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:

- One particular concern is the authors' argument, unproven, that the use of salinity and δ^{18}O 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 δ^{18}O 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.

- 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 δ^{18}O /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. 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 δ^{18}O/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. 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 δ^{18}O is challenging, as isotopic values in local precipitation and ice shelf melt are similar. The age and source of ice melting at the base of the ice shelves remain uncertain, further complicating the δ^{18}O analysis. Given these uncertainties, considering a larger range of plausible δ^{18}O meteoric endmembers based on regional literature would provide more realistic estimates with appropriate uncertainties. 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.

- 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. 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 $\delta^{18}O$ of GMW, which is weak given the accumulation of meteoric water in the WW layers from various sources, both local and remote. The authors claim that the mean $\delta^{18}O$ 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. An invariant $\delta^{18}O$ endmember is not a surprise as we would not expect GMW isotopic change in annual time scales. 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 $\delta^{18}O$ analysis less advantageous. 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 $\delta^{18}O$ in this context, again.

- 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. 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. 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.

- 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.

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

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.

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

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

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.

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?

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

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.

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?

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

L. 393: This is a speculative assertion.

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.