Tromsø,
10.07.2024

Response to the comments from Laurie Padman on “Hydrography and circulation below Fimbulisen Ice Shelf, East Antarctica, from 12 years of moored observations”

We thank Laurie Padman for his constructive and extensive comments on our manuscript. Below, we have copied the comments, and we give our replies in blue color. As we do not submit a revised manuscript at this stage, we respond to the referee’s major comments from the discussion window, but not to the more detailed comments in the uploaded manuscript PDF. We have gone through these comments, too, and agree with a great majority of them. We will give a response to every single comment at a later stage along with a revised manuscript.

Review of Hydrography and circulation below Fimbulisen Ice Shelf, East Antarctica, from 12 years of moored observations, by Lauber et al.

I don’t need to be anonymous … Laurie Padman

Overall rating: I rated this "accepted subject to minor revisions" as I think the science is excellent and, despite all my comments, the presentation is good, my comments should all be fairly easily addressed, and the editor can assess whether the responses are sufficient. However, it is almost "major revisions", and I'm willing to re-review it.

This manuscript describes an impressive record of data, often longer than a decade, from three moorings that were deployed in 2009 in the ocean cavity beneath Fimbulisen. Analyses of these records are supported by other data sets from outside the cavity.
The authors use these data, with Optimum MultiParameter (OMP) analyses, to identify source water masses that enter the cavity, and to explore their seasonality. They conclude that Winter Water (WW) in the most important inflow, but that there are seasonally varying contributions from Antarctic Surface Water (ASW) and Warm Deep Water (WDW). The latter appears to be caused by warm-core eddies.
I am including a marked-up PDF with all my comments, both minor and major. There are a lot of these! However, the paper was well written and easy to read, and I think most comments are fairly “minor” and can be decided on quickly. Below, however, I will repeat some of the more major comments.

Thank you for taking the time to review our study and for your positive assessment.

MAJOR COMMENTS (repeating Anon Ref. #1)
I agree with all major comments by Anonymous referee #1, and these are repeated here for emphasis.
“I'm very concerned with the selection of ASW as an end member for the OMP analysis. Clearly, if ASW enters the cavity, and the authors do a good job of making the case that it does, then the analysis must handle it in some way. But ASW

properties change during the year and it makes some of the longer term trends in Fig 6, for example, highly suspect. As the winter comes on, we would expect ASW to be cooled to the freezing point quite rapidly. In any case, there will be a strong seasonal signal to its properties. Therefore, the trends we see in Fig 6 are almost certainly a result of changes in the end member properties.

This is partly handled by the large uncertainty bounds that the Monte Carlo analysis places on the time series of concentrations, but the authors still refer to seasonal variation in the different water mass concentrations. Placing large uncertainty on the ASW end member properties doesn't solve the problem, as the properties move in a well-defined way within the box in properties space - essentially, the distribution is not Gaussian.

Perhaps the authors can explain their thinking on this point. This can be resolved by carrying out experiments to see the extent to which a likely seasonal profile in ASW properties would change end member concentrations. That would then indicate whether more caveats need to be declared.

Below, we have copied our response to Anonymous Referee #1:

Thank you for raising this concern. We agree that the properties of ASW change during the year, and that this likely affects our quantification of the ASW fraction inside the cavity. To explore the extent of the effect of changing ASW properties on our results, we defined, as suggested, a seasonal profile of the ASW properties for the OMP analysis. To define this seasonal profile, we used the hydrographic climatology from Hattermann (2018) and data from the Seaglider sg564, both mentioned in the manuscript. We derived ASW properties from both datasets by averaging the properties of water at a potential density below 27.6 kg m^{-3} for each available month. Oxygen data (at an offset of 1.5 ml l^{-1}, which was removed) were only available from the Seaglider between December and March, so we interpolated the values in between. The resulting seasonal cycles of the conservative temperature, absolute salinity, and dissolved oxygen of ASW are shown in Fig. R1. We used these seasonal cycles as end member properties of ASW in the OMP analysis to re-do Fig. 6 in the manuscript. We did not use a seasonal profile for any other water mass than ASW, and we kept all end member uncertainties constant with time.

