Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-54-AC3, 2019

Ocean Science Discussions

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

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Received and published: 24 July 2019

Dear Referee #2,

We are very grateful for Your attention to work and comments. Of course, we will shorten the manuscript in accordance with Your comments, but we will try to keep a description of the method of estimating geostrophic volume flow rate, since we believe that this section is important for readers who make similar assessments in other areas of the ocean. We will also discuss in detail the effect of the barotropic velocity component on volume flow rate and take into account all the works recommended by You.

In this preliminary answer to Your comments we would like to discuss some questions. Here we want to focus on two comments.

1. You write: 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."

From our point of view, the choice of a reference horizon for estimating geostrophic volume flow rate is very important. Yes, we agree that there are the cases when the horizon, where the velocity equals to zero, does not exist (due to the barotropic velocity component). Therefore, it is important to find a horizon where the velocity is low. Such a horizon is located in a layer in which the horizontal density gradients are minimal. It is highlighted in the manuscript.

Regarding the barotropic velocity component: the effect of barotropic additives on volume flow rate is described below in the manuscript (lines 118−124). According to our analysis, the CTD sections were made in the areas of the Arctic Ocean covered with ice, and, therefore, the wind-driven elevations of the free surface could not cause a large barotropic velocity component. Exceptions could occur in the eastern part of the Eurasian Basin, where the ice cover could be discontinuous. We will discuss these cases in detail in the article. We will also re-analyze the available information on the ice conditions during the measurement period under the NABOS program. The question is: do You suppose that even when the sea surface is covered with ice, a barotropic additive can significantly affect the volume flow rate near the slope?

2. You write: 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)."

This paragraph is devoted to the analysis of the “signals” of the presence of different water masses in the various zones of the Basin and is necessary to clarify some results obtained in paragraph 3.1. It is important to note here that a pronounced signal clearly indicates that the water mass has entered the area of observation. However, the absence of a signal indicates one of the following: a) the water mass did not enter the area of observation; b) it entered the area of observation being highly transformed, namely, mixed with other waters. We will insert a brief explanation in the text.

If by the term sigma-1 You mean a potential density calculated from the 1000 m horizon, then all figures 8a-e should be redrawn, since the potential temperature was calculated from the zero horizon. Moreover, each figure of Fig.8 contains a T-S diagram, based upon the CTD profiles, obtained in the St. Anna Trough. However, the depth of the Trough, where these CTD profiles were obtained, was only 600 m (see Fig. 9), and the knee-like feature was observed in the depth range of 300−600 m (see Fig. 9). It should be also taken into account that in paragraph 3.1a all transects were drawn using the sigma-theta from the zero horizon, and it is logical to observe a certain correspondence in the presentation of data. It is also important that such a presentation of our results makes it convenient to compare them, for example, with the results of the

work (Dmitrenko et al., 2015), in which the zero reference horizon was used to estimate potential density.

Now we think that the most optimal solution is to draw lines of equal density calculated from the zero horizon in Fig. 8a-e, and leave Fig. 8f unchanged.

In the same comment You write: "One question addressed here appears to be the contributions from the Barents Sea that is lost beyond the section at 103E."

There is no such statement in our article! Possibly we could not accurately express our reasoning.

Based on the analyzed data, we obtained (to be precise) that the BSBW signal (the main part of the “knee”) either weakens strongly and distorted towards 126E (similarity is lost with a “perfect” knee signal; see Fig. 1 in the attachment), or is not observed at all at this longitude (Fig. 8 of the manuscript). We assume that such a situation is typical. It suggests that the BSBW and FSBW begin to mix intensively immediately after 103E. However, the FSBW signal is well identified at 126E and further along the slope of the Eurasian Basin (and even in the Makarov Basin), while we cannot say the same about the BSBW signal. Naturally, we also observed rare cases of deviation from the typical situation. For example, the observations were exceptional in 2002, when a rather intense BSBW signal was observed at 126E (see Fig. 2 in the attachment to the answer). The analysis of rare atypical cases is beyond the scope of this work. We will clarify our conclusions on the results obtained in the manuscript. We also do not exclude that additional studies are required based on new CTD data to verify our conclusion about the typical BSBW signal pattern at 126E and further along the slope of the Eurasian Basin.

Further You write: "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."

NABOS-09 CTD data at 126E in 5 cases reached a depth of 1100 m, and in one case 1800 m. In the caption to Fig. 8 it is signed that the depth range for the curves "12" is 500−1200 m. In this figure we have presented the T-S diagram of curves “12” up to the maximum depth of measurements for each profile. It should also be noted that some data of the NABOS CTD profiling, on the basis of which our conclusions were made, reached the depths of more than 1500 m.

