

## ***Interactive comment on “Assessment of variability of the thermohaline structure and transport of Atlantic water in the Arctic Ocean based on NABOS CTD data” by N. Zhurbas and N. Kuzmina***

**Anonymous Referee #2**

Received and published: 1 July 2019

Comments on: “Assessment of variability of the thermohaline structure and transport of Atlantic water in the Arctic Ocean bases on NABOS CTD data” by Zhurbas & Kuzmina

General: This work uses CTD data from mainly the NABOS experiment to compute geostrophic velocities and transports of Atlantic water along the continental slope of the Arctic Ocean (the Eurasian Basin). A level of no motion is chosen either at 1000m or, in the case of St. Anna Trough, close to the surface to catch the expected bottom intensified flow. The variability of the Atlantic water properties and transport both along the path of the Atlantic water and between different years is presented. The inflows through Fram Strait (the Fram Strait Branch) and down the St. Anna Trough (the

C1

Barents Sea Branch) are identified and discussed separately.

The manuscript is long and introduces many details, but some crucial discussions are missing. Only CTD data are used in the transport calculations, but no comparisons with other existing estimates are made, neither with current measurements at the slope, nor with transport estimates from the gateways, Fram Strait and the Barents Sea opening. The discussion about the variability of the properties of the Atlantic inflow branches is different. Here many comparisons with existing descriptions about the Atlantic water circulation are made, and most of the conclusions agree with previously published results. In spite of analyzing an extensive data set, both in time and space, few new results are presented.

Below some specific points are listed that should be looked at. They are given in order of appearance not significance. The authors way of writing is different from mine and I have not tried to make any specific, editorial changes. However, the manuscript would benefit from a tighter and more focused writing.

Specific points: line 12: Polarstern

line 45: change “increased” to higher.

line 47: The BSB is also cooled directly by the atmosphere.

line 52: change “observationally derived velocities” to direct current observations. There are also several estimates of the geostrophic transports through Fram Strait which could be cited.

line 55: I would remove “density driven” most readers know the meaning of geostrophic flow

line 57: Internal waves disturb the density surfaces and will therefore affect the geostrophic calculation.

line 62: “array”?

C2

line 64: Interannual is temporal.

lines 66 to 70: are not needed, remove.

line 75: "famous" Perhaps an overstatement.

line 77: "locations. . . .are".

line 92: Equation 1 shows the thermal wind relations. The geostrophic velocities are given by  $-rho_f v = -dp/dx$ ,  $rho_f u = -dp/dy$

lines 102-104: If you go directly for the transport between the stations, it is just to integrate the specific volume anomalies of two neighbouring stations from the reference level to the surface.

lines 105-120: I think that this part is too detailed and more obscures than clarifies what is done. Moreover, the main problem with the geostrophic computations in the Arctic Ocean, especially near the continental slope, is not where the level of no motion is located but rather: Is there any level of no motion at all? The geostrophic estimates made of e.g. the transport of the West Spitsbergen Current in Fram Strait are always lower than estimates based on direct current measurements. There is a barotropic velocity component that is not captured by the geostrophic calculations.

lines 124-137: This long paragraph, indicating what should be, but is not, done, could be shortened considerably (or removed). I do not know what "masking" means in this context, but if you have a dense enough station net to resolve the eddies (or waves) the transports generated by the eddy will cancel. If it is not dense enough, there will be an error.

lines 148-149: The in situ density is what you use in geostrophic computations. It is not something that can be adopted or not at will.

lines 158-160: Here you claim that the potential density anomaly, not the in situ anomaly, determines the transport. Which is it? (see comment above) I think you

C3

could shorten (or remove) lines 155-161.

lines 177-199: This should be shortened. Furthermore, I miss here references to Hanzlick and Aagaard (1980), Schauer et al. (2002a) and Dmitrenko et al. (2015) who discuss the recirculation of the Atlantic water in the trough and the outflow of dense water from the Barents Sea.

I also do not see why the Voronin Through is mentioned here. If the section on the top panel is the one shown on Figure 1, it does not extend to the Voronin Trough, and the bathymetric feature seen on the right is not a ridge separating the St. Anna Trough from the Voronin Trough.

lines 200-213: The properties of the inflows to the Arctic Ocean vary and I think that it is of little use to try to fit the water masses into strict definitions. The FSBW is in its upper part generally warmer and more saline than the BSBW and also in the deeper layers the BSBW shows up as a salinity minimum. However, some denser BSBW contributions can show up as high salinity contributions, but these inputs would sink to deeper levels below 1000m (Dmitrenko et al., 2105).

lines 213-237: The description of this section is difficult to understand because the sections shown in Figure 4 are too small to reveal any of the details discussed in the text. If I had not known this section from Schauer et al. (2002b) (not referred to here) I would not have been able to follow the details.

In general, the figures and the comparison between figures would be improved, if the horizontal scale is the same on all figures. As it now is, a section across the Eurasian Basin occupies the same width as a short section just down the slope.

lines 241-243: It is not appropriate to talk about a gravity current here. The outflow from the shelf, either from St. Anna Trough or farther east, has here reached its neutral density level and moves horizontally with the boundary current. After all, we do not refer to the North Atlantic Deep Water moving south along the North and South American

C4

continental slopes as a gravity current.

lines 290-419: TS analysis. This part is too meandering, and it is difficult to find out what the authors want to communicate. Showing isopycnals (preferably sigma-1) in figure 8 might help. Also 8f should not have sigma-0 but sigma-1 as vertical axis. One question addressed here appears to be the contributions from the Barents Sea that is lost beyond the section at 103E. Are the NABOS stations from 126E shown in figures 8e & 8f taken to the bottom or only to 1000m? The “true” BSBW referred to by Dmitrenko et al. (2015) should at 126E be found below 1000m and if the stations do not extend deeper than 1000m it is no wonder if this contribution is not observed. In the upper part the TS shape of the NABOS stations looks like less saline Barents Sea Branch Water being intrusively mixed into the more saline Nansen Basin water column.

This section should be shortened and made clearer. It was nice to see the references to Dmitrenko et al., (2015) and Schauer et al. (2002b) referred to here, but they should be referred to earlier as should Schauer et al. (2002a) (not referred to).

lines 427-428: The stations at 82N in the St. Anna Trough shown on Figure 1 do not extend to the Voronin Trough. Are there other stations not shown on the map?

lines 420-545: This again is a long section and could be shortened. The most significant result here is the small transports and comparisons with direct current observations should be made. Woodgate et al. (2001) find a flow in the boundary current at the Laptev Sea slope of 5 Sv, which then splits with 2.5 Sv moving in the Amundsen Basin along the Lomonosov Ridge and 2.5 Sv crossing the ridge and flowing along the East Siberian Sea slope. Why are there so large differences in transports? The possibility of the presence of a barotropic component of the transport and also the effects of the sloping bottom should be discussed. There are also no comparisons with the measured and computed inflows through Fram Strait and over the Barents Sea.

The deduced recirculation in the Eurasian Basin is not new, but was suggested by Rudels et al. (1994) and for the warm Atlantic inflow in the 1990s a return flow along

C5

the Lomonosov Ridge was observed in 1994 by Swift et al. (1997).

Summary: I think this manuscript could be published but not without a major rewrite. It should be shortened, especially the more general descriptions of the circulation and of the methods used, and the focus should be on the new findings.

---

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2019-54>, 2019.

C6