Fig. R2 shows the comparison between the new (seasonal cycle on ASW properties) and the old (no seasonal cycle on ASW properties) Fig. 6 from the manuscript. Adding the seasonal cycle to the ASW properties increases the values of the maximum ASW concentrations by around 20% at $M1_{upper}$ and 10% at $M2_{upper}$. However, it does not affect the timing of the maxima and the pattern of a rapid ASW increase and a slow decrease afterward. That is, the overall temporal evolution of the water mass fractions is not sensitive to the dynamical definition of ASW, only the absolute water mass fractions are.

A T-S diagram for $M1_{upper}$ and the Seaglider shows which water at $M1_{upper}$ can be derived from ASW after interaction with the ice base, following meltwater mixing lines. Water connected to the center of the “cloud” of ASW properties in T-S space is only found in March/April at $M1_{upper}$. This is why the timing of the maximum ASW concentration is not sensitive to the exact definition of ASW, as shown in Fig. R2. During the rest of the year, the meltwater mixing lines indicate that the water at $M1_{upper}$ is derived rather from winter water, although some mixing lines still cut the side of the ASW cloud, giving some remaining fraction of ASW independent of if it is given a seasonal profile or not.

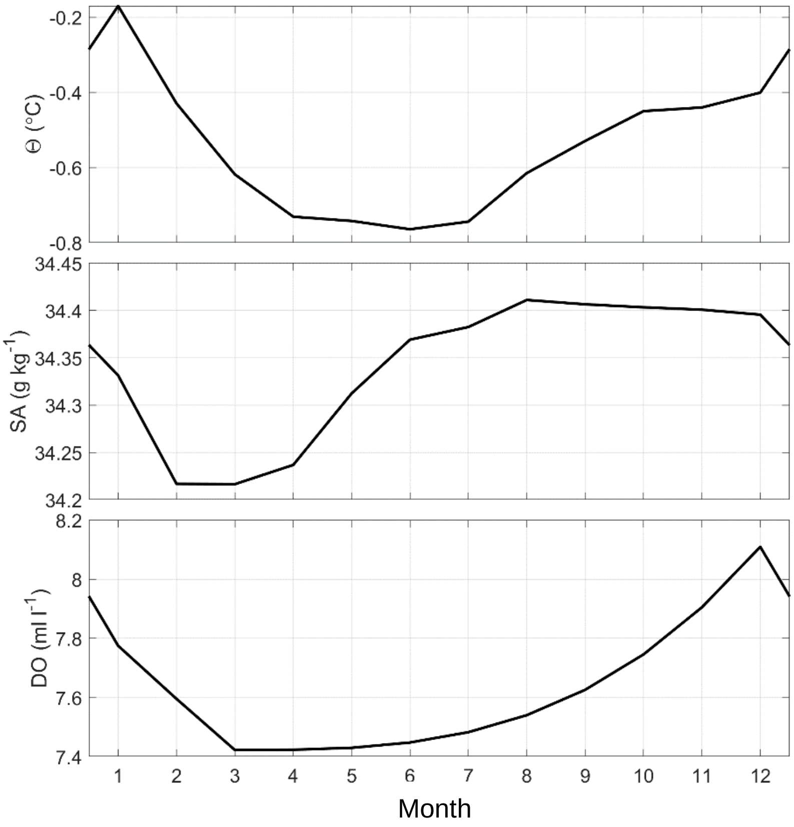

Fig. R1: Seasonal cycles of conservative temperature, absolute salinity, and dissolved oxygen derived from the data from Hattermann (2018) and the Seaglider sg564.

