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 plan to address 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 will add more 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 climatological ocean temperature and salinity at rest and was forced with the Coordinated Ocean-sea ice Reference Experiments version 2 (CORE2) reanalysis.

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.

The 9km model output was available on a daily timescale (instead of just monthly mean) and so was 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 will make 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 can make reference to this earlier in the text.

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 will address the increasing trend in transport through Bering Strait in the revision.

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.