RC1: REVIEWER 1

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 presented the structures of the hydrography, velocity and nutrient concentrations in vicinity of the Herald Canyon using the data from two cruises. The similarities and discrepancies of these two cruises were also shown in the model. The model reveals the transports through the Herald Canyon have large variations daily and interannually. As the measurements are quite valuable, from the model I expected to learn more about the mechanisms of the circulation and how it distributes the water masses and biogeochemical properties, which I thought is also the main goal of the study. Unfortunately, the authors put more efforts on describing the model outputs, without diagnosing the mechanisms in detail. The paper mentioned several times that the wind forcing plays an important role in modulating the circulation. The southward-flowing water likely originates from the East Siberian Sea and might recirculates within the Herald Canyon. All of these are less convincing by just characterizing, while I think might be more enlightening if they take the further step of the model. In any case, before presenting the model results, I think the authors need to show the readers how robust the model is by comparing the outputs with the observations. I am not saying everything has to match with each other, but at least show the transports during the two cruises are somehow comparable.

Overall the paper is well-written and clear, I recommend a major revision.

We have addressed your comments by looking more closely at the wind field and providing additional analysis from the model output. This should provide more information on the mechanisms controlling the circulation and biogeochemical property distribution. As you mention below, we have already compared the transports and vertical sections of T, S, and velocity from the observations and model output in Figs. 4, 5, 8, 9, 10 and Tab. 2.

The specific comments and questions:

As the focus of the paper, the conditions of hydrography and circulation in the Chukchi Sea certainly deserve more descriptions in the introduction, instead of the briefly summary in one paragraph. It is also necessary to mention the particularity/importance of the western Chukchi Sea in the introduction.

We have expanded the introduction as you suggest.

Line 65. These water masses are Pacific Summer Water.

Corrected.

Line 67. It should be Bering Summer Water in Pisareva et al. (2015), while was named as Bering Sea Water by Coachman et al. (1975). You would need to adjust your references.

Corrected.

Line 75. Any reference?

Linders et al. 2017 reference added.

Line 165. The model configuration needs more details, i.e. what are the initial conditions and the forcing? How long did the model run?

We have added details to the model description, such as: The RASM historical (1979-2018) simulation results analyzed here were produced after a 78-year spinup, which started with no sea-ice and the Polar Science Center Hydrographic Climatology (PHC) 3.0 (Steele et al., 2001) climatological ocean temperature and salinity at rest and was forced with the Coordinated Ocean-sea ice Reference Experiments version 2 (CORE2) reanalysis (Large and Yeager, 2009).

Lines 181-183. Some of the water masses are not appreciate (or not seen in previous studies including the Linders et al. (2017) mentioned in the paper). Is the Winter Water actually the Pacific Winter Water? What about the River runoff? I am not sure what the Summer Water represents for, maybe Pacific Summer Water? But the authors already listed the two types of Pacific Summer Water, Bering Summer Water and Alaskan Coastal Water.

Thank you. We have corrected the water mass labels. We now are consistent with Linders et al. (2017), except that our Pacific Winter Water is a combination of their Remnant Pacific Winter Water and Newly Ventilated Pacific Winter Water.

Figure 3. I am curious why not show the nitrate. I thought biologists care more about nitrate.

The strongest signal in the upper halocline of the Canada Basin is in silicate, and thus it is the most obvious tracer. Any of the nutrients could be used, but nitrate is the least suitable as it is lost through denitrification in the upper sediment. As this figure only aims at showing which water masses have the highest nutrients, we choose to use the one with the strongest signal.

Lines 199-200. There must be a reason to make such a speculation. I understand that the authors want to focus on the BSW and WW, but showing all of the water masses in the Figs 4e, 4f may help interpret the water mass mixing that they pointed out.

We have added the surface water masses to panels e and f in Fig 4 and changed the sentence to the following:

“With salinities of 30-32 some of the shallower water with BSW characteristics in the western part of the canyon (Fig. 4) in both years lies on the mixing line between WW and MWR in 2008 and WW and SWC in 2014 (Fig. 3). Being distinctly fresher than the BSW modes that Linders et al. (2017) identify as having a Bering Strait origin and is probably of local origin.”

Lines 223-224. This sentence is confusing. Did you mean Fig. 5b?

We have changed the figure reference to Fig. 2.

Line 272. The authors said that the wind is important, but never compared the wind condition between the two cruises.

To address this comment and your comment regarding the discussion of the atmospheric and sea ice variability, we have changed Fig. 13 to include panels that show the surface winds for the week before and including the surveys.
We have added the following to 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. ‘

Figure 6. Whether the Beaufort Shelfbreak Jet was not well resolved or was just not shown in the subsampled plot. The authors did the comparison of 9km- and 2 km-

res model outputs, but never explained why they eventually used the 9km-res data for the following analysis.

Added to section 3.3: We use the 9km model output because it is available on a daily timescale (instead of just monthly means in the 2km output), which is better for comparing with the observational estimates.

Figure 8. I did not expect to see a good agreement of T/S/U sections between observations and model, but it will be more convincing if you can at least compare the mean transport during each of the occupations.

