Glacial meltwater in the Southeast Amundsen Sea: A timeseries from 1994-2020

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Response to Referee #2 Comments

Thank you very much for reviewing our submission with insightful comments. A revised discussion and text, with additions to the Appendices, has improved the manuscript in ways that we trust will satisfy your concerns.

General comments:

The pace of melting of Antarctic ice shelves due to warming along the coastal margin and the associated changes in the grounded ice sheet are a major concern in terms of future sea level rise. Models that are used to project future changes still entail large uncertainties and current estimates of changes largely stem from remote sensing data. Ocean tracer measurements that can be used to quantify the glacial meltwater content and its changes accumulated in the ocean provide an opportunity to better understand the melting of ice shelves and its temporal variability.

The study by Hennig et al. provides novel data collected over more than two decades from the Amundsen Sea sector, which is a region where a large increase in melt has been reported previously, mainly driven by warm water intrusion on the shelf. Using the isotopic composition, they find that the regional freshwater budget is dominated by glacial meltwater and that the meltwater inventory exhibits large decadal fluctuations superimposed on a comparatively small long-term trend. These results support other recent studies based on remote sensing data that have found substantial fluctuations of the ice shelf melt on decadal time scales.

This is a very timely and interesting study that is of importance to the wider Antarctic ice shelf and ice sheet community as well as the oceanographic community. It is overall well written and I think that the methods are mostly robust and support the results. Particularly the authors' approach to circumvent issues of laboratory offsets in the isotopic measurements, that have been a known issue for a while, is quite elegant and I think leads to meaningful results. However, I also think that the paper would benefit from a more in-depth comparison to previous work and from highlighting the novel aspects of this work more clearly. In addition, I have some concerns regarding the uncertainty discussion, in particular to biases induced by the spatial sampling and I think that caveats should be communicated more clearly. Overall, I think that the manuscript is suitable in principal for publication in The Cryosphere, after addressing some points.

> We have expanded the discussion, including a direct comparison to the results of the original study using the 2014 data (Biddle et al., 2019). We have revised our spatial sensitivity analysis along the lines of your suggestions, and moved much of that content into the main body of the paper. We have also conducted another independent spatial sensitivity analysis, which is also presented in the main manuscript body.

Specific comments:

1. I think that the motivation for this study and the importance of the results is not communicated sufficiently. Currently there is a strong focus and emphasis on the collection of a timeseries, but very little on why the timeseries is collected and what we can learn from such a timeseries. I think that discussing this in more detail, in particular in relation to the recent literature on the temporal evolution of melt in the Amundsen Sea, is critical to highlight the novelty of the results. A particular example is the following sentence in the introduction (P2L31-33): "[…] some studies have shown a greater interannual variability in the basal melt rates than increase […], and some have even suggested a slowing of basal melt rates […] and grounding line retreat […]." I think that this point has to be extended by rewriting the sentence, adding a time perspective (what happened when / what timescales are we talking about), has to put into perspective of natural climate variability versus anthropogenic forcing, and used as an explicit motivation for the study and how the seawater isotopic composition might help to contribute to this discussion.

The data used in this study was not collected with the explicit intent of producing a timeseries of meteoric water inventories (or even a timeseries of seawater $\delta^{18}O$ data) but was compiled from multiple datasets collected independently for different projects. This has now been clarified in the Introduction and Methods. Most data were obtained with the intention of enhancing understanding of ocean-ice shelf interactions and melting at the time of measurement. We have rewritten the discussion of interannual variability of basal melt in the introduction.

The motivation for this study was to aggregate as much $\delta^{18}O$ data as possible from a single region important to the WAIS, and use it to examine changes through time. This allows us to assess the viability of the technique for monitoring basal meltwater input from ice sheets.

2. Following the point above, I think that the paper would benefit from an extension of the discussion on the temporal variability shown in Figure 4. To me, this is the key result of the paper. However, the discussion on details in variability seen in this Figure and how they relate to other recent findings and what new aspects can be learned from this Figure is very limited. In fact, there is not even a reference to Figure 4 in the main text.

We have revised our results section and now Figure 4 more explicitly. The discussion section has been expanded, and now includes consideration of variability in meteoric water content through time. We focus on the utility of this technique for identifying changes in melt rates, and its potential utility in better constraining mass loss through basal melt.

3. P4-5L80-82: I think that the approach taken here indeed mitigates some of the known issues of salt effects between IRMS and CRDS. However, it is not very clear in these sentences here that the salt effect is indirectly removed by using different CDW reference values for each respective data set. I think that should be written more explicitly at this point. In addition it might be useful to actually point to the differences in CDW d18O in Table 2 where the CRDS measurements (2019/2020) yield a much lower CDW value than the IRMS measurements (2014). Is this difference in line with the values reported for the salt effect in literature?

We ran a subset of 100 samples from 2019 and 2020 on both IRMS and CRDS systems and observed no analytical offset between the two instruments. The literature on possible salt effects on seawater $\delta^{18}O$ measurements shows inconsistent offsets between instruments and labs, and is based on a very small number of samples. We are not convinced that there is a significant salt effect impacting our results as well as all other published paired isotopic datasets from CRDS and IRMS methods.