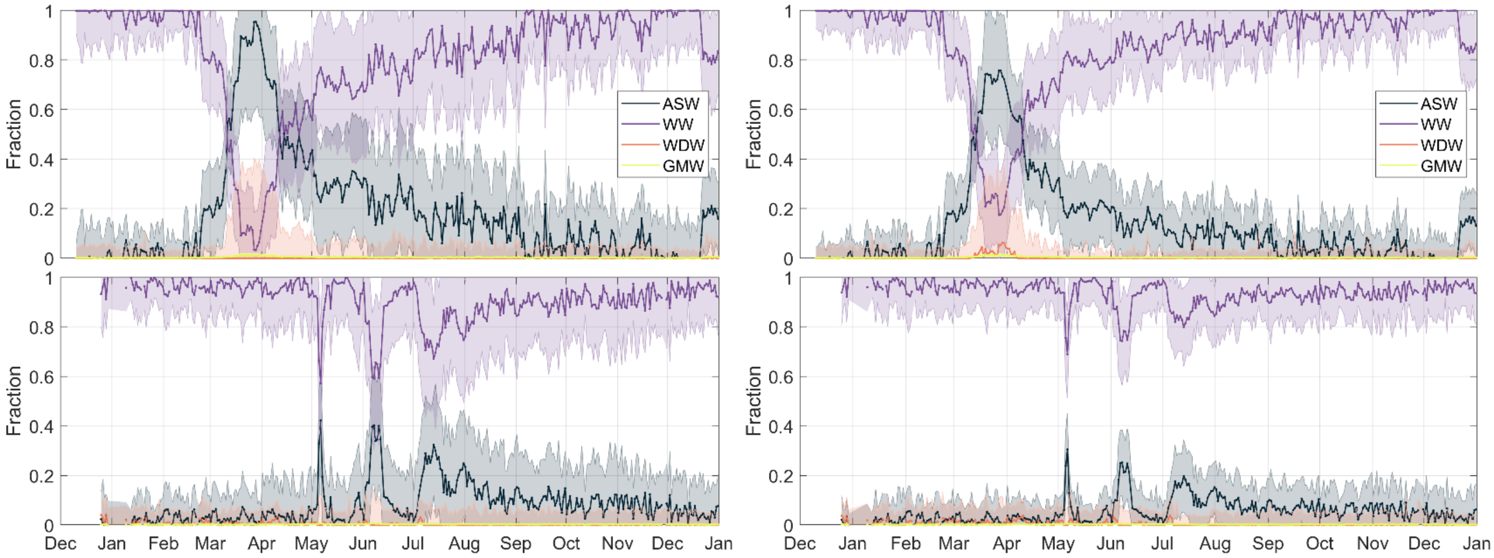

Fig. R2: Temporal evolution of water mass fractions during 2010, using a seasonal profile for ASW (left), and no seasonal profile for ASW (right).

