The Cryosphere Discussions

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Reply on RC2

Ryan Schubert et al.

Author comment on "The Antarctic Coastal Current in the Bellingshausen Sea" by Ryan Schubert et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-43-AC2, 2021

General Comments

Schubert and collaborators use existing hydrographic data from the Bellingshausen Sea continental shelf to characterize the Antarctic Coastal Current (AACC). The paper is clearly laid out and has significant new results of the horizontal and vertical scales of the AACC, its pathways on the shelf, as well as the evolution of its along-shore transport. This a very nice contribution well within the scope of The Cryosphere.

In the section below, I detail some concerns about the manuscript in its present form that should be addressed. I'm fairly confident that addressing them won't change the main thrust of the results, but nevertheless they require a few significant changes, thus amounting to moderate revisions (this isn't an option in the review form so I'm choosing major revisions).

- We thank the reviewer for their time in reviewing this manuscript. We are grateful for the positive comments on the manuscripts as well as the numerous thoughtful and constructive suggestions below. We agree with almost all of these and we feel that adopting these changes in our revised manuscript will strengthen the paper. We address each comment in detail below.

Specific Comments

1.) My main concern is with the framework used to present the freshwater budget of the shelf, and the meltwater calculations in section 2.4. The authors start that section by stating that the shelf is occupied by "CDW, Winter Water, and glacial meltwater". This framing oversimplifies the sources of freshwater that impact the hydrographic structure of this region. Precipitation, runoff from land, sea-ice melt, and glacial melt are sources with distinct tracer signatures and spatial and temporal scales of variability. The authors recognize these other sources briefly elsewhere (e.g. section 330) but their quantitative analysis only includes glacial meltwater.

- We agree that our discussion of water masses over the continental shelf was rather limited. This was a conscious choice as the focus of the paper was on the physical and dynamical properties of the AACC and also because we have recently reported on Bellingshausen shelf properties in two manuscripts, Ruan *et al.* (2021) and Schulze Chretien *et al.* (2021). However, we agree that a more detailed description of the water masses would help this paper to stand alone. Therefore, we have added some additional text in Section 3 that provides a more complete discussion of the freshwater sources. However, because our analysis is focused away from the surface layer and we only have measurements of temperature and salinity, we have focused our interpretation on contributions from glacial meltwater, which will dominate freshwater changes in the CDW and what we refer to as the transitional layer.

As a result, the method outlined in 2.4 fails to represent the above complexity. The references that the authors cite (e.g. Jenkins and Jacobs; Biddle et al) are sound, but it should be noted that those authors had additional tracers other than temperature and salinity at hand, and/or where able to focus on meltwater only because they were looking at data very close to the ice, at depth, where they showed specific signatures of meltwater in T/S diagrams. Neither is the case here. Temperature and salinity on their own cannot be used to obtain a meltwater fraction or index over a broad region of the shelf, and particularly near the surface, where other sources of freshwater are very likely to heavily influence, or even dominate, the freshwater budget. The authors recognize there's an issue in section 2.4, as they obtain negative meltwater fractions in much of the shelf. This is readily explained because their mixing model lacks all the necessary end members. Including them would require additional tracers.

- We agree that the Jenkins and Jacobs paper has a better framework for calculating meltwater with its additional tracers and vicinity to the ice shelf. However, as we state in the paper, the main goal of our meltwater calculations is to qualitatively look at how meltwater fraction evolves along the AACC. We are not concerned with absolute values or establishing a quantitative value for meltwater fraction in the AACC, rather just an idea of whether it increases from east to west.
- Our calculations of meltwater fraction was also highlighted by the other reviewer as a potential issue, therefore we have chosen to remove this analysis from the manuscript. The main use of the meltwater index in the previous version was to define the lateral extent of the AACC. We will revise the AACC's definition to use either a salinity threshold at a fixed depth, the point at which net transport flattens out, or where the dynamic height gradient is smaller than some value. The goal of revising this definition is to better identify the AACC's offshore extent so that it is more consistent with the reduction in the geostrophic transport. Note however, that our results on changes in the AACC's transport in the along-coast direction does not change, since the transport is largely confined to the coast, regardless of our detection criteria. The figures and text have been updated to address this change.