We now explicitly describe how the potential impact of interlaboratory offsets is indirectly removed by defining mCDW and meteoric water sources using data from each year separately. We have also added a section and pointed the reader to the appendix where we discuss the observed offset between the 2014 data and other years.

4. P7L132-135: I think it is important to discuss the difference in results associated with using a constant and varying mCDW and meteoric endmember at this point. A constant value would yield a GMW estimate that is spatially integrated and the varying endmember yields local fluxes. Likely, this choice will also affect the long-term trends in the GMW estimate (largely through changes in the meteoric endmember), which I think should be discussed as a possible caveat at this point.

   We have added an extended discussion about the utility of defining the meteoric water endmember using salinity-$\delta^{18}O$ intercepts. We include a comparison of the 2014 data using our methods, vs those used in Biddle et al. (2019), where the 2014 data were originally published.

5. I am still a bit concerned about potential artifacts from the changes in the spatial sampling from one year to the other. Fig. 1 and also Fig. A3 clearly show substantial spatial differences in GMW content in the region and I think that the paragraph on p. 9 Lines 184-187 is not sufficiently accounting for the issue. I appreciate that this issue is investigated in Section A4. However, I think that the manuscript would benefit in terms of the credibility of the results, if a more detailed spatial analysis was added to the main text. In the end, the main results in Figure 4 are interannual variations with a magnitude of about 1.5m, which seems to be within the range of spatial variations shown in Figs. 1 and A3.

- So, I am wondering if the reported uncertainties in Table A2, last column ("Average GMW inventory (m)), as well as the uncertainties shown in the main text also include the spatial standard deviation of the samples? Is this included in the "environmental" uncertainty within the Montecarlo simulation? I think that it would be transparent and beneficial to simply report the spatial standard deviation of GMW for each box also in Table A2, which would give a measure of the range of spatial variations.
- In addition, I have difficulties understanding how the boxes were chosen and why they seem to be not consistent between the years, i.e. sometimes a location falls in one box and sometimes in another. I think it would be helpful to have boxes that are rather fixed in time and represent certain regimes within the region. For example, I found the Boxes in Fig. A3 for 2014 quite logic, since there is an "offshore" box (c), a TGT box (d), a PIIS box (a) and a central box (b). Looking at these boxes over all years and samples would be, i.e. having a figure similar to Figure 4 for each of these regions would be very helpful to understand how the variability might differ spatially and if the variability is a signal that is consistent across the entire domain or just arises from local signals would be very helpful to have. I would suggest to actually have a figure like this with a brief discussion in the main text if possible.

The boxes used in the spatial sensitivity analysis were based on groupings of stations as sampled. The groupings were selected based broadly on the criteria described in your comment – a group as close as possible to the ice shelf, a second group more distant from the ice shelf front, a third further offshore – and a fourth group around Thwaites Glacier Tongue for 2014.

We have redone the spatial sensitivity analysis across all years using consistent geographic boundaries. We have tried (occasionally unsuccessfully) to draw boxes in a way to avoid any groupings with only 1 or 2 stations.

We have also added an additional spatial sensitivity analysis, wherein results were calculated using random selections of 3 stations; this process was performed 10,000 times, with the standard deviation measured as the standard deviation of meteoric water inventories produced using 10,000 random groups of 3**.**

The Monte Carlo simulations described in the original manuscript will incorporate some spatial variation of the endmembers – mCDW and meteoric water endmembers vary in each simulation based on the observations. All three endmembers (mCDW, meteoric water, sea ice melt) are subject to appropriate environmental uncertainty. For mCDW, that uncertainty is based on the variation in mCDW S and $\delta^{18}O$ signatures observed across the whole field site, and for meteoric water, the uncertainty is based on the standard deviation of $\delta^{18}O$ in the nearest ice core (ITASE01-2).

The spatial sensitivity analyses, with supplementary tables have been moved up into the main body of the manuscript, with more detailed results tabulated in the Appendix.

6. I am a bit concerned about the conclusion (P11 Line 243) that the long-term trend is insignificant without discussing the fact that this only reflects the data presented here but might not reflect the actual trend in the melting. It would be good to discuss some of the caveats of the use of the data set and its limitations. In particular, I think that the data set will not capture the entire amount of meltwater coming from the Amundsen Sea, as the authors' report that the residence time of the water in the region is only about 1 year. So, it may well be that there is a strong long-term trend in glacial melt in the region, but that the signal largely propagates out of the region and does not accumulate there. Also, the fact that the endmembers vary throughout the years, in particular the glacial melt endmember, could affect the long-term trend. So, I think it is important to discuss such potential limitations here.

