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

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Responses to Referee Comments

We would like to thank the referees for their comments – it's clear that they were thorough and thoughtful in carefully reviewing our manuscript. We have carefully considered and worked to address all of the comments, and the manuscript has been greatly improved as a result.

Both a "clean" and "marked-up" version of the revised manuscript have been uploaded. All line numbers used in comment responses reference the revised "clean" version of the manuscript.

Response to Referee #1 Comments

General comments:

The authors have done an important job in assembling oxygen-18 observations in this study to investigate meltwater inventories in the Amundsen Sea sector of WAIS. It is of great interest for the community to address questions related to freshwater flux evolution especially in this region. However, I was left wondering what the key message is here and what the results are.

> We have changed the title of the manuscript to reference "meteoric water."
>
> We have rewritten the results section and added more information about changes in meltwater content and the influence of precipitation in a new discussion section (Section 4.2, L313-356)
>
> We have removed the term "glacial meltwater" (GMW) when referring directly to the results but argue that >90% of the measured meteoric water consists of GMW. We carried out an analysis where we recalculated the meteoric and sea ice melt water fractions after adding 2 years worth of precipitation to the water column – the results of this analysis are described in depth in a new Appendix. (Appendix A5, L600-620, Table A4)

I am a bit confused about the main results and it seems the authors are as well. The flow and progression of ideas is missing with a lot of unsupported speculation rather than solid results and I wonder what the conclusion is here. While some paragraphs are largely true because they are based on a literature review, I am not sure why they are sometimes presented suddenly and how the results of this study influence them.

> We have tried to better explain or eliminate extraneous information throughout the manuscript. We have expanded the discussion of spatial (Section 3.4.2, L250-286), seasonal and interannual variability (Section 4.3, L358-400).
>
> Much of the spatial sensitivity analysis has been moved into the main body of the manuscript, and we now point more explicitly to the Appendix for further depth. We

have also added an additional, independent spatial sensitivity analysis. (Section 3.4.2, L250-286)

The glacial meltwater (GMW) term is used in different places, while in other places it is referred to as meteoric freshwater; the terminology is inconsistent and may reflect the suspect methodology used here; it is not possible to separate glacial meltwater from precipitation directly from the combined salinity and 18O observations.

While it is true that precipitation cannot be explicitly separated from GMW on the basis of δ^{18}O and salinity alone (since GMW too, consists of precipitation), the δ^{18}O of Antarctic precipitation at sea level differs substantially from its values in continental precipitation (and thus glacial ice). This is confirmed both by our observations, and by several other studies. We have significantly elaborated on this in the discussion. (Section 4.2, L312-356)

While the data cover 26 years, there are in fact seven summers of observations that are not evenly spatially distributed across the region of interest with high spatial variability and a very low amount of data in 1994. While the authors claim a modest increase in meltwater, this is at odd with the insignificant change and interannual variability mentioned in different places in the manuscript that are ultimately consistent with downstream freshening in the Ross Sea. We are puzzled by these contradictory remarks on the evolution of meltwater content. If there is no change, it is difficult to see how this could influence downstream freshening.

A linear increase in freshening only requires a relatively constant influx of freshwater that is greater than the rate at which it is cycled out by saltier waters being brought on from off the continental shelf. However, the Ross Sea is out of scope for this paper, and so references to the Ross Sea and freshening therein have been removed.

The lack of strong signal is indeed surprising, as one would have assumed that increased melt from ice shelves in the region would have significantly influenced the meltwater content in the water column. A possible solution would be to adjust the focus of the study and concentrate on why there is such constant meltwater fraction which to me at least is an unexpected and interesting finding to investigate.

> The result of a relatively constant meltwater inventory (after 1994) is now elaborated in further detail in the discussion (L360-400) and conclusion (L402-412). The result is consistent with more recently published studies (Flexas et al., 2022). The meteoric water column inventories measured in our study might not directly pace mass loss as measured by satellite remote sensing methods, however, assuming relatively a constant seawater residence time, the relative inventories we calculate are indicative of a significant change in melt rates, and further sampling and analysis in the vein of the paper would be diagnostic of changes in the future, if and when they happen.

