The Editor

Dear Dr. Fer,

Many thanks for your kind attitude to our work. All your comments have been accounted for as follows.

The write-up needs substantial improvement. Please make an effort to improve the narrative. I attach a marked-up version with some comments you could consider (material to avoid, move, restructure etc.).
All your comments (the sticky notes) in the attached marked-up version were accepted.

Regarding the analysis, I find it crucial to include a careful error estimate for the geostrophic volume transport analysis, and a better discussion on the barotropic contribution.
We added to Table 1 and 2 the mean values of the geostrophic volume transport estimates along with the 95% confidence intervals. The barotropic contribution was thoroughly considered in the Discussion chapter, lines 603-622.

Reviewer 1

Dear Reviewer 1,

Thank you very much for your comprehensive review. All your comments have been accounted for as follows.

1. All findings of the paper should be placed in the context of the existing literature. I provided some references in Minor comments.

2. Care should be taken to separate spatial and temporal variability. See my minor comments for specifics.

Minor comments:

1. Abstract, lines 11-12. Sentences like "Estimates. . ." in the current for do not carry any useful information and should be modified or skipped completely.
The sentence was dropped.

2. Line 15: Instead of "on" one can use "along"
"On" has been changed for "along" is several similar cases.

3. Abstract, lines 24-26: Same as in comment 1.
We left it lines as is.

4. Line 34: 2000s, not 2000-ies.
Corrected.

5. Lines 35-38: Same as comment 1.
We included to here the mean values of the transport estimates.

6. Intro: More recent papers related to analysis of NABOS data can be useful for the analysis and should be mentioned in the Intro: Pnyushkov, AV, et al. Heat, salt, and volume transports in the eastern Eurasian Basin of the Arctic Ocean from 2 years of mooring observations, Ocean Science, 2018 Dmitrenko, IA, et al. Atlantic water flow into the Arctic Ocean through the St. Anna Trough in the northern Kara Sea Journal of Geophysical Research: Oceans 120 (7), 5158-5178 Pnyushkov, AV, et al. Structure and variability of the boundary current in the Eurasian Basin

of the Arctic Ocean, Deep Sea Research Part I: Oceanographic Research Papers 101, 80-97
We included the recommended references to the Introduction.

7. Fig 1. One can use different colors to show different years
This would not be convenient because e.g. the 126°E section was repeated in 10 different years, so the colors would merge.

8. Line 111: "original", not "primary".
"Primary" was dropped.

9. Lines 134-148: The paper by Pnyushkov et al. 2018, Structure and dynamics of mesoscale eddies over the Laptev Sea continental slope in the Arctic Ocean, Ocean Science 14 (5), 1329-1347 can be useful in this context.
We included the reference to the Discussion chapter.

10. Lines 191-210: Materials of this paragraph should be viewed in the context of the recent paper by IA Dmitrenko et al. (2015) cited by the authors.
This paragraph was re-worked accordingly.

11. Lines 217-231: These estimates of water mass parameters may be obsolete considering strong changes which occurred over the recent decade or so (e.g. Polyakov et al. Greater role for Atlantic inflows on sea-ice loss in the Eurasian Basin of the Arctic Ocean, Science 356 (6335), 285-291)
The difficulties in definitions of water mass parameters due to climate change are addressed in Discussion chapter, lines 553-563. Reference to Polyakov et al. (2017) was included to there.

12. Line 238: "Strip", not "stripe".
Corrected

13. Fig. 8. Please use colors to separate profiles.
Done.

14. Lines 382-396. Somehow the authors should take into consideration temporal change of water masses at the selected locations vs. spatial changes. They can do that by analyzing repeated NABOS CTD sections and compare temporal and spatial changes.
The temporal change of water masses and transport has been discussed in Chapter 3.3. The spatial change was addressed in Abstract (lines 29-37), Chapter 3.1 (lines 137-406) and Discussion (lines 630-660). Also please see our previous reply to your comments during the interactive discussion.

15. Line 411: Please repeat your conclusion here.
Done (see lines 369-371).

16. Fig. 9: Please provide profiles in color.
Done.

17. Lines 432-439: Method of defining the area is not well described.
Now the method is well defined (see lines 399-400).

18. Table 1: Please include year as the third column for each line.

Year is given in the first column (see the Table 1 caption).

19. Lines 476-478: Please place these estimates in the context of paper by Pnyushkov et al. 2015.

The reference to Pnyushkov et al. (2015) was added.

20. Line 493: What authors are referred here? Why the distance of the AW core from the slope is the key parameter for the flow dynamics?
AW is not a passive mixture, it can affect the basin's dynamics. The distance of the AW core to the slope controls the flow instability. We added an explanation and a reference (lines 440-441).

21. Lines 511-522: Please place these results in the context of recent Pnyushkov et al. 2018 paper.
The reference to Pnyushkov et al. (2018) was added.

22. Discussion of volume transports should incorporate sensitivity of these results to northward extension of sections which varied in time and space.
Done (see lines 583-585 in Discussion chapter).

23. Lines 537-547: This paragraph should be placed in the context of existing mooring-based estimates of water transports across the Barents Sea and Fram Strait.
Done (see lines 567-629 in Discussion chapter).