While not all of the Amundsen Sea meltwater will accumulate in our study area, all of that from the Pine Island Ice Shelf, and much of that from the Thwaites Ice Shelf will necessarily pass through our study area, so the results we present are likely specific to those two ice shelves. While the glacial melt endmember can be expected to remain relatively stable, it will vary with depth (ITASE01-2 and Siple Dome ice cores have $\delta^{18}O$ standard deviations of 1.9‰ $\delta^{18}O$ – greater than the standard deviation of our yearly meteoric water endmembers) and would not be expected to be static through time. Given the extrapolation required to determine meteoric water endmemebers as we do in this paper, sampling and analytical uncertainty will also play a role.

We have expanded the discussion and conclusion to include the limitations of measuring meltwater content this way, including the influence of residence time, and the export of meltwater. Icebergs calving will be a significant component of glacier mass loss (and a contributor of glacial freshwater flux to the Southern Ocean), however if melting occurs outside of the study area, this component of mass loss will not be accounted for in the meteoric water inventories.

Technical corrections:

- P2L35: I don't think that "SE" has been defined yet.

  Corrected

- Figure 1: Please do not use "rainbow" colormaps that are not scientific colormaps. For detailed reasons and tools to generate an appropriate colorbar e.g. for Matlab, please see for example this paper by Stauffer et al. (2015; https://doi.org/10.1175/BAMS-D-13-00155.1)

  We have revised Figure 1 with a more appropriate colormap.

- Figure 2: I found it difficult to depict the difference in blue. Since only dark blue is used, it may be good to keep those dark blue sample and exchange the other blue(s) by gray.

  We have revised Figure 2 using a gray colormap showing all depths, which is more illustrative of the deep-shallow mixing line described elsewhere in the paper.

- P6L107: Probably important to add that also "sea ice formation and melt" will affect the signal at this point.

  Thank you – reference to sea ice melt and formation added here.

- Equations 1-3: the placement of these equations seems odd as there are somewhere in the text where they are not discussed. Please place them right below a description of and reference to these Equations.

  We have added a reference to the equations in the text and revised the text to more clearly reference them.

- P6L125: I guess it should be not just sea ice melt but also "formation"

  Added reference to sea ice formation.

- P7L149: I think that "extremely unlikely" is a stretch here. Please reformulate to "[…] mean, the potential impact of analytical calibration offsets between laboratories on the calculated GMW fractions are mitigated.

  Corrected

- P8L154: "mathematical artifacts" seems odd and would not understand what is meant, do you mean "sampling and analytical uncertainties"?

  Text amended as suggested.

- P8L166: It is unclear at this point where the uncertainty estimate is coming from. Could you please refer to the part of the manuscript where it is calculated and/or briefly mention it here.

  We have described the uncertainty estimate and directed the reader to the discussion for further depth.

- Figure 4: Is this trend statistically significant or not. Please report the statistical significance here.

  We have added additional measures of uncertainty (standard deviation, 95% confidence interval, p-value) here to expound upon the statistical significance of the trend.

- P10L194: I have difficulties understanding the meaning of "a range of the range" (e.g. +-1.5 – 1.7 g/kg) in uncertainty, if I understand this correctly. Please report a single range, e.g. +-1.7 g/kg.

  The reason for a range of values is because the uncertainty varies by year, largely dependent on the fit of the data in $\delta^{18}O$ -salinity space. In 1994, 2019, 2020, the uncertainty of the meteoric and mCDW endmembers is quite low, while in 2009 and 2014 it is higher, due to the spread of the data. We have adjusted the text to make this clearer.

- P10L201: Change to "This minimizes systematic isotopic offsets"

  Text amended.

- P11L249: I think what the authors are really trying to say here is that the decadal variability of the melt is actually substantially larger than the long term trend (1994 to 2020). The way that this is currently written it is difficult to understand what is actually meant. It seems not surprising that there is interannual variability in the first place, but what is in fact interesting is the magnitude of the variability compared to the trend and the time scale over which this variability occurs. I think that needs clarification.

  We have amended the text to make the meaning clearer.

- P11/12L252-253: It would be helpful to note at this point that the tracer approach has the advantage that the ocean integrates the temporal meltwater changes and thus a single measurement actually reflects a longer period of melting.

  Thank you – we have added a description of the period captured by these meltwater measurements.

- P13L260-264: Please correct typological and formatting errors.

  Errors corrected.

- P17L331: I have difficulties understanding the meaning of “a range of the range” (e.g. +-0.5 – 0.7 m) in uncertainty, if I understand this correctly. Please report a single range, e.g. +-0.7 m.

  The reason for a range of values is because the uncertainty varies by year. In 1994, 2019, 2020, the uncertainty of the meteoric and mCDW endmembers is quite low, while in 2009 and 2014 it is higher, due to the spread of the data. We have adjusted the text to make this clearer.

- P17L333: I think that this should read “95.1%”, right? Otherwise I would not understand this number.

  Typo corrected, and the description has been expanded upon for clarity.

- Generally, the numbering of subsections is wrong; always starts with “1.x”

  Thanks - Appendix subsection numbering has been amended.

References

Biddle, L. C., Loose, B., and Heywood, K. J.: Upper Ocean Distribution of Glacial Meltwater in the Amundsen Sea, Antarctica, J. Geophys. Res. Oceans, 124, https://doi.org/10.1029/2019JC015133, 2019.