I apologize for the negative comments, I really think the paper needs more work to make the chain of reasoning clearer and the main results compelling. I have raised a few points below that I hope will be helpful for the authors.

Specific points:

Line 38 – I think the authors can add references here, many studies have included 18O to the temperature-salinity combination to define the characteristics of SO water masses.

> We have added the following references: $\delta^{18}O$ (Jacobs et al., 1985, 2002; Meredith et al., 2008, 2010, 2013; Brown et al., 2014; Randall-Goodwin et al., 2015; Silvano et al., 2018; Biddle et al., 2019).

Line 38-39 – The sentence is a bit confusing; zero-salinity is probably too much and I would mention meteoric water rather than glacial freshwater; 18O is useful for differentiating freshwater signals coming from meteoric (precipitation and continental ice) or oceanic (sea ice) sources. References are also needed here

> We are puzzled about “zero-salinity” term being “too much”. Meteoric water (which includes precipitation and glacial meltwater) has zero salinity. We have elaborated in the discussion about how $\delta^{18}O$ and salinity can in-fact be used to discriminate between

meteoric water sources, as sea-level precipitation at this latitude has a very different $\delta^{18}O$ relative to even low-altitude continental ice. We have added the following references: (Jacobs et al., 1985; Hellmer et al., 1998; Jacobs et al., 2002; Meredith et al., 2008; Randall-Goodwin et al., 2015), as well as some analysis of all available Antarctic precipitation $\delta^{18}O$ data from the IAEA Global Network of Isotopes in Precipitation (GNIP) database. A section addressing this explicitly has been added to the discussion (Section 4.2, L313-357).

Line 39-40 – I do not understand this sentence. Are the authors saying that only in regions where basal melting is deep (and deep relative to what?), glacial meltwater is more depleted in oxygen-18 than local precipitation? Or is the content of glacial meltwater more important than the content of local precipitation in these regions? I am not convinced in either case. Are there any references to support these claims?

This has been edited for clarity. The intention of that sentence was to explain how the subsurface introduction of meteoric water must be dominated by glacial meltwater. Glacial meltwater is a significantly greater freshwater contributor than precipitation in this region, as now elaborated on in the discussion (Section 4.2, L313-357).

A section has been added to the Appendix wherein we examine the impact of 2 years of precipitation on the calculated meteoric water inventories. (Appendix A5, L600-620, Table A4)

Figure 1 – The wide map of Antarctica is not very useful here nor is panel b showing only bathymetry. I am not sure how relevant they are to the results. Jet colormap is not perceptually inconsistent and a poor choice for data visualization as it can mask significant changes.

We have changed the colormap to something more linear and added a simplified diagram of the local circulation. We have retained the wide map of Antarctica to locate the field site, and the relative location of the shelf break is likely to be helpful for many readers. We removed the bathymetry-only map and added bathymetry to the maps showing sample locations and column inventories. Sea floor topography provides information about bathymetrically-influenced ocean flow pathways.

Line 84 – Actually no, meteoric freshwater can be continental ice or precipitation, as they cannot be separated on the basis of salinity and 18O.

We now expand on our rationale in the discussion, noting the very different δ^{18}O contents of sea-level precipitation at this latitude and melting continental ice formed at higher latitudes and elevations. In addition, the highly ^{18}O-depleted freshwater at depths hundreds of metres below the surface mixed layer have δ^{18}O values consistent with glacial ice and not with local precipitation (Section 4.2, L313-357).

