Meteoric water and glacial melt in the Southeast Amundsen Sea: A timeseries from 1994-2020

Andrew N. Hennig, David A. Mucciarone, Stanley S. Jacobs, Richard A. Mortlock, Robert B. Dunbar

Response to Reviews

Editor comment

I am sorry for the long review process, but I thank the two referees for their comprehensive second examination of your work. You will see that in spite of several improvements, they still require major revisions. Please provide a point-by-point response to their comments and revise manuscript where necessary. I consider that the discussion on precipitation minus evaporation versus glacial meltwater has clearly improved the manuscript, although it could be clarified earlier. On this point, you may consider adding a reference to Bett et al. (JGR, 2020, their Figs. 5-7) to further support your claim. You will see that this reference also makes it clear that the Amundsen Sea continental shelf is a region of net sea ice production, and it therefore seems essential that you address the 2nd comment from Referee#2 on sea ice formation/melt.

Thank you for your comments. The revised manuscript brings our discussion of GMW vs precipitation fraction of meteoric water closer to the beginning of the manuscript. This conclusion is based on our results, so we have retained the extended discussion/explanation later in the manuscript. We have also re-analyzed our dataset and now include a fraction of precipitation in the upper water column, which we think reviewers will find agreeable. With respect to major revisions, we cannot respond further to reviewer #1's main issues without degrading the credibility of our paper. This is discussed below.

I also would like to add a personal comment: models show that the coastal circulation in the Amundsen Sea is intensified for higher melt rates (Jourdain et al., JGR 2017, Fig. 8). This makes the interpretation of meteoric water inventory in terms of glacial meltwater trends more complicated as more melt is associated with an intensified export of meltwater. This nonetheless only applies to the glacial meltwater coming from ice shelf melting (the iceberg part being non negligible, e.g., in Rignot et al., Science, 2013 or Greene et al., Nature, 2022).

Our revised manuscript includes discussion on this point. Any ice sheet mass loss via exported icebergs will, of course, not be included in our meteoric water inventories. We have added further discussion on the meltwater potential contained within icebergs, as well as the circulation caveats around using mean inventories to interpret melt trends.

Referee #1

I am pleased to see that the authors have addressed some of my detailed comments and suggestions. I still hope that this paper, with its valuable technical insights, will be published for the benefit of the community. However, it is somewhat frustrating to note that most of my suggestions, which would have required substantial revisions to the manuscript, were politely rejected by the authors and/or does not reflect the work needed in the manuscript. The paper may benefit from further work, additional analyses, or retaining the annexes to create a new comprehensive technical paper. In light of my significant disagreements with the authors, both previously in Nature Comm&Env and now in The Cryosphere, I have chosen to terminate my review at this juncture. This decision stems from the realization that I may no longer be the most qualified individual to provide a fair assessment. Additionally, I would like to reiterate my primary concern:

With respect to what seems to be their primary concern (that GMW and precipitation in this region cannot be distinguished based on δ^{18}O), as this is an undeniably incorrect view on their part. There are a few comments made by this reviewer that have allowed us to improve the manuscript.

- One particular concern is the authors' argument, unproven, that the use of salinity and δ18O in tandem allows for the estimation of glacial meltwater content, for instance: this statement is made in Line 49. Additionally, in the Methods section, they assert that meteoric water deeper than 200 meters is dominated by ice shelf basal melt, but they also mention surface waters (Lines 111 and 131). It isn't until Line 220 that the authors discuss that meteoric water inventories are likely composed of 90% glacial meltwater. It would be more logical to introduce this hypothesis, supported by literature and analyses, in the Methods section to establish that meteoric water in this study is synonymous with glacial meltwater. This consistency would eliminate the need to use terms like 'glacial' or 'GMW' in parentheses in the manuscript. I concur that deep freshwater primarily originates from glacial melt, which is not a novel observation. However, considering that δ18O cannot effectively distinguish meteoric sources in this region, it would have been beneficial for this hypothesis to have been proposed and substantiated earlier in the study.

Many other studies have distinguished between precipitation and glacial melt using δ^{18}O. (Jacobs et al., 1985; Meredith et al., 2008, 2010, 2013; Randall-Goodwin et al., 2015). Several studies have used mixing lines on δ^{18}O-salinity plots to distinguish these freshwater sources (Fairbanks, 1982; Jacobs et al., 1985; Meredith et al., 2008, 2010, 2013; Randall-Goodwin et al., 2015). A study by Potter and Paren (1985) observed that the ice flux into the ice shelf had a δ^{18}O value of around -20‰, whereas direct accumulation onto the northern part of the ice shelf in the form of precipitation had a much higher δ^{18}O value, around -13‰. This study and the ability to distinguish between precipitation and glacial melt is also called out explicitly in (Meredith et al., 2008).

Our greatly expanded discussion cites all available and accessible coastal precipitation data from the northern WAP to well west of our study area. These data show a clear latitude-correlated trend. Snow we collected and analyzed in the field during the 2019 cruise had a δ^{18}O of -15‰ (while the local glacial ice has an endmember -30‰), consistent with results from Masson-

Delmotte et al. (2008), and all of the available $\delta^{18}O$ precipitation data across West Antarctica. (IAEA/WMO, 2019)

In several prior papers using stable isotopes in a similar fashion, 150 m is indicated as the depth below which all, or nearly all meteoric water is presumed to be glacial melt (Randall-Goodwin et al., 2015; Biddle et al., 2019). With atmospheric influences minimized (or even eliminated), meteoric water below 150m consists almost entirely of glacial meltwater (Jenkins, 1999; Randall-Goodwin et al., 2015; Biddle et al., 2019).

