Ocean Science Discussions

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Comment on os-2021-43

Anonymous Referee #1

Referee comment on "On the circulation, water mass distribution, and nutrient concentrations of the western Chukchi Sea" by Jaclyn Clement Kinney et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-43-RC1, 2021

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.

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.

Line 65. These water masses are Pacific Summer Water.

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.

Line 75. Any reference?

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

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.

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

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.

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

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

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.

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.

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.

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.

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).

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

Line 395. Fig 11cd?

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.