Line 85 – (Data and Methods)

Line 84-93 – I am not sure about the analyses here. Clearly, the 18O endmember does not vary on interannually, the differences among -28.4‰ and -30.2‰ certainly reflect uncertainty in the data. I do not see the need to separate years here to determine endmembers since the isotopic composition of the continental ice is not expected change suddenly from one year to another. Furthermore, as the authors point out and by Masson-Delmotte et al., 2008 show, there is a large variability in 18O of meteoric water on a local scale, so estimating the endmember separately every year is a key result here.

Although there is some analytical uncertainty in the data, variability within and between (0.8‰) our intercepts is significantly smaller than the variability seen throughout local ice core depth profiles (1.9‰, Steig et al., 2005). Since this study compiles data from different laboratories using different techniques, inter-laboratory variation could impact the results, if the same endmembers are used for each year. Salinity and δ^{18}O data produce a strong mixing line each year, and we think the use of annual mixing line endpoints is the most appropriate procedure for our purposes.

Masson-Delmotte et al., (2008) do not show large local variability, but demonstrate that 88% of the variability in Antarctic precipitation δ^{18}O can be attributed to latitude, altitude, and distance from the coast, with altitude having the strongest impact. Their data, and the output from their model show coastal sea-level precipitation at the latitude of our field site with a $\delta^{18}O$ of ~-15‰ – consistent with $\delta^{18}O$ content of multiple local precipitation samples we collected in 2019, and with several other studies now cited in our discussion (Gat and Gonfiantini, 1981; Ingraham, 1998; Noone and Simmonds, 2002) We also place these measurements and data in context of available $\delta^{18}O$ data from the IAEA Global Network of Isotopes in Precipitation (GNIP) database. (Section 4.2, L313-357).

Figure 2 – There is no discussion of the scatter toward lower salinity and constant 18O from the mixing line above 200 m depth and even in the deep waters in 2009 and 2014. It would be interesting to explore and discuss sea ice imprint if possible

We discuss the scatter at shallow depths as the influence of sea ice, and the scatter shown in 2009 and 2014 in deeper waters as likely resulting from sample storage issues. The revised text makes this more explicit and expands on data quality and interlaboratory offsets in the appendix. (Appendix A2, L456 – 483)

While sea ice fractions are another output from the three-endmember mixing model we use, our focus on meteoric waters includes a specific link to ice shelf basal melt. We have added a discussion of the sea ice melt/formation and mCDW fractions to the appendix. (Appendix A6, L637 – 682)

Line 104 – sea ice melt and/or sea ice formation. The mixing model can give a negative estimate for sea ice endmember reflecting net sea ice formation.

We have amended the text accordingly.

Line 110 – sea ice and meteoric water are not water masses. I would rather write water sources.

We have amended the text accordingly.

Line 125 – influenced by sea ice melt and formation. But also, non-local precipitation, mixing, advection

Text has been amended to include sea ice formation, and non-local precipitation. Discussion of mixing and advection are expanded upon in the discussion. (Section 4.2, L313-357).

Line 128 – I am not sure how this approach differs from studies that use an average 18O for meteoric water. Biddle et al., 2019 and Meredith et al., 2010 do not use approximate 18O values for glacier but a plausible average of meteoric water because again 18O does not disentangle continental ice and precipitation. Using the zero-salinity intercept on 18O-salinity plots, the authors use the same method; they deduce an average 18O of meteoric water in region where this component is highly variable (Masson-Delmotte et al., 2008).

Meredith et al., 2008, 2010, 2013; Biddle et al., 2019; Randall-Goodwin et al., 2015 use plausible average meteoric water values. Meredith et al. 2008 shows mCDW-precipitation and mCDW-glacial melt mixing lines on salinity-$\delta^{18}O$ plots, with surface water observations falling between the two, as they did for their “mean meteoric water” calculations. Biddle et al., 2019 adopts values used by Randall-Goodwin et al. 2015, who followed Meredith at al. 2008 by selecting a midpoint between a range of freshwater endmembers.