In our 7 years of data, we see surface influence diverging from the mixing line as deep as 160m (2000). Including shallower depths, where data move off the mixing line produces a less negative intercept, as the mixture now incorporates sea ice melt and may also include less-depleted precipitation. We took a more conservative approach than previous studies, selecting 200m as the depth below which surface influences are minimized, to determine the mean meteoric water endmember from the seafloor up to the point where surface influences appear.

The relative enrichment of $\delta^{18}O$ in surface waters relative to salinity is due to the influence of sea ice melt (which has a slightly positive $\delta^{18}O$) and some amount of precipitation (which has a less negative $\delta^{18}O$ value than glacial meltwater). While surface waters will include precipitation, most of the surface meteoric water will still consist of glacial meltwater. (Meredith et al., 2008, 2010, 2013; Bett et al., 2020).

- Regarding the methodology, the authors suggest using a simple linear regression on all data below 200 meters depth to constrain the meteoric water endmember each sampling year. Upon initial examination of the δ18O /S plot, the data appear to form a single mixing line for salinities greater than 34. This is surprising considering the clear distinction in the T/S plot between waters influenced by melt and those composed of simple CDW/WW mixtures with WW characterized by salinity higher than 34 and temperatures near the surface freezing point.

$\delta^{18}O$ is a distinct tracer from temperature, and plots against salinity reveal different things. Winter water does not have as distinct a fingerprint in $\delta^{18}O$-S space as it does in T-S space in this region (see Figure A1 in manuscript, where there is no $\delta^{18}O$ distinction between points falling on the GMW mixing line vs the WW mixing line). Biddle et al. (2019), who discusses winter water in their noble gas and traditional hydrographic tracer study, do not attempt to determine a $\delta^{18}O$ signature for winter water. The production of winter water is indirectly accounted for in the 3-endmember mixing model by the negative sea ice melt fractions (indicating sea ice formation).

The implication is that runoff and precipitation are less negative in δ18O than ice shelf basal melt. However, when sea ice formation increases surface water salinity to around 34, the δ18O/S of the WW ends up to the right of the meltwater mixing line influencing the CDW/GMW line estimated by the authors. The extrapolation of the meltwater mixing line to zero salinity gives a more negative intercept due to the influence of deep WW. Furthermore, the authors must acknowledge that the estimated freshwater content reflects contributions from large spatial and temporal scales, including meltwater from ice shelves upstream, icebergs within the Bellingshausen Sea and WAP, and precipitation inputs carried into the region by surface circulation.

There is no distinct salinity- δ^{18}O fingerprint for winter water in this region (see Figure A1 in manuscript). Work by Jenkins (1999, p.199) showed that winter water has a less negative δ^{18}O than waters influenced by GMW, in temperature-δ^{18}O space. The intercept method has been used to determine a glacial freshwater endmember in numerous other studies (Fairbanks, 1982; Jacobs et al., 1985; Paren and Potter, 1984; Potter et al., 1984; Hellmer et al., 1998; Jenkins, 1999).

Further, the intercepts produced using our method are consistent with local ice cores and glacial melt. Studies from further northeast show less negative intercepts, consistent with ice cores near those study areas (Meredith et al., 2008; Thomas et al., 2009; Meredith et al., 2010, 2013; Goodwin et al., 2016).

We conducted 2 exercises to assess the influence of winter water on our intercepts:

1) We applied an aggressive "winter water filter" to our data (removing all points with S>34 and Θ <-1.5°C) and recalculated our intercepts. In 3 years, the intercept was unaffected (2007, 2014, 2019), in 2 years, removing the "winter water" resulted in a *more negative* intercept (1994, 2000, 2020). Only in 2009 did the intercept value increase (by 0.3‰) after removing the "winter water" data from the regression. The largest change in intercept value was a decrease of 0.5‰ in 1994.
2) We selected only those data falling on the mCDW-GMW mixing line in T-S space, and then ran the salinity- δ^{18}O regression through only those data. Once again, our intercepts were unaffected.

Our uncertainty analysis applies uncertainty with a SD of 1.9 ‰ to our meteoric water endmember, in addition to changes in the meteoric endmember resulting from geographic sampling selection and instrumental precision (also accounted for) so any confounding influence of winter water is accounted for within our error envelope as defined by the SD. We have added discussion of winter water to our discussion, results, and appendices.

We have elaborated on meltwater input from upstream, however we note that the freshwater endmember identified using our intercept approach is most consistent with local sources.

The use of data below 200 meters excludes a portion of meteoric water in the surface and subsurface layers. Therefore, it is essential to consider the broader context of freshwater sources and their uncertainties. Distinguishing subsurface glacial meltwater, a meteoric input, from surface- derived meteoric inputs using δ18O is challenging, as isotopic values in local precipitation and ice shelf melt are similar.

