The Cryosphere Discussions

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Comment on tc-2021-43

Anonymous Referee #2

Referee 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-RC2, 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).

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.

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.

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

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.

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.

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

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.

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 seems 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.

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.

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.

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.

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.

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?

Technical/Minor Corrections

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

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.

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

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

135: Can you report the resolution in km?

190: References for this text?

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

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."

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

270: If they 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.

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

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

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

350: Why doesn't Fig 8 include the northern-most 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."

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.

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

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