In this study, we demonstrate that by producing a mixing line between mCDW and a freshwater (meteoric) endmember for subsurface (>200m) depths, the extrapolated 0-salinity intercept more tightly constrains the (glacial) meteoric water endmember. This procedure reveals the fingerprint of glacial meltwater introduced at depth. While surface waters may contain precipitation with a less-depleted $\delta^{18}O$ signature than deep waters, we demonstrate that >92% of the meteoric water can be assumed to be comprised of glacial meltwater.

Choosing a midpoint between precipitation and glacial ice as an “average” meteoric water endmember assumes a 50/50 mixture of precipitation and glacial melt. Since the meteoric water is dominated (>92%) by GMW, using a precipitation-GMW midpoint endmember will overestimate meteoric water fractions and underestimate sea ice melt fractions. It could be argued that an endmember comprised of ~92% GMW (~-30‰)

and ~8% precipitation (~-15‰) would be the most appropriate "mean" meteoric endmember (~-28.8‰), however our primary interest is in basal melt, so using the zero-salinity freshwater defined by δ^{18}O-salinity >200m is most appropriate.

A new discussion section on the utility of δ^{18}O intercepts has been added (Section 4.1, L286-312)

Line 149 – The data are collected during summers so it is unlikely that GMW endmember values are based on the average of the annual data

This is a miscommunication in phrasing. What we intend to describe is that the meteoric water endmember we use for each year is the one produced using only that year's data. While these data were all collected during summer, and the region experiences seasonal variability, the residence time of seawater here is ~2 years (Tamsitt et al., 2021), reducing the impact of seasonality. We have added discussion about the seasonal variability of mCDW in the region. This has been expanded upon and covered explicitly in a new discussion section (Section 4.3, L358-400)

Line 160-162 – I do not see the connection between the results discussed in figure 3 and the mCDW heat extent

The discussion of remaining mCDW heat and the formation of a polynya are perhaps beyond the scope for this study, and we have removed this inference.

Line 145-162 – I am not convinced that the authors are properly discussing the results here, but rather a speculative explanation of the origin of glacial melt

We have changed references to GMW to meteoric waters when discussing our results. However, the distributions of meteoric water shown in our results indicate introduction of basal meltwater at depth. Revised figures show this more clearly, as do added references demonstrating the depth of outflow from beneath ice shelves. (Biddle et al., 2017; Naveira Garabato et al., 2017) (L203-208)

Line 167 – The authors point to a modest increase (what is a modest increase? relative to what?) of the mean GMW inventories and at the same time acknowledge that the low average in 1994 may be responsible for this ‘trend’. The low number of samples in 1994 should be reflected in the estimate, and if this year is not taking into account, no claim of an increase can be made. Also, how is the linear trend calculated? Are there any uncertainties associated with this calculation?

The linear trend is calculated from the mean meteoric water column inventories as calculated using the Gaussian fit lines depicted in Figure 3. This has been made more explicit (L214-219). The low number of samples in 1994 is accounted for in the depicted uncertainty, aligns with estimates from other years, and displays the strongest fit.

We have added further expressions of uncertainty to Figure 4, and discussion of the pattern of results – revealing little about changes in meteoric water inventory, other than an increase since 1994. We also discuss the consistency between the pattern of our results and a more recently published modeling study (Flexas et al., 2022).(L367-376)

Line 168-170 – I do not see the link between these two sentences and the discussion of results here, what is the connection between interannual GMW inventories in summer and seasonal variability of mCDW in the region? It is hard to understand; the authors claim a modest increase of GMW inventories and then refer to invariable overall melt rates during the austral summer when the samples used in this study were collected (do they mention a particular year?)