<u>Values in local precipitation and glacial ice melt are not similar</u>. Masson-Delmotte et al.'s (2008) study of Antarctic precipitation presents an Antarctic-wide study of precipitation δ^{18}O where they show that 88% of the variance in precipitation δ^{18}O can be explained by latitude, distance from the coast, and elevation – <u>with elevation being the most important factor</u>. The value of our collected snow samples (-15‰) is consistent with the model presented in this study. As mentioned previously, many other studies demonstrate the ability to distinguish between local precipitation and glacial melt on the basis of δ^{18}O (Jacobs et al., 1985; Potter and Paren, 1985;

Meredith et al., 2008, 2010, 2013). This seems to be a recurrent sticking point for this reviewer – but they are demonstrably wrong on this point. We ask the editor to check with some experts and/or consult the literature. They will find that this statement by reviewer 1 is wrong.

The age and source of ice melting at the base of the ice shelves remain uncertain, further complicating the δ18O analysis. Given these uncertainties, considering a larger range of plausible δ18O meteoric endmembers based on regional literature would provide more realistic estimates with appropriate uncertainties.

No other papers employing similar methodologies (Meredith et al., 2008, 2010, 2013; Randall-Goodwin et al., 2015; Silvano et al., 2018; Biddle et al., 2019) use a range of δ^{18}O endmembers (though Biddle et al. use a similar uncertainty analysis). We vary endmember δ^{18}O determined by the regression by a SD 1.9 ‰ (on top of changes to the endmember from geographic variability, and perturbations to account for instrumental precision) in our uncertainty analysis. The age of the glacial ice is not a great cause for concern in this case – especially since we can estimate the δ^{18}O of the glacial melt using our intercept approach. We have consulted with several glaciology colleagues. The Antarctic cores that we cite go to bedrock (i.e., Epstein et al., 1970) and show reasonably consistent mean δ^{18}O values to about 1100m, whereafter they have more negative δ^{18}O on average. As described previously, salinity- δ^{18}O intercepts have been used by numerous studies to identify mean glacial meltwater endmembers.

For our purposes, the age of the ice itself is immaterial. We estimate the mean glacial meteoric water endmember from observational data; this will estimate the δ^{18}O of the mean meteoric water endmember being introduced at depth – the age of the ice does not matter. The values derived from our intercept calculations are consistent with the values from local ice cores. If we were to use a "plausible meteoric endmember from regional literature", we would use the values from those ice cores.

However, to account for the presence of precipitation more explicitly, our updated analysis includes the use of a different mean meteoric water endmember in the upper 200m, determined by a mixture of the glacial endmember and precipitation. Both the relative proportions of the GMW-precipitation meteoric water, and the δ^{18}O of precipitation are varied in the uncertainty analysis – on top of the uncertainty already preformed.

Finally, I also disagree with the authors' claim that Antarctic precipitation at sea level substantially differs from its values in continental precipitation and glacial ice in the region of their study.

We have addressed this concern previously. The data is clear on this point. The reviewer can disagree all they want but they are demonstrably wrong and is not appropriate for us to degrade the credibility of our paper by trying to address their miscomprehension of facts.

- In their mass balance calculation only three endmembers are used, and even I am a passionate user of this approach, it does make numerous assumptions, necessitating the consideration and propagation of errors. Among other water masses in the region, the authors also fail to address the presence of WW in their analysis, which can extend deeper than 200m.

Winter water is not explicitly addressed in the model, as it does not have a distinct salinity- δ^{18}O fingerprint in this region. However, the negative sea ice melt fractions are implicitly indicative of winter water – indeed sea ice melt and winter water are two sides of the same coin. Our study is intended to focus on glacial meltwater – it is not a hydrography paper - however we have pulled further sea ice discussion forward into the main body of the manuscript.

We conducted extensive uncertainty analysis, including environmental variability, instrument precision, and endmember uncertainty – testing the influence and propagation of errors from the raw sample data through to the output of the mixing model at every step. This was included in the first version of this manuscript that was submitted, and further uncertainties were introduced and propagated in the last submission. Very detailed accounts of the specifics of the uncertainty analysis are found in the Appendix, and the high-level results are clearly and prominently displayed on the yearly mean meteoric water inventory figure.

Our updated revision includes *further* uncertainty analysis, with the inclusion of precipitation with a varying fraction and δ^{18}O in the upper water column.

The use of 200 meters as the limit between the surface layer and the mCDW layer is questionable and requires clarification. Again, the authors rely on a single value of approximately -30‰ to represent the δ18O of GMW, which is weak given the accumulation of meteoric water in the WW layers from various sources, both local and remote.

200 m is not used as a "limit" between the surface layer and mCDW and was never described as such.

It is the depth below which we consider that meteoric water inputs will be nearly entirely glacial. Other studies in this region use 100m-150m as the depth below which waters are free of atmospheric influence, and therefore consisting of nearly pure glacial meltwater (Jenkins, 1999; Randall-Goodwin et al., 2015; Biddle et al., 2019). We chose to use 200 m as a more conservative estimate to identify the glacial meteoric water endmember. The same approach has been used by numerous other studies. (Fairbanks, 1982; Paren and Potter, 1984; Potter et al., 1984; Hellmer et al., 1998). This is very clearly described in the text.

We do not "rely on a single value" for our meteoric water (or any other) endmember. In our uncertainty analysis, the mCDW and meteoric endmembers change with each iteration based on randomized spatial selection of data, introduced perturbations to account for instrumental precision. On top of these perturbations, we varied the calculated endmember in each simulation randomly by a SD of 1.9‰ – the standard deviation of local ice core δ^{18}O (which, as noted, have very similar values to our calculated intercepts). In the most extreme cases, this means shifting the meteoric water endmember produced by the intercept regression by up to 7‰ - and again, the endmember produced by the regression changes in each iteration dependent on the random geographic selection of stations, and the perturbations to observations to account for instrumental precision. At the far end of the distribution, this results in possible perturbations of the meteoric endmember of up to 21‰! This is a much greater degree of uncertainty analysis, with a much wider array of endmembers tested than any of the published studies.