24. Lines 549-554: This paragraph should be placed in the context of the mooring- based estimates by Woodgate et al. 2001. Note, that my comments 23-24 will encourage the authors to discuss why their estimates are much lower than those coming from mooring observations e.g. Pnyushkov et al., Woodgate et al., Dmitrenko et al. Paragraph, lines 563-587, gives some hint, but discussion is far from complete. Particularly, I am not happy with the authors' attempt to explain their low values of water transports by limited area of sections.
Done (see lines 567-629 of the Discussion Chapter where a thorough comparison of the geostrophic volume flow rate estimates obtained in this work with the other studies is considered.

25. Lines 602-625: Please place these results in the context of recent Pnyushkov et al2018 paper.
Done (see Discussion chapter).

26. Line 618: "Pulse", not "impulse".
Corrected.

27. Summary: Please try to avoid sentences like lines 627-628, 632-633. Summary includes materials which were essentially a brief overview of the previous sections. Thus, I do not repeat my comments related to the previous sections here for Summary, but the authors should go through their summary and check whether my criticism in in the previous comments is applicable here.İ
We dropped those sentences.

Reviewer 2

Dear Reviewer 2,

Thank you very much for your comprehensive review. All your comments have been accounted for as follows.

Specific points:
line 12: Polarstern
line 45: change "increased" to higher.
Corrected.

line 47: The BSB is also cooled directly by the atmosphere.
We pointed out it in the revised MS

line 52: change "observationally derived velocities" to direct current observations.
Corrected.

There are also several estimates of the geostrophic transports through Fram Strait which could be cited.
Some citations of the kind are given in Discussion chapter. If you mean a specific article, please let as know about its title and authors.

line 55: I would remove "density driven" most readers know the meaning of geostrophic flow.
Removed.

line 57: Internal waves disturb the density surfaces and will therefore affect the geostrophic calculation.
Internal waves are ageostrophic disturbances. For correct calculation of geostrophic velocities, the internal waves' disturbances should be removed. We focused on geostrophic estimates of the volume flow rates because they are less affected by internal waves.

line 62: "array"?
Corrected.

line 64: Interannual is temporal
That's right, 'temporal' was dropped.

lines 66 to 70: are not needed, remove.
We would prefer to keep these paragraph to formulate clearly the main goal of the paper and approach we used.

line 75: "famous" Perhaps an overstatement.
"famous" was removed.

Corrected.

line 92: Equation 1 shows the thermal wind relations. The geostrophic velocities are given by - rhofv = -dp/dx, rhofu = –dp/dy
Equation 1 was dropped (the Editor's advice).

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.
Why to the surface? Cold and low saline surface layer has nothing common with the AW. These lines were dropped (the Editor's advice).

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.
To our mind, the choice of the no level motion level is always important because e.g. if we took it at the bottom in the St. Anna Trough, the geostrophic transport of BSBW would change sign. This paragraph was considerably shortened, and a discussion on the barotropic velocity

component was added to the Discussion chapter.

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.
This long paragraph was considerably shortened; now it does not contain mentioning of "masking".

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.
This sentence was dropped.

Comment: 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 could shorten (or remove) lines 155-161.
This sentence was removed.

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.
This paragraph was shortened, and the references were added.

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.
The section on the top panel does not extend to the Voronin Trough, but it does show that the BSBW overflows a ridge-like elevation (i.e. a bathymetric feature) seen on the right and therefore one could easily imagine that the BSBW would overflow a ridge separating the St. Anna Trough from the Voronin Trough. We modified this peace of text accordingly.

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).
You are right. However, to calculate the volume flow rate of a specific water mass, one needs to have a strict definition of it. We added a discussion on the issue to the Discussion chapter.

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.
We agree that the horizontal scale in Fig. 4 provides a poor resolution of the BSBW near the slope. We referred Schauer et al. (2002b) to here and described their results here and also in Discussion chapter.

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.

Given that the NABOS CTD section at 31°E, 2008 (Fig. 2), and the PS-96 section (Fig. 4) are 60 and 900 km long, respectively, the same horizontal scale on all figures will result in 15 times difference in horizontal size of the figures. We could not imagine it…

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 continental slopes as a gravity current.
We dropped mentioning of gravity flows everywhere in the MS except for the St. Anna Trough.

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 part was thoroughly re-worked. The sigma-0 isopycnals were added to Fig. 8. To our mind, the sigma-0 is more appropriate to here than sigma-1 because most of TS curves in Fig. 8 corresponds to the upper 1000m layer and it is better for comparison with Figs. 2-6 where the sigma-0 contours are presented. Some NABOS stations from 126E shown in Figs 8e and 8f were taken to 1800m. The shape of the curves in Fig. 8f will not change principally if sigma-1 is taken instead of sigma-0 for the vertical axis. All the references imaginable were added. Some results of the TS analysis were discussed in the Discussion chapter. Also please see our previous reply to your comments during the interactive discussion.

Comment: 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?
Mentioning of the Voronin Trough to here was dropped.

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.
Please see the Discussion chapter where the issue was thoroughly discussed. It's not clear to us what do you mean by the term "sloping bottom effects". Please explain. If you believe that there is a need to include more references to the Discussion chapter or elsewhere in the manuscript, please provide information about the titles and authors.

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

We know that this recirculation has been known for a long time. We estimated the transport and explained what this transport should be attributed to. A reference to Rudels et al. (1994) and Swift et al. (1997) was included (see line 224).