnetic moment, relative standard accuracy of 3x10^-10: everything is uncertain, and quantities in the environmental sciences may be very uncertain; but does that uncertainty matter? Is it important? Why? There is enough published in the Arctic context on altimetry, gravimetry, tide gauges, land movement (and so on) for the authors to fill in thoroughly on what text is needed to embrace all of these five points, and to make the case for why their work was worth doing. I strongly suspect that it was worth doing: but the authors still need to state their reasoning clearly, explicitly, and with evidence. A similar criticism applies to the Conclusions: what new have we learned, above and beyond what we already knew from previous studies? At present, the Conclusions are a summary but do not state what is new. This is why I stated in my first review that the manuscript resembled a technical report, and that assessment has not changed.

This manuscript is still not a mature document, and I consider it wrong first to send a document in such a condition to a journal, and then to expect the reviewers and the editor to repair its deficiencies. It is the authors' responsibility to think for themselves, to seek advice from colleagues, and to consult online resources, if that is what's required, in order to present a mature document for consideration for publication. Then the reviewers will engage willingly, so that it can be checked in the usual terms, and the process not used as a substitute for manuscript development.