All of this has been described clearly in every version of the manuscript. The results and impacts of the uncertainty are explicitly described in the text, and the error bars in the mean meteoric water column inventory by year figure represent those analyses. Our updated analysis includes all the previously accounted for uncertainty, with the addition of precipitation – varied widely in both quantity and δ^{18}O – in the upper 200m.

The authors claim that the mean δ18O of the glacial melt may be represented by their methodology, but it does not offer a more accurate estimate of freshwater source fractions compared to previous studies by for instance Meredith et al. 2010 or Biddle et al., 2019 (but there are many other works), because without a proper GMW isotopic composition range, they might over or under estimate the freshwater source fractions.

We disagree. The results of the 3-endmember model used in this study, and by previous studies in the region (Meredith et al., 2008, 2010, 2013; Randall-Goodwin et al., 2015; Biddle et al., 2019) are *strongly* dependent upon the endmembers used – therefore more accurate endmembers will produce more accurate water fractions. We used the data collected in the study area to calculate the mean freshwater mixing endmember, while the other studies merely selected a plausible mean endmember. Using collected observational data to estimate the mean meteoric endmember directly allows meteoric water fractions derived from data collected from different labs without concern for potential calibration offsets – while if the same endmember is used with offset data, the meteoric water fractions *will* be significantly impacted. This is described in the main text, with further detail in the appendix. This is one of the key advancements of this study.

Our subsequent analysis has determined that >87% of the meteoric water input in this area consists of glacial meltwater. Extrapolations of δ^{18}O-salinity data have been used to determine glacial endmembers by numerous other studies. (Fairbanks, 1982; Paren and Potter, 1984; Potter et al., 1984; Jacobs et al., 1985; Hellmer et al., 1998; Jenkins, 1999; Meredith et al., 2008; Randall-Goodwin et al., 2015).

Using a midpoint between precipitation and glacial meltwater as a mean meteoric water endmember (which intrinsically assumes a 50/50 composition of precipitation and glacial melt), as in Meredith et al. (2008, 2010, 2013); Randall-Goodwin et al. (2015) is less appropriate, and will overestimate meteoric water inputs – especially in deep waters, where meteoric water *will* be pure or near-pure glacial meltwater. Ensuring that endmembers are accurate is critical for determining source water fractions using this approach. <u>Our approach better constrains the meteoric endmember using in-situ data</u>, producing more accurate source fractions. This is one of the advances we highlight in this paper, but this reviewer does not seem to appreciate the nature of the advance. We have tried to make this point more clearly and prominently in our revision.

In our revision, we make an additional advance the method through use of a 2-component GMW-precipitation meteoric water endmember in the upper 200m, further improving the accuracy of the technique.

An invariant δ18O endmember is not a surprise as we would not expect GMW isotopic change in annual time scales.

We agree. This was included to show the reliability of the technique year-on-year, with data from different laboratories.

The primary source of uncertainty lies in the meteoric endmember, which dominates the uncertainty in the freshwater fractions. Furthermore, subsurface inputs of glacial meltwater can be differentiated from other freshwater sources based on potential temperature and dissolved oxygen concentration, making δ18O analysis less advantageous.

We disagree. We have significantly reduced the uncertainty associated with the meteoric water endmember here. if anything, $\delta^{18}O$ measurements when used for GMW estimation are more accurate and are at least additive to other tracers, including dissolved oxygen and noble gas concentrations. Further, traditional hydrographic tracers (and noble gases) cannot be used to estimate GMW in the upper water column due to influence from atmospheric processes and sea ice melt (Jenkins, 1999; Biddle et al., 2017, 2019). $\delta^{18}O$ has no such limitation, which is why $\delta^{18}O$-derived meteoric water estimates are used to calibrate new techniques to detect GMW in surface waters from satellites (Pan et al., 2023). We have improved this technique further in the revision, by including a realistic fraction of precipitation in the mean meteoric water endmember used there.

Since the authors concentrated solely on meltwater fractions in the main manuscript without discussing any sea ice influence, they could be calculated using the Gade line analysis. Therefore, I fail to see the advantage of employing δ18O in this context, again.

We have added a section on sea ice melt to the main body of the manuscript, pulling much of the sea ice melt discussion from the Appendix forward. The focus of our paper is on using a large $\delta^{18}O$ dataset (the largest ever, for a study of this kind – the unpublished data included in this study increases the *total* amount of Antarctic $\delta^{18}O$ data available by nearly 60%) to discuss glacial meltwater. Sea ice is discussed, but not a focus, and in any event is unlikely to impact our results. Recent advancements in remote sensing of GMW calibrate using $\delta^{18}O$ estimates because $\delta^{18}O$ can be used to estimate meteoric water in near-surface waters, which traditional hydrographic tracers cannot.(Pan et al., 2023)

- Another issue is the potential outlier or interannual variability in the year 1994. The claim of an increase in meteoric water inventories based on data spanning only seven years between 1994 and 2020, with limited observations in the initial year, lacks statistical significance.