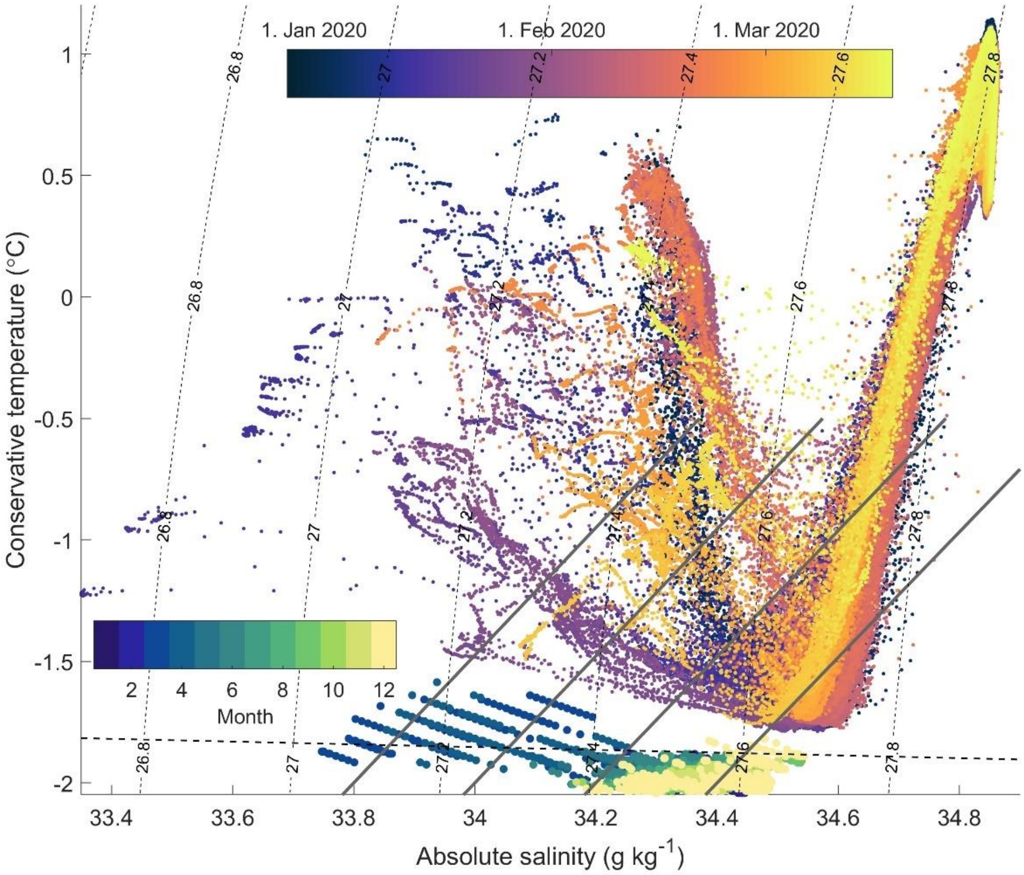

Fig. R3: Conservative temperature - absolute salinity diagram of $M1_{upper}$ (blue-green-beige colormap) and the Seaglider (sg564) presented in the manuscript (purple-orange-yellow colormap). The colors indicate the month from December 2009 and December 2010 for $M1_{upper}$, and the time from December 28th, 2020 to March 17th,2021 for the Seaglider. Grey lines are meltwater mixing lines. The thin dashed black lines show contours of the potential density anomaly, and the thick dashed black line shows the surface freezing temperature.

We acknowledge that it is a valid option to include a seasonal profile for the ASW properties. However, we believe that this method introduces some new issues:

- The seasonal changes in ASW, e.g. cooling and salinification of the ASW in autumn/winter, are related to the occurrence of WW. This collapse of ASW onto more homogeneous WW between March and July can be nicely seen in Fig. R4 (Nøst et al., 2011).
  That is, by defining the ASW properties with seasonally varying properties, we would already mix ASW and WW properties, and the separation between the end member properties would become less distinct.
- It is challenging to define a reliable seasonal cycle of ASW properties. ASW generally describes a class of water masses with a wide range of properties. Those vary not only seasonally, but also spatially (with distance from the coast, along the coast, and with depth) and interannually. Hence, even a seasonal approach for the ASW would be incomplete, facing the same problems as a single end member, with the addition of the unclear distinction with the WW.

- Seaglider sg564 data are only available from December to March, requiring interpolation in between. These data are widely spread even within a single month (Fig. R3). The data from Hattermann (2018) are generally applicable to the Fimbulisen region, but the hydrographic seasonal cycle was also shown to have a temporal offset of 1-2 months compared to 6 °E (Lauber et al., 2024).
- The seasonal cycle defined for the ASW properties is valid for the coastal open ocean. Due to downwelling at the ice front and advection to the mooring sites, this seasonal cycle is likely delayed in the cavity. This delay is not known and would have to be estimated, introducing additional uncertainties.

Due to the reasons given above, we prefer to keep the “non-seasonal” definition of ASW. This definition does not account for varying ASW properties, but one can identify when other water masses than those spanning the WW-WDW-GMW space contribute to the composition of the cavity water masses – this is the main intention of our approach, and we will highlight this intention in a revised version of the manuscript. We will introduce ASW and WW more clearly as end members of surface water that transition into each other and the properties of which seasonally vary due to air temperature and sea ice formation/melting. We will make sure to interpret the results of the OMP analysis in accordance with this definition of the water mass end members.