The mention of relative stability in mCDW and melt rates, along with the ~2 year residence time of waters in this region was intended to show that the meteoric water inventories described by our results are not simply a seasonal melt signal, but representative of a longer period of change. The discussion of variability in mCDW properties is intended to describe the impact of a source of uncertainty. This has been rewritten for clarity, and the discussion of the impact of mCDW variability moved to the discussion. (L377-383)

Figure 4 – It is nice to see the series here, but I doubt the authors are showing a volume as mentioned in the caption. Also, integrating from the surface will include precipitation, even if it is negligible. Therefore, I do not really agree with the following assumption; depth-integrated GMW between surface and 800 m depth. How is the linear regression calculated? What is the uncertainty? If the GMW inventory is time invariant as the authors claim, beside a modest increase (a choice has to be done here), I think the linear regression in the figure does not provide crucial information.

The caption has been amended to show meteoric water content. An added section discusses the impact of precipitation on the meteoric water column content (Section 4.2, L313-357). The linear regression is calculated from the meteoric water column inventories produced from the Gaussian fits in Figure 3, updated with appropriate uncertainty expressions. While the trend is not statistically significant, an increase from 1994 is evident "by eye" and we feel the trend is worth retaining.

Line 177 – This section belongs to Methods rather than Results Line 178-180 – I assume this analysis corresponds to Appendix A4?

We have retained uncertainty analysis in the Results section, but it has been rewritten and now contains several tables and figure displaying the results of uncertainty analyses. References to the relevant appendices have been made more explicit throughout the text. (Section 3.4, L230-285

Line 181 – Then why use a single salinity and 18O value for mCDW each year if the authors claim that this water mass is relatively stable over time?

The data compiled for this study comes from different labs and was analyzed using different techniques. Calculating results based on each dataset independently minimizes concerns over inter-lab offsets (L103, L136, L195, Appendix A2: L456-483).

We have expanded the discussion of seasonal and interannual variability of mCDW (L377-383) in the discussion and added another section to the Appendix about data quality and offsets. (L186-190, Appendix A2: L456-483)

Line 184 – Do the authors really compare GMW content and 18O-salinity relationships? I do not see any comparisons of 18O-salinity relationships in Appendix A4

> The meteoric water endmembers used for each geographic grouping were defined based on δ^{18}O-salinity data for only those stations, so any comparison of the meteoric water endmembers effectively compares the δ^{18}O-salinity relationships. Appendix A4 has been rewritten for clarity. (L531-598)

Line 186 – I am not convinced to the authors' decision to simply remove the 2014 data near to TGT from the analysis (which are still shown in figure 1 in glacial meltwater inventory panel g and I assume in figure 2 as well? Which is confusing) to improve interannual comparability. I would keep all available data from the region if the aim is to make comparisons on a regional scale. Excluding data because the GMW values are simply higher seems problematic and not a good reason to me.

> We have added the 2014 data alongside Thwaites back into the broader analysis and discussion, noting the higher inventories at these sampling locations. There is also further discussion of the inventories alongside Thwaites in our updated spatial sensitivity analysis. (L250-286)

Line 196 – But the authors stated earlier that the properties of mCDW endmember are invariant

> This is a description of model sensitivity. While the mCDW signatures are relatively stable and the most well-constrained endmember, changes to the selected mCDW endmember have a larger impact on the outputs of the 3-endmember mixing model than changes to other endmembers. (L231-249, Appendix A3: 485-527)

Line 199 – Unable to estimate glacial meltwater using salinity and 18O

> See responses to earlier comments re: distinguishing between sea level vs continental precipitation. content (Section 4.2, L313-357)

Line 201 – and compare GMW fractions rather than d18O values. Not helpful, it did not need to be said

> This was intended to emphasize that the calculated meteoric water content will not be influenced by inter-laboratory variability in δ^{18}O values with our method. The discussion has been rewritten.

Line 203 – Are there any uncertainties in the meltwater content values? I do not think low and high are useful here

> Uncertainties in the meteoric water content values are described in that analysis. Corresponding uncertainties have been added to the discussion where relevant. (Section 3.4, 230-286)

Line 215 – Hard to tell if 2000 was a local high compared with subsequent years due to uncertainties. And what about the year 2020?

