RC2: REVIEWER 2

We thank the reviewer for taking time to provide helpful comments that will improve our manuscript. We have provided responses to these comments below in blue color.

This paper scrutinizes the observations made in 2008 and 2014 (especially in Herald Canyon) and attempts to explain the behavior of WW, BSW, nutrients, etc. using numerical models. The numerical model has been developed and improved vigorously by the authors, and I believe that it is one of the most reliable models applied to the Arctic Ocean. However, even though the model output is provided, it is no different from the description of the observation results, and the details of the mechanism are not mentioned. It is recommended that the paper be improved by examining the results of the numerical model more closely.

~Major points~

Line 227 (Westward shift in boundary between northward & southward flow) : Why did the westward shift of the boundary eventually occur, the mechanism would need to be explained since it affects the flow rate of WW. Line 271-272 says that it is strongly affected by wind stress because of forward pressure. The cross sections of the 9-km resolution model (Figs. 10 and 11) do not show a westward shift. Isn't it necessary to show the wind stress field (field and model) during the observation period? In Fig. 4, the WW seems to be constrained by the topography. What are the results of the 2-km resolution model?

In response to this comment and a comment by reviewer 1 we have changed Fig 13 to include panels that show the winds just before and during the two surveys. We have added the following to the observational part of the results section:

“Winds were southerly in the week before and including the 2014 survey (Fig. 13b) and may have enhanced the flow forced by the forward pressure coming from Bering Strait, while strong easterlies in 2008 (Fig. 13a) may have caused a build-up of water towards Wrangel Island that potentially induced stronger southward barotropic flow across section 3.“

Figs 6 & 7: Are you using the 2-km resolution model output only to explain the faster flow speed, the greater number of eddies in the ocean basin and more complex circulation north of 100m isobath? First of all, the authors should add the 100m isobath (there may be one, but I can't see it.). If the average velocity field or

cross-sectional view does not change the results much, then I think only 2-km is sufficient. "source from flows across Herald Shoal" can be said for 2008, can't it?

The differences, as well as similarities, between the 2km and 9km circulation are interesting to modelers and we would like to keep these figures for that reason.

Figs. 8 & 9: The model output of T, S, and velocity shows a fundamentally different structure from the observation: in 2008, the WW is unevenly distributed to the west in the observation, but not in the model output. The structure of the surface layer (up to 20 m depth) is also completely different in 2014. Why is the northward velocity distribution split into two in the model?

We plan to address this comment in the revision.

Discussion: The authors mention heat loss and residence time to explain the fresh WW in 2014. However, these explanations are only speculations at present. Since the model output is available, heat loss and residence time (and impact of brine rejection) can be calculated explicitly by tracer experiments. It should also be possible to study in detail the water mass properties and their sources of variation in upstream and downstream areas with the model output. My personal impression is that WW freshening cannot be explained by local phenomena alone.

We have added a reference to Woodgate and Peralta-Ferriz (2021) who show that the Bering Strait inflow freshened by around 0.3 between 2008 and 2014 explaining around half of the observed WW freshening. We have retained the variability of the winter circulation and sea ice formation that transform the Bering Strait inflow on the East Siberian shelf as an explanation for the remainder of the freshening signal in Herald Canyon. However, we have removed the heat flux discussion following a comment by Reviewer 1.

Since data assimilation is not applied, when explaining the reproducibility of the model, etc., the snapshot of the model output will naturally show some differences from reality. For example, how about using the Ensemble mean of the results from a year with a north wind and a year with a west wind to illustrate how much the velocity structure and WW flow rate changes with wind stress?

We can address the question with more model results in the revision.

~Minor points~

Figs 4. 5, 9 & 10: Please improve the diagram so that we can see the distances between the points.

We can change the horizontal scale from longitude to km in these figures for the revision.

Line 93: The observation period of SMMR is 1978-1987, so it must be SSMI.

Corrected.

Line 109: Isn't the frequency of RDI ADCP 300 "kHz"?

Corrected.