Please note that the different components of the freshwater budget have been studied fairly extensively in the West Antarctic Peninsula using T, S, oxygen isotopes, and numerical models. Those papers (I suggest some of them should be referenced) show that freshwater sources other than glacial meltwater are also significant in this region:

Meredith, M. P., Brandon, M. A., Wallace, M. I., Clarke, A., Leng, M. J., Renfrew, I. A., et al. (2008). Variability in the freshwater balance of northern Marguerite Bay, Antarctic Peninsula: Results from δ18O. Deep Sea Research Part II: Topical Studies in Oceanography, 55(3), 309–322. https://doi.org/10.1016/j.dsr2.2007.11.005

Meredith, M. P., Venables, H. J., Clarke, A., Ducklow, H. W., Erickson, M., Leng, M. J., et al. (2013). The Freshwater System West of the Antarctic Peninsula: Spatial and Temporal Changes. Journal of Climate, 26(5), 1669–1684. https://doi.org/10.1175/JCLI-D-12-00246.1

Meredith, M. P., Stammerjohn, S. E., Venables, H. J., Ducklow, H. W., Martinson, D. G., Iannuzzi, R. A., et al. (2017). Changing distributions of sea ice melt and meteoric water

west of the Antarctic Peninsula. Deep Sea Research Part II: Topical Studies in Oceanography, 139, 40–57. https://doi.org/10.1016/j.dsr2.2016.04.019

van Wessem, J. M., Meredith, M. P., Reijmer, C. H., Van den Broeke, M. R., & Cook, A. J. (2017). Characteristics of the modelled meteoric freshwater budget of the western Antarctic Peninsula. Deep Sea Research Part II: Topical Studies in Oceanography, 139, 31–39. https://doi.org/10.1016/j.dsr2.2016.11.001

- Thank you for these suggestions, we will indeed incorporate some of these citations in our revised discussion of the freshwater budget.

Fortunately, the authors use the results from their meltwater index calculations in a limited way: to delineate the cross-shore structure of the front and thus determine the limit of the integration for the transport calculations. They also present the meltwater index in the supplementary materials (Figure A7). Given the above issues, I believe section 2.4 and the associated meltwater index results should be removed, and the transports and other AACC properties (e.g. mean velocity) recalculated with an alternative definition of its offshore extent.

- We agree with the reviewer and have made this change as discussed in our reply above.

Aside from the issues with the meltwater calculations themselves, the resulting delineation of the AACC (Figures 6 and A6) is problematic. This is because most of the AACC frontal hydrographic structure is found away from the surface, but the meltwater index emphasizes the surface properties. For example, in Section 2 it seems to significantly underestimate the transport as the offshore limit cuts across the core of the current. In Sections 4, 5, and 6-7, they result in what I think is an AACC that is far too wide. Note that the tilting subsurface isopycnals and associated transport are well shoreward of that estimated offshore boundary. Instead, I suggest the authors use one of a number of more straightforward criteria for the offshore limit of the AACC: when the transport 'flattens out' as you integrate offshore, when the deep isopycnal slope drops below some level, or when the dynamic height gradient in the cross-shore direction is smaller than some value. Checking which one of the above more robustly captures the transport and the offshore limit should be straightforward, and has the advantage of publishing estimates that are readily comparable with other studies, instead of depending on the problematic meltwater index.

- We agree that using our meltwater index as the definition of the extent of the AACC was not optimal. We appreciate the reviewers encouraging us to reassess this definition. We feel that a more straightforward approach would be both more consistent with our geostrophic velocity and transport estimates and easier for readers to follow. Again, we emphasize that the transport results do not change significantly since the geostrophic velocities are confined to a narrow region near the coast, regardless of our AACC definition.

2) Note that recalculating the offshore limit might help with some of the discussion of the evolution of the average velocity of the front in Section 4, because the overly wide estimate of the AACC will result in an average velocity that is likely too low. Note also that the authors should be careful when discussing the average velocity of the AACC. A buoyant current like the AACC will tend, in the absence of wind or other forcing, to be "attached" to a particular isobath, whose value depends on the density difference of the plume, and its along-shore transport. If the bathymetry becomes shallower (e.g. section 4) the current will move offshore to find that isobath, and therefore create a nearshore region with weak velocities and an offshore region with strong currents (this is evident in sections 4 and 6). The "average" velocity of the plume becomes rather meaningless in

that context, while the average velocity over the frontal region remains meaningful. See this and related papers for details:

Lentz, S. J., & Helfrich, K. R. (2002). Buoyant gravity currents along a sloping bottom in a rotating fluid. Journal of Fluid Mechanics, 464, 251–278. https://doi.org/10.1017/S0022112002008868

- Thank you for this suggestion -- this will be a nice addition to the paper. We agree that although the AACC does have a baroclinic structure due to the lateral buoyancy gradients, due to the weak stratification over the continental shelf, there may be a barotropic component that is strongly influenced by bathymetric contours. Our new definition of the offshore extent of the AACC has led to a revision of the average velocities of the AACC, but we still focus our discussion of the geostrophic transport which avoids issues of the AACC becoming wider or narrower. We now include a brief discussion of the impact of topographic steering and include a citation to Lentz and Helfrich (2002).