The mean model transport (and variability) during each occupation is stated in section 3.3 for comparison with the observations.

Line 331. I think the authors cannot make such a statement by comparing the 2-month mean transports in Fig. 8 with the snapshot observations. See my suggestion above.

We have made the comparison more clear and precise in the revision.

Line 346. I see now they present the transports from the model for each occupation, which I think needs to be shown above as a model robustness check before getting into the details.

We have added this to the beginning of section 3.3:

The observed total transport across section 3 was -0.279 Sv during the September 2008 occupation and was -0.385 Sv in the model results. In 2014 the observed total transport was 0.240 Sv during the August occupation, while the model results showed a value of -0.030 Sv.

Lines 355-360. I believe the BSW transport peaks in fall, but it is hardly seen in the Figure 11. I am confused why they discussed the seasonality based on the interannual variation in Figure 11. There are certainly more to discuss in terms of the interannual variation, i.e. any increasing trend in transport as the Bering Strait inflow (Woodgate, 2018).

We have added more text on Bering Strait in the Discussion section.

Lines 381-382. This sentence is not obvious to me. Does it mean that the comparable DIC concentrations are due to the similar salinity?

The text has been expanded to make our arguments clearer.

Line 395. Fig 11cd?

The reference to 11cd is likely a remnant from an earlier manuscript version. The sentence and reference were removed as part of the revision process.

In the discussion section, how do you define the net ocean surface heat flux? Is it the air-sea heat flux? As shown in Fig.13, the ice covers in the winter, how did you (or model) deal with the ice for the heat flux calculation? If it is the air-sea heat flux, it supposes to be zero in the region where was covered by ice. It seems not the case in this paper (Fig. 13). Did you consider any effect of the advection? The WW may not be locally formed, but be advected from the upstream. The authors argued that the origination and fate of the southward-flow water in the Herald Strait with the limited observations. Why not look at the model output which may provide some evidences? For example, tracking a tracer in the model.

We have removed the heat fluxed from the discussion and figure. You are right in saying that heat flux products in sea ice covered areas are difficult to use and interpret. What we were showing was strictly a "net surface heat flux" not a "net ocean surface heat flux".

We have integrated a very helpful comment from Rebecca Woodgate into this discussion. Her Bering Strait mooring record show that around half of the WW freshening that we observe between 2008 and 2014 does indeed come from upstream.

However, this implies that half of the freshening needs a different explanation and the change in wind pattern between 2007/08 and 2013/14 together with the Pickart et al. (2010) mechanism offers this. We have edited Fig. 13 to show the change in the winds more clearly and intuitively.

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 25, 50, 100, and 1000m isobaths are labeled blue lines. 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 believe the model velocity distribution has 2 cores at times due to the local bathymetry distribution. This is similar to the observed sections 2 and 4, which are upstream and downstream of the main section 3.

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 have examined the annual mean wind field (from the years 1980-2017) in the model results, but cannot find a clear relationship with the flow through Herald Canyon at this timescale. Shorter timescales appear to be more important, therefore, we are presenting results on the wind velocity at synoptic timescales in Fig. 13 and have added text to the Discussion.

~Minor points~

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

We have changed 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.

CC1: Rebecca Woodgate

Very interested to see this. A few quick questions:

1) You report that cold WW in Herald Canyon is fresher in 2014 than 2008 and discuss (lines ~ 400) that that may be due to changes in advection and brine release. However. I wonder how much of that freshening can be related to the freshening in the Bering Strait, Woodgate and Peralfa-Ferriz, 2021. See, e.g., Figures 5 and 6a of Woodgate, 2018, and data available at psc.apl.washington.edu/BeringStrait.html (monthly mean salinities for example) where you can clearly see 2014 is mostly fresher than 2008. Do you see this freshening in the Bering Strait in your model? And can you quantify how that affects the salinity in Herald Canyon?

Thank you for bringing this article to our attention. Your Bering Strait results show that there is a freshening of about 0.3 between 2008 and 2014 from upstream. This is about half of the WW freshening we observe in Herald Canyon. We have updated the paragraph to add this. However, we have retained our line of argument that changes in circulation and brine release the previous winter on the East Siberian shelf likely also contribute. We have edited the relevant figure to show that 2008 follows the wind pattern identified by Pickart et al. (2010) with strong northeasterlies in late autumn and early winter and a weakening in spring. The 2013/14 winds are very weak both in winter and spring and will not have helped to constrain waters to the shelf west of Herald Canyon.

We do see a freshening in Bering Strait in the model results. The annual mean freshwater flux was 47.9 mSv in 2008 and 56.4 mSv in 2014.

2) Line 470 .. "a substantial fraction of PW ... continues to the ESS via Long Strait". It would be helpful to be specific as to how much.

We added the long-term mean net transport through Long Strait (0.17 Sv northwestward) to the text.

3) Related, it would also be interesting to quantify in the model how much of the Chukchi outflow enters the Arctic via Herald Canyon directly (rather than through Barrow Canyon).

The long-term mean (1980-2018) net transport between Wrangel Island and the center of Herald Shoal is 0.29 Sv northward and between the center of Herald Shoal and the Alaskan coast (Point Lay) is 0.24 Sv northward.