> We have rewritten the discussion. 2020 measures the highest average meteoric water column inventory (though 2000 is very close). We have also added a table summarizing the results. (Table 3, L246)

Line 217 – Again, it is not clear that there was an increase in average GMW after 1994

> Discussion has been rewritten to more appropriately describe the increase after 1994, followed by relative stability thereafter. (L367-376)

Line 219 – How is a steady GMW inventory consistent with a linear, long-term freshening trend? This assumption, which I think is confusing, does not add much here because the study does not examine the contribution of freshwater input on the reported downstream freshening in the Ross Sea

Linear freshening in the Ross Sea does not require accelerating freshwater input – only consistent freshwater input at a level above its output rate. However, discussion of the Ross Sea freshening is out of scope for this paper and has been removed.

Line 222 – meteoric water inventories from the surface to 800 m. Integrating from the surface will include precipitation

We have added a section to the discussion with more in-depth analysis and discussion of the influence of precipitation on meteoric water inventories, including a new Appendix. (Section 3.4, 230-286, Appendix A5: L600-620)

223 – volume or content?

Corrected to content.

224 – gyre-like circulation; adding regional circulation to the map would be helpful and it would influence meltwater advection

We have amended Figure 1 to include a simplified schematic of local circulation.

243 – If it is statistically insignificant, there is no linear increase, I am not sure it is useful

We have modified our discussion of the trend in meltwater inventories (L358-400), however retained the description of the linear increase, with multiple updated measures of uncertainty.(L211-229)

244 – This is tricky, because of the error bars, the lowest and highest melt periods claimed seem difficult to believe

Text has been revised to better describe the uncertainty and (lack of) significance in the trend. We have also added multiple expressions of uncertainty to the figure. (L211-229, L358-400)

250 – 18O observations do not allow estimation of basal melt rates

While $\delta^{18}O$ (and salinity) observations alone cannot allow the estimation of basal melt rates, with the ~2 year residence time of waters here, so our results will integrate meteoric (GMW) content over a similar timescale. A sudden change in the average meteoric water column inventories are strong indications of a change in basal melt rates, and would be diagnostic thereof. The discussion has been amended to more clearly reflect our intention. (L358-400),

251 – interannual fluctuations potentially masking an increase over 2.6 decades; this is very speculative

We have removed this sentence from the conclusion and rewritten it in a way that is less speculative, and more explicitly referential to our results. It now focuses more on the technique's utility for diagnosing changes in melt rates (assuming a relatively constant residence time). (L401-422)

255 – the last sentence is very confusing; how can meltwater volume rates be measured 18O observations? How is the invariant GMW inventory mentioned in the paper consistent with any downstream freshening

As mentioned previously – a linear freshening only requires a constant influx of freshwater that is out of balance with the system's capacity to cycle it out. However, Ross Sea freshening is out-of-scope, and we have removed this mention. We have changed the reference to "meltwater volume" to meltwater content.

As in response to a previous comment, while $\delta^{18}O$ and salinity cannot be used to directly and explicitly assess melt rates, a sudden change in measured meltwater content would be diagnostic of a change in basal melt rates, since the measured meteoric (meltwater) content integrates melt over the residence time (~2 years) of waters here. (L367-376)

Response to Referee #2 Comments

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) (L301-312).

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. (L250-286)

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 δ^{18}O data) but was compiled from multiple datasets collected independently for different projects. This has now been clarified in the Introduction and Methods (L74). 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. (L358-400)

The motivation for this study was to aggregate as much δ^{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 reference 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. (L358-400)

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. (L103, L136, L195, Appendix A2: L456-483).