Our geographic sensitivity and randomization analyses indicate that even with the small number of sample locations, that a meaningful average meteoric water inventory can be obtained from the 1994 dataset. However, the manuscript explicitly discusses the impact of the low inventory in 1994 as responsible for the trend (or lack thereof) and explicitly states that the data show greater interannual variability than trend in calculated meteoric water inventories. It is also mentioned explicitly that the uncertainty in 1994 may be artificially low due to excellent fit of the data, but relatively low number of samples.

The authors assert the stability of meteoric water inventories after 1994, but it's important to note that this conclusion is based solely on the data presented in this study, which spans only seven sampling years and may not fully represent the broader dynamics of meteoric water in the region.

We have been even more explicit in this latest revision that we are only discussing the data presented within it.

These findings also do not account for the significant 2012 event in the region, and overall, they suggest a relatively stable pattern. The absence of sensitivity tests on the linear regression is a concern, and the authors' reliance on visual assessment to justify the trend as an answer to my previous review is not scientifically robust.

We did perform extensive sensitivity tests on the linear regression (and the meteoric water fractions from which it is derived). Two different expressions of uncertainty are clearly displayed on the figure and discussed within the text. Details of the sensitivity and uncertainty analysis are in the main text, with further details in the supplement. The reviewer appears to have missed the presence of this text. We also note explicitly that the apparent trend is insignificant and based dependent on the low inventory in 1994.

- Additionally, it is unclear whether the authors are using problematic data in their analysis, likely stemming from sample storage issues. If these data points are problematic and from storage issues, they should not be discussed in the main results as they do not contribute meaningful insights to the study.

Great effort went into identifying and flagging problematic data, more than for any other seawater stable isotopic paper that we are aware of. This is *not* a problem with this data set. We suspect that this reviewer may not be well-informed on seawater stable isotopic analyses and QC'ing. We have, however, decreased the amount of discussion of this quality control process in the main text, instead directing readers who may be interested to the appendix.

L. 90: What does "scrutinized visually" entail or involve?

We elaborate on our sample and data QC for a portion of the 2009 data in the supplement to make the methods more explicit. When the water level in an unopened bottle is lower than its peers – it has likely been subject to evaporation. The text has been modified to make this point more clearly.

L. 96: If the comparison between CRDS and IRMS analyses revealed no discernible salt effect, it could be argued that this paragraph may not provide significant value to the current discussion and might be more appropriately placed in an appendix.

We added this based on earlier feedback from another reviewer who suggested that a salt effect may be impacting our results. In addition, the fact that the two methods yield similar results with seawater samples is not yet well-known or well published.

L. 140: I'm having difficulty comprehending the trends that the authors are discussing

We have multiple years in which we're calculating meteoric water fractions. Any change taking place across these years may be indicative of a trend. The fractions produced will be dependent on endmembers used in the model. We have tried to make this clearer in the revision.

L. 193: The characteristics of mCDW are derived from observations, while those of meteoric water are estimated rather than directly observed in this context

Meteoric water endmembers used in our study are estimated/calculated directly from our data, rather than using a largely hypothetical "plausible mean" value, as in other studies. We have addressed the issue with assuming a "plausible mean" meteoric water endmember both in the most recent submission, and in earlier responses. We have advanced our approach further by using a 2-component meteoric water endmember that includes precipitation in the upper water column.

Figure 3: If the authors assert that negative meteoric fractions, which are theoretically impossible, result from erroneously flagged data, they may need to consider either removing such data points and explain why from the analysis or adjusting the mass balance calculation to restrict negativity to only the sea ice term.

These do not result from erroneously flagged data. We used an average mCDW endmember. This means that some of the mCDW samples had less depleted $\delta^{18}O$ than our mean mCDW endmember (because that's how averages work). When you run the model on these sample points, the result is negative GMW. The same happens if the sample is saltier than the mCDW endmember. If we just use the max $\delta^{18}O$ and max S as our mCDW endmember, then no negative meteoric water fractions will be produced, but we do not feel that this would be appropriate.

All other published studies (Meredith et al., 2008, 2010, 2013; Randall-Goodwin et al., 2015; Biddle et al., 2019) using this technique also have negative meteoric water fractions at depth clearly visible in their figures, and in their data if you recalculate their fractions. They, however, do not acknowledge the negative fractions in the text. We carefully address this result, for any future readers who may notice what appears to be a theoretical impossibility.

L. 210: What is the rationale for discussing sea ice melt and mCDW fractions in the appendix? L. 219: What does the environmental variability of the model inputs entail?

Environmental variability is based on the range of possible values for each endmember, as all will vary naturally within the environment (e.g., mCDW is not a single S and $\delta^{18}O$ value, even within a single year, but rather varies slightly through space and time). The specific values by which each endmember was perturbed, and how they were derived, are described explicitly in Appendix A3, and referenced in the main text. "Environmental Variability" is the same terminology used in Biddle et al. (2019) for perturbation of the endmembers in their uncertainty analysis.

L. 233: I continue to be impressed by the superior analytical precision offered by CRDS compared to IRMS.

Thank you. We worked very hard over several years to develop methods and monitoring to achieve the level of precision and calibration we do using the CRDS. We're hopeful that the broader community can take advantage of our work, here.

L. 236: How do the authors go about estimating the uncertainty related to environmental variability? Additionally, while the term "environmental variability" is recurrent in the text, I'm still curious about the specific processes that the authors consider within this framework.

Described above.