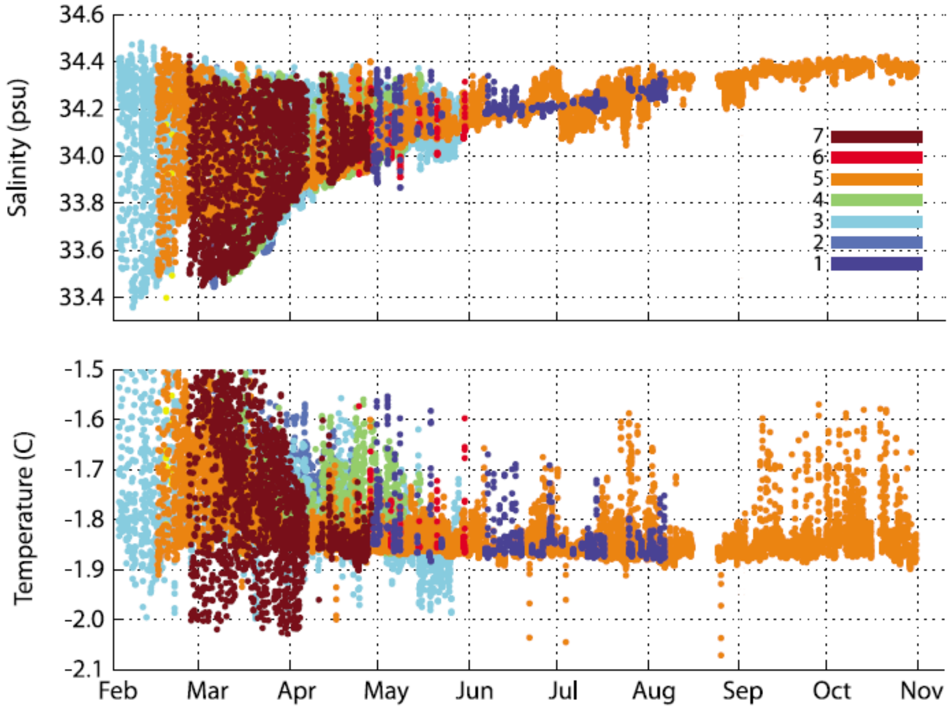

Fig. R4: Salinity and temperatures from each data point within 100 km of the ice front/coast and above the thermocline plotted against time. The individual seals are represented with colors. The figure and caption were taken from Nøst et al. (2011).

Additional remark.
Identifying the participation of WDW and ASC in basal melting is useful, but much more useful would be an attempt to quantify the contributions of WW, WDW and ASW. Or at least place bounds on the contributions. It's likely that a model would be required to extrapolate from the mooring data, but it might be possible to make progress shy of a model."

Below, we have copied our response to Anonymous Referee #1:
Thank you for this suggestion. It would be indeed more interesting to quantify the contributions of the water masses for basal melting.
We attempted a back-of-the-envelope calculation, using the satellite-derived basal melt rate data (mean state) from Adusumilli et al. (2020) and ice draft data from Morlighem et al. (2020). We calculated the mean basal melt rate for pre-defined depth bins in intervals of 100m and weighed these melt rates with the respective area over which the depth bins occur, giving the mean mass loss of Fimbulisen depending on the ice draft. We then interpolated our derived mean water mass concentrations at the instrument depths on the same depth bins. Finally, we multiplied the mass loss, water mass concentrations, and depth-dependent thermal forcing for each water mass, giving relative contributions for basal melting for all water masses. The result for ASW and WDW is shown in Fig. R5: based on these results, ASW contributes to more basal mass loss at Fimbulisen than WDW down to 500m ice draft. Below, WDW dominates. Note that the deepest instrument ($M2_{lower}$) is located at 681m depth, restricting our estimate to this depth, but that the contribution of WDW is expected to strongly increase toward larger depths.
Following the above-described calculations, the contribution of WW to basal mass loss for the shown depth range is one to three orders of magnitude larger than the contributions of ASW and WDW. This is due to the high concentration of WW at all instruments, although the thermal forcing is smaller for WW than for ASW and WDW.

