Response to Reviewer 1’s comments

We thank Anonymous Referee #1 for their review of our manuscript. In this response letter, the reviewer's comment is written in black Arial and our response is in purple (this font style). Our final response will include more details together with a new version of the manuscript with tracked changes not provided here.

Summary

In this manuscript the authors suggest an existence of newly emerging pathway for the warm and saline Atlantic Water entering the Arctic Ocean via the northwestern Barents Sea. They propose that the new corridor is located between Edgeøya and Hopen, the islands situated southeast of Spitsbergen, in the easternmost, shallow part of Storfjordrenna. The authors present and thoroughly analyse recently collected observations, including a year-long time series from moored instruments spanning 2018-2019, as well as three hydrographic transects from ships during the autumns of 2019-2021. The obtained results are put in a wider climatological context drawing upon a large database of quality-controlled observations spanning nearly a century, from 1930 to 2019. Although the available database primarily covers summer seasons, it provides sufficient information to reconstruct the climatological vertical hydrographic section near the mooring location.

General assessment

The manuscript's topic aligns well with the scope of 'Ocean Science 'and addresses the evolving role of the Barents Sea in the context of the warming Arctic and changing ocean climate, a subject of extensive discussion within the scientific community in recent years. Despite the somewhat limited dataset in terms of both time and spatial coverage (comprising a single shallow mooring and relatively short hydrographic surveys obtained in autumn), the authors made an effort to comprehend and characterise the variability occurring across various time scales in meticulous detail. Through the application of various filtering techniques, the authors have successfully distinguished between slow-acting and fast-acting mechanisms underlying the oscillating currents and observed changes in water temperature. In my opinion, the manuscript is written and illustrated well enough and makes a reasonable research statement in the previously undocumented area. However, I also found it slightly overloaded with content and details and thus difficult to follow at times. Therefore, I suggest

that it can be published after some minor corrections.

Thank you for the thorough and detailed review and the constructive feedback. We are very pleased to read that our manuscript was well-received. In the revised version, we reduced the details not central to the paper and streamlined the content for an easier reading. Please see below where we address your specific comments.

Comments and remarks:

Introduction:

Line 28-30: Please add some newer references since the mentioned, although crucial studies, are more than 20 years old. Incorporating some recent studies will help strengthen the manuscript's relevance and ensure that it remains up-to-date with the latest advancements in the field.

Thank you for pointing that out. We have added citations to several recent studies for this paragraph, including Rudels et al. (2013), Lind et al. (2018), Årthun et. al. (2011).

Line 59: 'Summer of 2016 had the warmest and longest-lasting marine heatwave in the Barents Sea' – you may add 'so far'.

We have added 'so far'.

Line 70: 'Here, we investigate the physical processes that drive and mediate the inflow of Atlantic-origin water masses' – I would say that the water masses are rather briefly classified in this paper, if at all. I wonder why: is it problematic to compare the CT and SA with previous classifications made for T&S? Or there is another reason behind it., i.e. you focus on mechanisms rather than structure of the water column itself? In this case, providing a brief overview of water masses may be sufficient (one-two sentences).

That is a good point – we did not use strict water mass classifications, as this is not central to our study. Using the relevant water mass classifications that are appropriate for the nearby regions would complicate the analysis and discussion without adding more insight. Classifying water masses using CT and SA instead of T and S is often done and is typically not an issue. We chose, in the end, to focus on the mechanisms and whether the water observed is of Atlantic origin, i.e. whether it is (modified or transformed) AW from Storfjordrenna that carries

heat into the colder domain of the Barents Sea. We have now added a definition of Atlantic Water (CT exceeding 2°C and SA exceeding 35.06 g/kg) and clarified what we mean by Atlantic-origin water by inserting “We define the Atlantic-origin water, as the AW that is modified or transformed en-route from the WSC through Storfjordrenna into relatively warm and saline water compared to the surrounding water masses but colder and less saline than pure AW.”

Data & methods

Line 78: ‘70 meter deep saddle on Hopenbanken between Storfjordrenna and the interior of the Barents Sea’ – describe here what ‘saddle’ is in bathymetry. Also, what ‘banken’ and ‘renna’ are (instead of indicating it only in the ‘Results’ section).

With “saddle” we refer to the crest of the sill in the bank between the trough Storfjordrenna and the interior of the Barents Sea (Olga Basin). We clarified it so that it reads ‘on the shallow bank Hopenbanken between the trough Storfjordrenna and the interior of the Barents Sea (Olga Basin)’.