L. 281: Is there any substantial evidence supporting the notion that the increased inventories in group B are indicative of basal melt accumulation, or is this interpretation more in the realm of speculation?

Modelling (Nakayama et al., 2019) and observational (Wåhlin et al., 2021) studies show the pathway of meltwater from PIG underneath Thwaites, before exiting beneath Thwaites on the NW.

L. 283: With the exception of those locations adjacent to the TIS, the sensitivity of sampling location remains a significant factor to consider.

While sampling location consistency will always be of some concern, the results of our spatial sensitivity and randomization analysis indicate that precise reoccupation may not be necessary to understand mean meteoric water content in the region. Strategic sampling could provide a meaningful mean inventory with relatively few sampling locations – furthering the utility of this technique for environmental monitoring.

L. 393: This is a speculative assertion.

Yes, it is. We have changed from "demonstrating the utility…" to "demonstrating the *potential* utility"

L. 397: The GMW inputs exhibit consistent standard deviations from 1994 to 2020. However, the authors do highlight a lower meteoric water content in 1994 and a higher content between 2000 and 2020.

It's not clear to us what the reviewer is asking.

Referee #2

I would like to thank the authors for considering the comments of both reviewers in the previous round. While I think that some aspects of the paper have improved and some concerns could be alleviated, one main concern raised by both reviewers, i.e. the novelty of the results and their implications, could have been given a bit more thought in the revision and the responses. I think that the paper remains rather technical without adding much discussion and understanding regarding the temporal variability of meltwater in the region, but certainly highlighting the usefulness of seawater isotopes and potential solutions to overcome certain issues. Therefore, I still think that these results should be published in principal, but my recommendation to think more about the implications still holds and might be further addressed in a revision (specific comment 1 below). While the revisions helped to clarify and better understand certain aspects of the analysis, they also brought up some new issues, that I think require further consideration in another major revision (specific comments 2-5 below). These issues concern in particular the interpretation of the sea ice melt fraction and the implication for the meteoric fraction in that balance, as well as the spatial analysis.

Specific comments:
1. I still think that putting the results a bit more in context would help. A particular example is the last sentence of the abstract. What does this imply? Could you add a last sentence with the implication of this result?

Thank you. We have added the following concluding sentence to the abstract, and manuscript to help strengthen and/or clarify our conclusions.

"The relatively long residence time in the SE Amundsen Sea allows changes in mean meteoric water inventories to diagnose large changes in local melt rates, and improved understanding of regional circulation could produce well-constrained glacial meltwater fluxes. Though local circulation, and melt-induced changes to circulation remain a source of uncertainty, the mean meteoric water inventories have remained relatively stable, and do not show a clear signal of accelerating melt rates. The advancement in our 2-component meteoric water technique improves the accuracy of the sea ice melt and meteoric fractions estimated from seawater $\delta^{18}O$ measurements throughout the entire water column and increases the utility for the broader application of these estimates."

2. Sea ice melt: I found the added discussion on sea ice melt/formation quite useful, but I also have some issues with the current interpretation:

a. The coastal/shelf regions around the Antarctic are well known as sea ice production sites, where ice is preferentially formed and then exported to the open ocean. Therefore, I would have expected a net sea ice formation signal from the region, as it is reported in the discussed paper by Biddle et al. (2019). I am surprised to see such strong positive signals in the column integrated sea ice melt. I understand that the samples come from the summer season. So a net sea ice melt at the surface can be expected. However, integrated over the water column, the signal should be close to zero or negative, unless sea ice is imported to the region or waters that are formed during sea ice formation in winter are not captured by these measurements. I understand that sea ice melt is not the focus of this study, but the results show that either the meteoric water endmember is too low (leading to an underestimation of sea ice formation), or water masses that are formed in winter during sea ice formation are not captured by these measurements. At least, I would be much more cautious on the interpretation of this result, as it seems counterintuitive.

The strong positive SIM signals in 2007, 2009 and 2014 at depths well below the surface (>200m) both result from samples diverging significantly from the δ^{18}O-S mixing line in the positive δ^{18}O direction (consistent with evaporation), suggesting that they may have been subject to evaporation. In the case of the 2009 data, we excluded any samples run in 2019/20 that appeared anomalous. The 2014 data has already been published in (Biddle et al., 2019), and so we have left it untouched.

We would argue that our mean integrated sea ice fractions *are* close to zero, given the size of the uncertainty (Table A6). The SIM fractions at below the surface are slightly negative in most years, resulting in a largely negative net SIM fraction, spatially (Fig A7). In all years, except 2007 and 2020, most locations show negative net SIM, e.g., sea ice growth. We note that the mean SIM inventories were positive for all years except 2009 – this result was produced by integrating the Gaussian fit of the data with depth and may have been unduly influenced by some samples with particularly high SIM fractions near the surface. All of the samples in this study were collected in the Austral summer. Ackley et al. (2020) observed “surface flooding” of sea ice melt in the Amundsen Sea persisting through mid-February – much of which was advected in from the Bellingshausen Sea. This may explain our observations, particularly for 2020, where almost no locations show net sea ice formation.