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-δ^{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. (288-312)

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. (L261-272)

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**.** (L252-260)

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 δ^{18}O signatures observed across the whole field site, and for meteoric water, the uncertainty is based on the standard deviation of δ^{18}O in the nearest ice core (ITASE01-2). (Appendix A3: L485-526)

The spatial sensitivity analyses, with supplementary tables have been moved up into the main body of the manuscript,(L230-286) with more detailed results tabulated in the Appendix. (L531-598)

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. (L286-400) 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. (L109)

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

  Added reference to sea ice formation. (L110)

- 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. (L200-202, Appendix A3: L522-526)

- 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. (L219, L231-249)

- 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. (L237, Table 3)

- 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. (L410-412)

- 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.

  We have added a description of the period captured by these meltwater measurements. (L377-383)

- 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. (L231-249)

- 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., Heywood, K. J., Kaiser, J., and Jenkins, A.: Glacial Meltwater Identification in the Amundsen Sea, J. Phys. Oceanogr., 47, 933–954, https://doi.org/10.1175/JPO-D-16-0221.1, 2017.

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.

Brown, P. J., Meredith, M. P., Jullion, L., Naveira Garabato, A., Torres-Valdés, S., Holland, P., Leng, M. J., and Venables, H.: Freshwater fluxes in the Weddell Gyre: results from δ18O., Philos. Transact. A Math. Phys. Eng. Sci., 372, 20130298, https://doi.org/10.1098/rsta.2013.0298, 2014.

Flexas, M. M., Thompson, A., Schodlok, M., Zhang, H., and Speer, K.: Antarctic Peninsula warming triggers enhanced basal melt rates throughout West Antarctica, Sci. Adv., 12, 2022.

Gat, J. R. and Gonfiantini, R.: Stable isotope hydrology: deuterium and oxygen-18 in the water cycle. Technical reports series No. 210, 1981.

Global Network of Isotopes in Precipitation (GNIP): Water Isotope System for data analysis, visualizaiton and Electronic Retrieval (WISER), IAEA, 2023.

Hellmer, H. H., Jacobs, S. S., and Jenkins, A.: Oceanic Erosion of a Floating Antarctic Glacier in the Amundsen Sea, in: Ocean, Ice, and Atmosphere: Interactions at the Antarctic Continental Margin, vol. 75, edited by: Jacobs, S. S. and Weiss, R. F., American Geophysical Union, Washington, D. C., 83–99, https://doi.org/10.1029/AR075p0083, 1998.

Ingraham, N. L.: Chapter 3 - Isotopic Variations in Precipitation, in: Isotope Tracers in Catchment Hydrology, edited by: Kendall, C. and McDONNELL, J. J., Elsevier, Amsterdam, 87–118, https://doi.org/10.1016/B978-0-444-81546-0.50010-0, 1998.

Jacobs, S. S., Fairbanks, R. G., and Horibe, Y.: Origin and evolution of water masses near the Antarctic continental margin: Evidence from H2 18O / H2 16O ratios in seawater, Oceanol. Antarct. Cont. Shelf, 43, 59–85, https://doi.org/10.1029/AR043, 1985.

Jacobs, S. S., Giulivi, C. F., and Mele, P. A.: Freshening of the Ross Sea during the late 20th century., Science, 297, 386–389, https://doi.org/10.1126/science.1069574, 2002.

Masson-Delmotte, V., Hou, S., Ekaykin, A., Jouzel, J., Aristarain, A., Bernardo, R. T., Bromwich, D., Cattani, O., Delmotte, M., Falourd, S., Frezzotti, M., Gallée, H., Genoni, L., Isaksson, E., Landais, A., Helsen, M. M., Hoffmann, G., Lopez, J., Morgan, V., Motoyama, H., Noone, D., Oerter, H., Petit, J. R., Royer, A., Uemura, R., Schmidt, G. A., Schlosser, E., Simões, J. C., Steig, E. J., Stenni, B., Stievenard, M., van den Broeke, M. R., van de Wal, R. S. W., van de Berg, W. J., Vimeux, F., and White, J. W. C.: A Review of Antarctic Surface Snow Isotopic

Composition: Observations, Atmospheric Circulation, and Isotopic Modeling*, J. Clim., 21, 3359–3387, https://doi.org/10.1175/2007JCLI2139.1, 2008.