Line 95: ‘the measured current were in fairly good agreement with Arc5km2018’ – what does ‘fairly good’ mean here? Also, change ‘current’ for ‘currents’.

We clarified by adding “i.e., ellipse inclinations agreed within 14 degrees and semimajor axes within 1 cm/s” and changed ‘current’ to ‘currents’.

Line 99: ‘Wind speed and direction in the region are taken from the ERA5 reanalyses’ - have you considered or tried to use CARRA reanalysis? Any thoughts on this?

We tried both ERA5 and CARRA in preliminary analyses. Comparing them, we found that they agreed well. And considering our use case, we concluded that ERA5 had a sufficiently good temporal and spatial resolution for our analysis. We preferred ERA5 because it made the analysis quicker and easier as CARRA is a relatively heavy data set on a less convenient grid.

Line 120-121: ‘the multitaper method from Percival and Walden (1993) with Slepian data tapers (e.g., Slepian, 1978; Thomson, 1982). To analyse the time-variability of the spectral components, wavelet transforms following Lilly and Olhede (2009) with generalised Morse wavelets (Olhede and Walden, 2002) were used’ – adding a brief explanation about the general purpose of using the methods mentioned (rationale behind and how they contribute to the study's objectives) in the manuscript could enhance the reader's understanding and

contribute to smoother reading.

We agree. Our general purpose in using these methods was to identify different time scales associated with different processes. To clarify this, we changed the first sentence to 'In order to identify the time scales of variability, we used spectral analysis', and reformulated the last sentence to "The values for the parameters that define the Morse wavelets were chosen to be $\gamma = 3$ for symmetric wavelets and $\beta = 5$ to resolve the variability on time scales between semidiurnal and a few weeks reasonably in both frequency and time."

Line 134: 'bottom half of the water column' – why? Do you aim to separate the surface layer circulation or rather ADCP data quality away from transducer was a limiting factor here?

The ADCP data quality was quite good through most of the water column throughout the time series (the range shown in Figure 6d is the range with quality-controlled data). The choice of using the bottom half of the water column was due to having temperature only at the bottom. While the near-bottom temperature may not be representative for the whole water column when the water column is at times stratified in temperature, available CTD profiles suggest that the lower half of the water column is typically well mixed. We added a sentence ("The velocity data are depth-averaged over the bottom half of the water column to use with the temperature measurements that are available only near the bottom.") to this paragraph to explain this.

Results:

I advise the authors to consider prioritising the most significant results and simplifying the presentation of detailed analyses, particularly in Section 3.4. The abundance of details makes it challenging for readers to follow the discussion effectively. Instead of presenting all available analyses, the authors might consider using a simple table with numerical data to present these results. Additionally, referring large part of the results solely to Figure 8 can be difficult for readers.

Thank you for the constructive feedback. We agree that there are details in the text and figures (especially in Section 3.4 and Figure 8), which are not critical for our results and conclusions. We do agree that especially Section 3.4 and Figure 8 can be simplified. In the revised version we removed Figure 8, as the important parts are visible in other figures (Figures 7, 9, and 10), and shortened the text in Section 3.4.

Line 152: ‘salinity has increased by 0.05 g/kg to 0.35g/kg’- it’s interesting how 34.4 isohaline deepened, and that negative T anomaly is noticeable in this location as well (Fig 4b-c) in the last decades in the Olga Basin. Is it an effect of more intense mixing? Or perhaps this is a signal of the Polar Front sharpening and concurrent steepening of isohalines/isopycnals? As we know from ‘Introduction’ section (Lines 38-42) the amount of inflowing freshwater has decreased in the northern Barents Sea…

Thank you for pointing this out, which we didn’t catch earlier. We now mention the deepening of the 34.4 isohaline toward the Olga Basin and the colocated negative T anomaly. In order to answer your question, a targeted analysis is needed. Having sparse data in OB, we are hesitant to interpret “details” or areas with especially sparse coverage and low standard deviation (due to coverage). In Section 4.4, we have a short discussion on the role of changing stratification in the Olga Basin.

Line 161: ‘At this time, AW was present in Storfjordrenna and close to the mooring’ – again, I miss the water mass classification in this manuscript. As the AW definition was not mentioned in the Introduction and Data & methods part, it’s necessary to introduce it here before you start to describe the results. As you use CT and SA, it will be good to define AW in these scales.

