General comments:

The authors have done an important job in assembling oxygen-18 observations in this study to investigate meltwater inventories in the Admundsen 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.

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.

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 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.

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.

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

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

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?

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.

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

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.

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

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.

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

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

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).

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

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

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

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?

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?)

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.

Line 177 – This section belongs to Methods rather than Results

Line 178-180 – I assume this analysis corresponds to Appendix A4?

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?

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

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.

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

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

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

Line 201 – ~~this~~

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

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

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

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

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

Line 223 – volume or content?

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

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

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

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

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

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