Meredith, M. P., Brandon, M. A., Wallace, M. I., Clarke, A., Leng, M. J., Renfrew, I. A., van Lipzig, N. P. M., and King, J. C.: Variability in the freshwater balance of northern Marguerite Bay, Antarctic Peninsula: Results from δ18O, Deep-Sea Res. II, 55, 309–322, https://doi.org/10.1016/j.dsr2.2007.11.005, 2008.

Meredith, M. P., Wallace, M. I., Stammerjohn, S. E., Renfrew, I. A., Clarke, A., Venables, H. J., Shoosmith, D. R., Souster, T., and Leng, M. J.: Changes in the freshwater composition of the upper ocean west of the Antarctic Peninsula during the first decade of the 21st century, Prog. Oceanogr., 87, 127–143, https://doi.org/10.1016/j.pocean.2010.09.019, 2010.

Meredith, M. P., Venables, H. J., Clarke, A., Ducklow, H. W., Erickson, M., Leng, M. J., Lenaerts, J. T. M., and van den Broeke, M. R.: The Freshwater System West of the Antarctic Peninsula: Spatial and Temporal Changes, J. Clim., 26, 1669–1684, https://doi.org/10.1175/JCLI-D-12-00246.1, 2013.

Naveira Garabato, A. C. N., Forryan, A., Dutrieux, P., Brannigan, L., Biddle, L. C., Heywood, K. J., Jenkins, A., Firing, Y. L., and Kimura, S.: Vigorous lateral export of the meltwater outflow from beneath an Antarctic ice shelf, Nature, 542, 219–222, https://doi.org/10.1038/nature20825, 2017.

Noone, D. and Simmonds, I.: Annular variations in moisture transport mechanisms and the abundance of δ18O in Antarctic snow, J. Geophys. Res. Atmospheres, 107, ACL 3-1-ACL 3-11, https://doi.org/10.1029/2002JD002262, 2002.

Randall-Goodwin, E., Meredith, M. P., Jenkins, A., Yager, P. L., Sherrell, R. M., Abrahamsen, E. P., Guerrero, R., Yuan, X., Mortlock, R. A., Gavahan, K., Alderkamp, A.-C., Ducklow, H., Robertson, R., and Stammerjohn, S. E.: Freshwater distributions and water mass structure in the Amundsen Sea Polynya region, Antarctica, Elem. Sci. Anthr., 3, 000065, https://doi.org/10.12952/journal.elementa.000065, 2015.

Silvano, A., Rintoul, S. R., Peña-Molino, B., Hobbs, W. R., Wijk, E. van, Aoki, S., Tamura, T., and Williams, G. D.: Freshening by glacial meltwater enhances melting of ice shelves and reduces formation of Antarctic Bottom Water, Sci. Adv., 4, eaap9467, https://doi.org/10.1126/sciadv.aap9467, 2018.

Steig, E. J., Mayewski, P. A., Dixon, D. A., Kaspari, S. D., Frey, M. M., Schneider, D. P., Arcone, S. A., Hamilton, G. S., Blue Spikes, V., Mary Albert, Meese, D., Gow, A. J., Shuman, C. A., White, J. W. C., Sneed, S., Flaherty, J., and Wumkes, M.: High-resolution ice cores from US ITASE (West Antarctica): development and validation of chronologies and determination of precision and accuracy, Ann. Glaciol., 41, 77–84, https://doi.org/10.3189/172756405781813311, 2005.

Tamsitt, V., England, M. H., Rintoul, S. R., and Morrison, A. K.: Residence Time and Transformation of Warm Circumpolar Deep Water on the Antarctic Continental Shelf, Geophys. Res. Lett., 48, https://doi.org/10.1029/2021GL096092, 2021.