In our revised analysis, we include a fraction of precipitation in the upper water column and use a less negative meteoric endmember for those data. This re-analysis decreases sea ice fractions, bringing them closer to zero – with half of the years showing small net sea ice growth, and the other showing small net sea ice melt. The revised analysis also shows a stronger sea ice formation signal in the upper 100 m, more similar to that produced by Biddle et al. (2019).

b. The issue might arise from the way that the meteoric endmember is defined in this study using the water samples below 200m. As these samples have the advantage of not being influenced by sea ice and snow melt or precipitation at the surface (as motivated in the paper), they could well be influenced by sea ice formation as sea ice is formed in winter and the brine mixes deep in the water column. Consequently, this signal could pull the regression line towards saltier values, which could result in an artificial lowering of the meteoric endmember derived from the regression line. I think this issue might require some attention.

While winter water does not have a distinct fingerprint in salinity-δ^{18}O space (see Figure A1 in manuscript, where there is no δ^{18}O distinction between the WW and GMW mixing lines on the T-S plot), work by Jenkins (1999) showed that winter has a *less* negative δ^{18}O than waters influenced by GMW, in temperature-δ^{18}O space. Many other studies have used regressions of δ^{18}O-S data to accurately determine glacial meltwater endmembers. (Fairbanks, 1982; Paren and Potter, 1984; Potter et al., 1984; Jacobs et al., 1985; Hellmer et al., 1998; Jenkins, 1999; Meredith et al., 2008).

We performed 2 exercises to test the influence of winter water on our 0-salinity intercepts:

1) We applied an aggressive "winter water filter" to our data (removing all points with S>34 and Θ <-1.5°C) and recalculated our intercepts. In 3 years, the intercept was unaffected (2007, 2014, 2019), in 2 years, removing the "winter water" resulted in a *more negative* intercept (1994, 2000, 2020). Only in 2009 did the intercept value increase (by 0.3‰) after removing the "winter water" data from the regression. The largest change in intercept value was a decrease of 0.5‰ in 1994.
2) We selected only those data falling on the mCDW-GMW mixing line in T-S space, and then ran the salinity- δ^{18}O regression through only those data. Once again, our intercepts were unaffected.

Our uncertainty analysis uses a SD of 1.9‰ for the meteoric water endmember perturbation, on top of the changes in meteoric endmember resulting from instrumental precision and geographic sampling variability—much larger than any possible influence of winter water variability. Our meteoric water endmember as calculated using the 0-salinity intercepts is also remarkably consistent with the local ice cores, as noted in the manuscript. In our updated analysis, the upper water column meteoric water endmember is subject to further perturbation, widely varying both the fraction and δ^{18}O of precipitation.

c. As a consequence, I do not fully agree with the interpretation in lines 301 – 312. If anything, I would argue that in light of the net water column sea ice melt signal detected in this study, the higher estimate by Biddle et al. (2019), or some intermediate solution, seem more realistic.

While we maintain that the signal below 200 m is one of nearly pure GMW, we concede that waters in the upper water column may be influenced by less depleted freshwater sources. In our revised manuscript, we have re-run our analysis using a 2 component meteoric water endmember for depths shallower than 200m. Using the same approach used in the last manuscript to determine the fraction of total water column meteoric water consisting of GMW, we determine an approximate GMW fraction for only the upper 200m (~75%).

We use a precipitation endmember of -15‰, and to create a new compound meteoric water endmember between precipitation and the 0-salinity intercept, that we use at all depths shallower than 200m. In our updated uncertainty analysis, we vary the precipitation fraction by 5%, and the endmember by the standard deviation associated with snow collection at Halley Bay and Rothera Point. The 2-component meteoric water endmember approach results in a meteoric endmember ranging from -27.3‰ to -25.5‰, similar to the -25‰ endmember used by Biddle et al. (2019) and Randall-Goodwin et al. (2015).

This re-analysis results in higher meteoric water fractions in the upper 200m (where most of the meteoric water resides) and lower (often negative) sea ice melt fractions. While we feel that using a GMW endmember is most appropriate for depths well below the surface, using a precipitation-influenced endmember in the upper water column could produce a more accurate result for total meteoric water (and by extension, sea ice melt/formation).

d. I don't quite understand the reasoning (lines 648 – 655) that sea ice melt/formation signals might be an artifact of the potential evaporation from the bottles. Ultimately, one of the strength of using seawater d18O and salinity is to separate meteoric water and sea ice melt/formation signals. So, if there is an issue of evaporation in the bottles is should show up as a meteoric water signal and not a sea ice signal.

Water containing lighter isotopes (i.e. ^{16}O) evaporates preferentially, leaving the remaining sample with less ^{16}O (higher relative ^{18}O). Higher $\delta^{18}O$ values for a given salinity will produce increased (positive) sea ice melt fractions, and lower meteoric water fractions, since the result of the model is an overall mixture of meteoric water/sea ice melt/mCDW – if sea ice melt increases, meteoric water decreases, and vice versa – so ultimately, it will affect both. This can be clearly seen in both the meteoric water vs depth and sea ice melt vs depth profiles for 2014 – the data that fall off of the $\delta^{18}O$ -salinity mixing line in the positive direction (suggesting potential evaporation) directly correspond to those points showing anomalously high sea ice melt and anomalously low meteoric water fractions at those depths.

3. I have to say that I do not understand the unit of the meteoric water fraction e.g. in Tables 3 and 4. Why is it in g/kg? According to the definition in Eq. 1-3 it should be in %.

We used g/kg for consistency with other studies in the region using similar techniques (Jenkins et al., 2010; Jacobs et al., 2011; Randall-Goodwin et al., 2015; Jenkins et al., 2018; Biddle et al., 2019). Our 2014 data is the same data used in Biddle et al. (2019).