These estimates inherit many simplifications, e.g. assuming that the water mass concentrations at the instruments are valid for similar depths below the whole ice shelf. We will still look for possibilities to refine our method and will include the estimates in the revised manuscript.

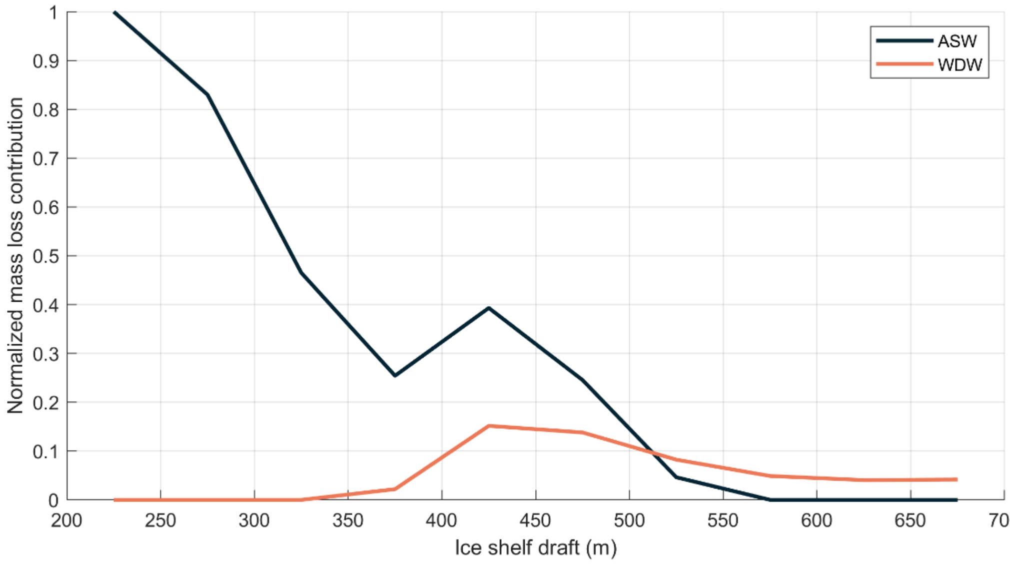

Fig. R5: Normalized mass loss contribution for ASW and WW depending on the ice shelf draft.

MY GENERAL COMMENTS

1) Most of the "supplemental" figures are just as important to the flow of the paper as the "main" figures. I would prefer that they just get cited and appear in main text in the right place.

We agree that it can be helpful for the reader to have some of the figures from the Appendix directly accessible in the main text. We will therefore move Fig. A2 and B2 to the main text. Fig. C1 contains some redundant information (e.g. the same velocity arrows shown already in the hodographs in Fig. 9 before). We will remove this redundancy and add the rest of Fig. C1 to Fig. 9 in the main text.

2) There is a lot of Introductory material in Results (section 4); e.g., the discussions about other data sets that explain what is already known about mWDW/WDW upwelling across sills, and the annual cycle of mWDW/WDW temperatures on the continental shelf. IMO, a better way to write the paper would be so tell us everything that is important, that is already known, setting up the rest of the paper to demonstrate the consequences of those external processes on what happens in the cavity, and surprises you find.
The problem the present format causes is that it is hard to tell what was previously known from prior studies, with what the new manuscript adds. However, like I said, the paper is well-written, so unless you see an easy way to make this structural change, I wouldn't recommend it.

We find it important to mention links to previous work during the presentation of our analyses in the Results section. However, we agree that the relevant studies can be already better introduced in the Introduction section, and we will do so in a revised version of the manuscript.

3) {really minor} I prefer citing as “BY {citation}” rather than “IN {citation}” wherever possible, to create the authors more for their past work.

We will change this as suggested.

4) I don’t really like the term “velocity shear” for what is really a “magnitude of the velocity difference”. Since the last expression is long, you could introduce a symbol and use that everywhere.