3) The paper would benefit from a table where the properties (width, depth scale, property gradients, along-shore transport) are summarized for each section. Here, it would be good to calculate the Rossby radius of deformation for this case (see Lentz and Helfrich for details) and compare it with observations.

- This is a nice suggestion. We have added an additional table to the paper that includes this information.

4) I was surprised there was no effort to understand the seasonality of the AACC, or what impact using unevenly distributed data (i.e. by season) would have on the averages. While it looks like in most of the western sections the data is heavily tilted towards the winter, some of the eastern sections seem to have more evenly distributed data. While the results seem robust, it should be noted that if the AACC is indeed heavily seasonal as discussed in the introduction, it is possible that the averaging of mostly winter data with some summer data in the west and more evenly distributed data in the east would result in an underestimation of the rate of along-shore increase of the transport. The authors should attempt to construct winter vs summer/fall sections at least in a few places and report their findings.

- We did some preliminary work on the seasonality of the data, which is included in Figures 2, A2, A3, and A4. These figures help to identify geographically where data is in the different seasons and what the properties look like, however it would require a fair amount of work to do a proper and thorough assessment of the seasonality and therefore is beyond the scope of this paper.

5) The discussion & use of previous results could be clarified. In the introduction, the authors imply that previous observational results showed the AACC is buoyancy driven (Moffat el al) but that models show it to be wind-driven (Holland et al). There's really no contradiction here because Moffat et al do discuss wind effects, showing there are likely significant, and Holland et al state clearly (page 8) that their model did not have runoff and only a small contribution of meltwater. This should be clarified.

- We agree that this should be clarified and we have modified the text accordingly.

Elsewhere in the results, and critically in Figure 9, the authors use their relative geostrophic velocities referenced to 200 m and state (section 145) that "In Sect. 4, we also present the geostrophic transport referenced to 200 m for comparison with the velocity structure in Moffat et al. (2008), who used a level of no motion around 200 m based on their LADCP velocities." This seems incorrect, as Moffat et al. used shipboard

ADCP integrated above a variable-depth isohaline, so there was no level of known motion.

- We agree that this should instead reflect that they found a zero crossing based on ADCP data close to the 200m depth and we have updated this in the text.

Finally, it is worth citing:

Beardsley, R. C., Limeburner, R., & Owens, W. B. (2004). Drifter measurements of surface currents near Marguerite Bay on the western Antarctic Peninsula shelf during austral summer and fall, 2001 and 2002. Deep Sea Research Part II: Topical Studies in Oceanography, 51(17–19), 1947–1964. https://doi.org/10.1016/j.dsr2.2004.07.031

as another early paper that recognized the presence of the AACC along the WAP.

- We have included a citation to this paper in the Introduction.

6) In Figure 3 and the associated text, the 27.4 isopycnal is used to characterize the isopycnal tilting associated with the AACC, which seems very reasonable. But "Winter Water" is defined by a temperature minimum during the non-winter months, and Figure 4 seems to show plenty of data where no temperature minimum exists, and when it does, it is distributed over a range of densities. Would the map look different if you were to show the depth, magnitude, etc., of the temperature minimum instead? What if there's no temperature minimum? Again, I think using that isopycnal is fine, but calling it "Winter Water" doesn't seem correct, particularly for data during the winter, or regions where there's no winter water.

- This is a good point and we agree that our naming of the layers could have been more nuanced. We prefer to show our property distributions on density surfaces in this section. We have opted to include some text at the start of the section that states clearly that end-member water masses may occur at slightly different density values (and therefore on slightly different layers) over the span of the Bellingshausen Sea. Overall, though, this does not change the key points of the paper.

7) In section 120, the explanation of the gridding procedure is very clear. The bin size (section 2.2/125) seems to have been chosen (reasonably!) to balance horizontal completeness and resolution. However, this doesn't necessarily guarantee that the resulting horizontal resolution (Table 1, 6.7-28 km) resolves the frontal structure of the AACC. In your estimation, is the bin size you've chosen enough to resolve the frontal scale of the AACC? How would we know?