We would like to draw Your attention to the fact that in the paper we consider only the main part of the “knee” (BSBW, region II in Fig.8 e). We did not analyze of the transformation of the “true” mode of the BSBW (see lines 378−380).

Further You write: “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.”

Fig. 8 provides useful information on the BSBW signal and its transformation as it moves away from St. Anna Trough. From our point of view the intrusive layering can play an important role in the transformation of the BSBW waters (see, for example, (Kuzmina et al., 2011)). To explain the mechanism of this process, targeted research is needed.

Kind regards, Nataliya Zhurbas and Natalia Kuzmina

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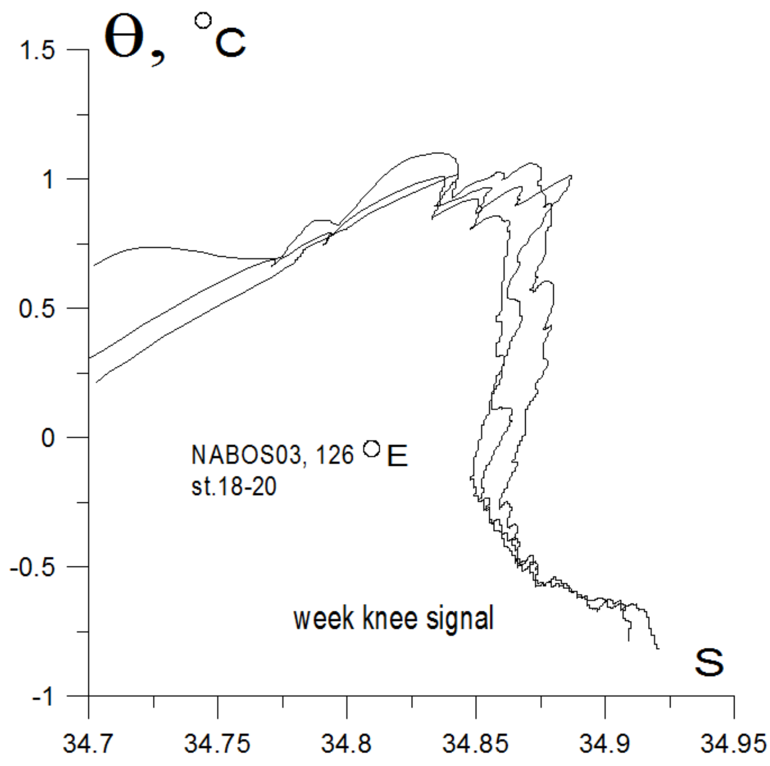

Fig. 1.

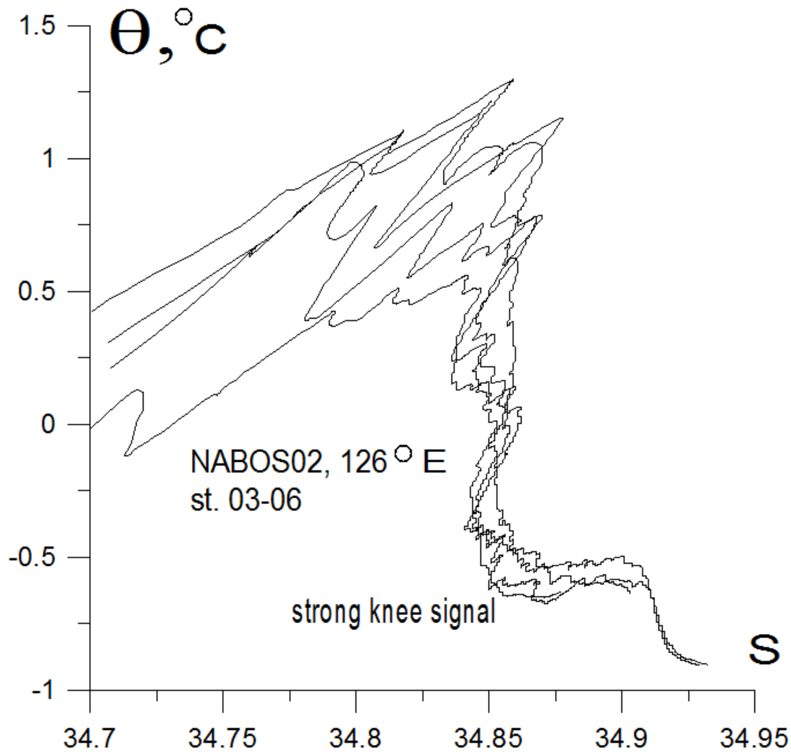
θ, °C
1.5
1
0.5
0
-0.5
-1
NABOS02, 126 ° E
st. 03-06
strong knee signal
S
34.7
34.75
34.8
34.85
34.9
34.95

Fig. 2.