We agree that the term “velocity shear” might be misleading. As suggested, we will introduce a symbol instead.

5) it is important that text and figures are consistent. Three examples: (a) you talk about “percent” of source water masses from OMP analyses, but figures are labeled with “Fraction”; (2) Fig. 7 has “Speed difference” on the y-axes, but the text always refers to “velocity shear”; (c) places in the annual cycle are always referred to by month name, but most time series of annual cycles use {1, 2, … 12}.

We will make the suggested changes to improve the consistency between text and figures.

6) I mentioned three Ross Sea papers that are relevant to your comments on ASW: Porter et al. (2019), Tinto et al. (2019) and Stewart et al. (2019). You don't need to cite these, but they might be worth looking at.

Thank you for suggesting these papers. We have looked into them and will keep them in mind when analyzing ASW in our data.

7) I'm not sure of the plan for the three Appendices: Will they be published in the main paper, or will there be Supplementary Online Material? I think the SOM approach is cleaner; however, it's a journal and editor issue. However, the flow of the paper sometimes relies on the reader jumping to an "Appendix" figure, and it'd be easier if anything that needed to be read sequentially was in Main Text figures. It's okay to hide away things that aren't needed to understand the science (provided the reader trusts the authors!), but important science content should not be in non-main-text figures.

According to our knowledge, the Appendices will be published in the main paper. In our reply to General Comment 1) we have specified which figures we are planning to move to the main text.

MY MAJOR COMMENTS

1) You need to be *very* clear, at all times, whether you are referring to the presence of a water mass, or the presence of a contribution from a water mass. e.g., no sub-ice-shelf moorings show WDW, so it is wrong to talk about WDW being present. But the OMP analysis finds a fraction of WDW, and so maybe mWDW intrudes *after* production involving WDW (and WW) offshore. This might take a while: every sentence involving a water mass name needs to be checked.

We will revisit these formulations and improve the accuracy when talking about the water masses.

2) The introductory "map" figure needs to be improved. There are too many features that are discussed which are not included in Fig. 1a. In addition, I have recommended adding a new first section to Data and Methods, "2.1 geometry", that describes (briefly) where sub-ice-shelf bathymetry and ice draft come from, and shows bathymetry, ice draft, and water column thickness separately.

We will better introduce the ice shelf geometry in a short new subsection, as suggested.

3) You should probably define the T, S and DO ranges for mWDW. Often, I think you are referring to presence of mWDW but you describe it as WDW (which it is too cold for). mWDW is useful to define since it is a "water mass" that is found in the cavity, even though its heat content is all from the WDW source water mass.

mWDW is not a source water mass, but a mixing product of two water masses that are already defined in the OMP (i.e., WW and WDW, and mWDW can be any mixing product of these two). Therefore, we prefer not to introduce mWDW as an additional water mass end member. However, in accordance with Major Comment 1), we will pay attention when writing about water mass contributions and use mWDW where applicable.

4) Sometimes you claim that the expected inflow path is following water column thickness (wct), sometimes following bathymetry. You note that this is seasonal (depending on baroclinicity), but any statement about expected flow path should, therefore, specify what season you're talking about. Or, possibly, in this region the bathymetry and wct are closely aligned, so it doesn't matter? But, especially if that's the case, an extra figure devoted to geometry (bathy, draft and wct) is needed.

Good point, we will finetune our wording regarding the flow along bathymetry or wct.

5) As an example of mixing water masses with fractions of source water types, on line 287 you state "and cold/oxygen-rich WW, which is the most abundant water mass at all sub-ice-shelf instruments." But … this is not true, right? For all upper sub-ice-shelf instruments, ISW dominates. WW is probably the dominant "source water" mass (as ISW is mostly a lot of WW and a little GMW), but it isn't the dominant water mass.

Right, we should have added "source" to "water mass" in that sentence for clarification. One might see ISW as a separate water mass, but it is the mixture of GMW with ASW, WW, or WDW, and therefore not a source water mass and not included as end member in the OMP analysis. We will add a sentence on that in that section for clarification.