- Absolutely this is an important point and something we grappled with when choosing our gridding procedure. The short answer is that if the AACC is narrower than our grids, we would not be able to see structure on those scales. The front would look broader than it actually is. We did some sensitivity analysis with the bin size and it did not change our results significantly. Critically, though, the geostrophic transport is not terribly sensitive to our choice of bin size since it only depends on the total lateral density gradient between the coast and our definition of the AACC's offshore extent. We acknowledge that we could miss contributions to the AACC transport that occur very close to the coast if we are missing observations in this region. However, this contribution is likely to be small. We have decided to keep the original gridding method, but we have added a couple of extra sentences to discuss the implications of our bin-size choices, in particular acknowledging that the AACC has previously been observed to be a narrow feature, ~20 km. We will also test the sensitivity of the AACC to the resolution and report our findings either in the text or with an additional figure.

Technical/Minor Corrections

Section 15: do you want to mention the absence of the ASC here as well?

- We believe that in this sentence, the ASC is not critical to mention and therefore have not added it.

20: worth citing: The IMBIE team. (2018). Mass balance of the Antarctic Ice Sheet from 1992 to 2017. Nature, 558(7709), 219–222. https://doi.org/10.1038/s41586-018-0179-y here.

- Thank you for this suggestion, we will add this citation.

Figure 1: There seems to be plenty room to spell out the names in the figure itself.

- There is room to spell out the names of the features, but we chose to abbreviate them so that the bathymetry is more easily viewable and isn't cluttered by words.

105: Do all the profiles in Figure 2 have salinity as well as temperature data? If not, can you state the difference in coverage?

- Yes, they all have salinity and temperature.

135: Can you report the resolution in km?

- We choose to keep the discussion in degrees here because the distance in km is slightly different in each section. However, we now explicitly state this in the text and refer the reader to Table 1 where distances in km are provided.

190: References for this text?

- We have revised this section and included references to Jenkins and Jacobs (2008) as well as Whitworth et. al. (1998).

205: "destroying the surface layer" should probably be replaced with "resulting in mixing and deepening of the mixed layer."

- We have made this change to the text.

230: I understand you're trying to navigate using the old APCC name from Moffat et al while trying to use AACC, but this is confusing. I think it would be better to commit to AACC early in the paper by saying something like "Moffat et al called this the APCC, but we will use AACC throughout this manuscript for clarity."

- We agree. At the first introduction to the Antarctic Peninsula Coastal Current, we now acknowledge that this current extends beyond the WAP and we will refer to it at the Antarctic Coastal Current (AACC). We have removed the acronym APCC.

235: I think you mean the opposite? strong in Summer and weak in Winter?

- This is correct, we do mean to switch this as the current they describe is strong in the summer and weak in the winter.

240: I didn't understand the purpose of the discussion of the Transitional Layer. Doesn't seem to add much to the discussions of the upper and CDW layers.

- The transitional layer is included because it roughly aligns with the pycnocline as well as the base of the AACC, making it important to understand how it evolves along the coast. We have added a few sentences to reflect this in the text.

270: If their mean/median calculations are similar, why go with median? Most of the other published (and likely, to be published) data/models will use means, creating confusion.

- We ultimately chose to use the median value because it is less sensitive to outliers. This is typical practice when binning CTD data, for instance.

285: Variations repeat here "There is greater variability in surface properties variations to the east over the WAP shelf."

- This should just be simply "there is greater variability in surface properties to the east over the WAP shelf." We have made this change in the text.

300: May explain? This seems like something that can be determined from the data.

- We will look into this and either report it in the text with a few sentences or add a figure to discuss.

330: The buoyancy frequency quantifies the density stratification, so this seems repetitive.

- We have reworded the text here to remove this redundancy.

350: Why doesn't Fig 8 include the northern-most sections?

- We are mostly concerned with the Bellingshausen Sea and the northernmost sections were included to compare to the Moffat sections.

365: If you are assuming a barotropic velocity (even if it's zero) you're assuming a level of "known motion", not "no motion."

- This phrasing was unclear, we have now removed "(or barotropic velocity)," because we are specifically choosing a level of no motion, rather than a barotropic velocity.

405: Could this be influenced, in the summer, by along-shore gradients in sea ice melt concentrations? It's important to describe all the contributions to the freshwater budget here.

- This is a good suggestion. We now acknowledge the potential that variations sea-ice melt may also impact the spatial distribution of properties. As mentioned above, we have also enhanced our discussion of freshwater sources in this region. However, it is beyond the scope of this paper to quantify the impact of sea-ice melt on the freshwater budget.

Figure 9: why not fit a line to this, and extract a rate from that? Seems more informative than to assume a 2 Sv/1000km rate.

- We agree and have now included a linear fit to the observations to quantify the strengthening of the AACC.

Figures 10: Typically we label the deployment location of drifters with a fat dot or a cross to make clear the start/end locations.

- We would gladly add these so that it is clearer where it was deployed and where it ended.