Ocean Science Discussions

Ocean Sci. Discuss., referee comment RC2
https://doi.org/10.5194/os-2022-17-RC2, 2022

Comment on os-2022-17

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Referee comment on "Import of Atlantic Water and sea ice controls the ocean environment in the northern Barents Sea" by Øyvind Lundesgaard et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2022-17-RC2, 2022

This study presents a detaile descriptive analysis of mooring data from the northern Barents Sea in order to identify the variability of Atlantic Water import from the shelf break region to the morth. To achieve this, it examines the variability of the currents, temperture and salinity at subtidal frequencies at the southern end of the Kvitøya Trough. Overall, there is little fault to be found in the careful processing, quality control and analysis of this data, and the implications of these observations are clearly explained and discussed. The part I found less satisfactory is the discussion about the atmopsheric forcing of the Atlantic Water pulses into the Barents Sea. I think that the authors can (easily) and should carry out a modest expansion of their analysis to eliminate some of the speculation. As such I think this probably straddles the line between a minor and major revision before I'd reccommend publication.

Revision recommendations:

Section 4.3; I was left unsatified by this discussion, it's still a bit too speculative for my liking and the actual analysis, i.e.correlation with the Kvitøya and Bjørnøya pressure difference is not sufficiently motivated at the outset. This choice needs to be better explained to the reader where it first comes up when you present the correlations. I also think you can't discount far-field forcing of variability at M1, and the region you've chosen to compute regressions (appendix) for is far too small to allow for topographically-trapped waves from the Yermak Plateau or Ekman pumping in the central Barents Sea that may yet impact circulation in the northen Barents Sea. So I reccommend you greatly expand the region to incorporate all of the Barents Sea and beyond. Alongside that, it would be good to know the propagation times of bottom-trapped waves and residence times/circulation timescales of Atlantic Water within these troughs and how it leaves the northern Barents Sea. Both figures from the Appendix are material to the discussion on the atmospheric forcing, so it would be great to have them actually used in the main text of the manuscript as well.

Minor points:

- The introduction is curious in its use of relatively recent literature to describe the circulation and background climate of the Barents Sea rather than the seminal studies reporting on these phenomenon. For instance, the literature cited on AW pathways is from 2007 and 2010 instead of the earlier papers by Schauer and Mauritzen etc... Seems a missed opportunity to acknowledge the contributions of our pioneering femal oceanographers who were greatly outnumbered at the time.
- Since this study has a focus on watermass, may I ask you to limit your use of acronyms to these? I don't think using nBS really enhances your work in anyway, as I have to actively remember this means a place rather than a watermass when I see it.
- One weakness of this study is that you don't and can't really distinguish between locally mixed and advected signals. So I'd suggest maybe just a bit more care in the way words like 'warming' and 'freshening' are used since you seem to sometimes think this is a result of local processes and sometimes advected signals. I.e. the warm salty pulses are associated with modified Atlantic Water - here you don't think the system is warming as a result of local processes. Whereas, there is clear seasonal forcing that would result in local cooling/warming freshening etc. So it would be great if you can be clear about the difference between when you think something is local or advected.
- I like the discussion on the increased freshwater content and impact on the sea ice of the following season. But it would be great if this can be more quantitatively assessed apart from the estimate of increased freshwater thickness. Does the sea ice freeze start earlier in 2019 for instance, and can you show that the increased freshwater near surface limits the convection depth pre-freezing so less heat needs to be lost etc? Or may just show the change in stratification using available potential energy, pv or potential energy anomaly or something?
- By the way, great job on the figures. I liked these.