4. I do not understand what is shown in Table 4. Why do the results differ from Table 2 and 3 and what numbers should the reader trust? Wouldn't it make sense to report a single number that includes all the uncertainties (analytical, environmental, spatial)?

Table 2 shows the endmembers and associated uncertainty used in the "base" analysis.

Table 3 shows the results of uncertainty analysis for the model outputs (i.e. meteoric water fraction and integrated meteoric water inventory). Those results were produced by perturbing the endmember inputs by both analytical precision and environmental variability (for mCDW and meteoric water this is indicated in Table 2, and for sea ice melt we use values from the literature).

For Table 4, all of the data was left unperturbed, but we:

1. Randomly selected 3 stations
2. Calculated mCDW and meteoric water endmember using only those data
3. Calculated the mCDW, meteoric water and sea ice melt fractions for those stations using endmembers from [2]
4. Integrated the freshwater fractions with depth using the Gaussian fit (as we did with all of the data in Table 2)
5. Repeat steps 1-4 10,000 times
6. Record each of the results of [1-4]

The numbers in Table 4 are the averages and SD of each of the 10,000 simulations.

In our updated revision, we have streamlined our uncertainty discussion in the main body of the revised manuscript to show only a single inclusive measure of uncertainty, with detailed breakdowns of the attribution to each source of uncertainty in the appendix.

5. A) I found the spatial analysis that is being done quite informative. It appears that the spatial variability is larger than most of the temporal variability. This also suggests, that the small positive trend shown in Figure 4 may be an artifact or at least not coming from the Amundsen Sea glaciers.

Our sensitivity analysis indicates that endmember uncertainty has the greatest impact on mean meteoric water inventories (with spatial variability being a close second). While the results of the "grouping" analysis may suggest very large variability, this is almost entirely due to a few select locations with uncharacteristically high or low inventories – with the high inventories occurring alongside Thwaites Ice Tongue, and low inventories generally being the result of a sample depth resolution in stations furthest from the ice shelves. Our uncertainty analysis suggests that a meaningful mean may be obtained with a relatively small number of strategically located samples. We have some text to the discussion and conclusions to this effect.

None of the meltwater source regions b-d in Table 5 show a positive trend, i.e. an indication of increased melt from PIIS or TIS, or a large interannual variability. The overall variability in Figure 4, seems to be dominated by the variability in region a. So, what is driving the variability in region a?

With the exception of 1994 and 2009 (which use very little data, as indicated in the figure) There isn't significant variability in Region **a**. 2007 does appear higher, but given the size of the uncertainty around the inventories in each year, we don't think we can meaningfully draw any conclusions about spatial variability in this subregion.

I found it very difficult to interpret this result and as a consequence the result in Figure 4 and some of the overall findings of the paper. Could it be, that the variability observed in Figure 4 is dominated by variability induced elsewhere? Or could it be due to the variability in the source water mass, i.e. mCDW?

The error bars produced here include environmental and analytical uncertainty. Variability in melt observed here could be due to differences in melting upstream, or due to differences in mCDW transport toward the ice shelves. We were surprised that there seemed to be remarkably *little* variability, given the constraints of the study, and the extent to which we introduced and propagated errors in our uncertainty analysis.

B) I also do not understand how to reconcile the total region average meteoric water column inventory (Table 3 and 4) and the average values for the subregions for a given year (Table 5). In 2020, the total values in table 3 and 4 are 9.2 and 8.8 m, respectively. However, the regional values are all 8.7 m or lower. How is it possible that the overall average is higher than the regions separately?

In the regional analysis, we defined the mCDW and meteoric water endmembers (as described in the methods) in each sub-region using only data from that sub-region. As a result, each region uses slightly different endmembers than the analysis including all of the data. The endmembers used in each sub-region are detailed in Table A2.

In the case of 2020, the data from groups c & d had a salinity lower than the salinity max for the whole region. Both groups also produced a meteoric endmember slightly more negative meteoric endmember than the data for the region as a whole, and the intersection line between the S-max and the linear regression through the >200m S-δ^{18}O data produced a slightly more negative mCDW δ^{18}O endmember than using the whole region's data.

These three factors all produce slightly lower meteoric water fractions than what are produced using the entire region's data in aggregate to calculate endmembers from. For group a in 2020, mCDW does fall at the S-max for the region, however the linear regression through data also gives the mCDW a more negative δ18O data than when the data for the whole region is used – once again resulting in slightly lower meteoric water fractions.

The case is much the same with the spatial sensitivity analysis, wherein using data from only 3 randomly selected stations most often produced mCDW endmembers that were less salty or more depleted than the region as a whole, and/or produced more negative meteoric water endmembers using the linear regression through the δ^{18}O-S plots.

Had we used the same endmembers for each of these analyses, you would not see the apparent discrepancy, however the purpose of the exercise was to illustrate the differences produced using only data from those groups of stations (including for the endmember calculation). The size of the error bars produced, accounting for analytical precision, endmember uncertainty, and spatial variability all have complete overlap in the mean values.

Variation between these results is to be expected, but our analysis shows that they are remarkably similar. We have added additional discussion to the results to explicitly address this apparent anomaly.

Technical corrections:
- Lines 212-221: Despite the previous review comment, Figure 4 is still not referenced in this paragraph.

Apologies for this oversight – it has been corrected.

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