We agree. In the Data and Methods section, we added the CT&SA thresholds we used for AW (CT ≥2°C and SA ≥ 35.06 g/kg) and what we mean with “Atlantic-origin water” (“We define the Atlantic-origin water as the AW that is modified or transformed en-route from the WSC through Storfjordrenna into relatively warm and saline water compared to the surrounding water masses but colder and less saline than pure AW.”). We also specified the AW properties observed in Storfjordrenna on this transect in the sentence “At this time, AW was present in Storfjordrenna and close to the mooring (not shown): 70 km southwest of the mooring, water with temperature 2.9 °C to 3.7 °C and salinity 34.84 g/kg to 35.02 g/kg was found at depth, which is within the temperature range for pure AW and slightly lower than the salinity threshold of 35.06 g/kg \citep{sundfjord2020}. Also relatively warm water of around 0.5 °C was situated at depth 10 km to the west of the mooring position.”

Line 198: ‘to 34 cm s−1 near the surface’ - when exactly? It will be good to find it in the plot - one cannot see the local maxima near the surface.

In the beginning of March – we have clarified this and improved the readability of the paragraph.

Discussion:

Winds, ice, tides, and AW inflows as well as upstream conditions definitely work together to shape the seasonal and interannual variability. However, attempting to address all the complex factors in one comprehensive article is probably ambitious task. I found the discussion too broad, it's understandable given the extensive range of results presented in previous sections. However, I miss a clear statement regarding which mechanisms are deemed most responsible for the observed high interannual variability.

We have added in Conclusions that the semidiurnal tides lead half as much heat across-saddle compared to weather-band processes, and also added a sentence about the large-scale forcing and upstream conditions that likely are major drivers of the interannual variability.

Line 323: 'water column has not yet cooled down at that time' – indeed, the observed differences between mid-October and November suggest a potential regime shift from Atlantic Water (AW) dominance to Arctic Water (AW) dominance during that time of the year. But of course, it could also be an exceptional year with more/warmer inflowing AW as you wrote.

Fig. 5 indeed indicates a shift from Atlantic to Arctic Water dominance during that time of year due to atmospheric cooling and influence from the colder, fresher environment in northeast. However, our Fig. 12 suggests that for autumn 2020, there was a stronger inflow of Atlantic Water lasting into winter, unlike other years where November was either Arctic dominated or cooled down. Autumn 2018 stood out as warmer and saltier through October and November, with sea ice starting later (Figure 6). Lundesgaard et al. (2022) also observed this south of Kvitøya. We interpret this as, in addition to the normal seasonal cycle, interannual variability in Atlantic Water inflow to the northern Barents Sea substantially influences the study area.

We modified the discussion in the manuscript to better reflect this point and include a reference to Lundesgaard et al.

Figures:

In general, I think a few plots and subplots would be better with some additional information that would make them more self-explanatory. Increasing the thickness of lines, enlarging markers, and reducing clutter can improve the visibility of key data points. It feels like this manuscript has many more figures than only 12 (plus these in appendices).

Thank you for the constructive feedback. We agree with your suggestions and have adjusted the figures accordingly. We have also removed Figure 8 (see below).

Figure 3: isotherm 0 deg C (light grey) - too like 200 m isobath - can it be changed?

We agree. The isotherm is now blue and should be more distinguishable from the isobaths.

Figure 7f-g: Perhaps some simple legend naming the lines can be added to read these plots without checking back and forward with the caption?

We have added a legend and also given each curve a distinct colour. We now show both rotary components for both seasons in a shared panel to reduce the number of curves.

Figure 8: This plot seems a bit too complicated to me. Also, it would profit from adding some legends.

We have removed Figure 8 since it contained information that was not central to the manuscript. We agree with your suggestion above to prioritise the most significant results. The main results which we wanted to communicate through Figure 8 are also visible in Figures 7, 9, and 10. Furthermore, removing Figure 8 allows removing some text that describes this figure, and further simplifies the content for the reader.

Figure 9: Why is there no current anomalies arrow in the subplot c)? The reference unit vectors for wind and currents are missing, as well.

We have added reference vectors for wind stress (0.2 W/m^2) in panel b and for current (5 cm/a) in panel d.

In Fig. 9c, the composite average of the current anomaly is minuscule (about a hundredth of the current anomalies in panels b and d) because we have averaged over the non-extreme wind stress conditions. Panel c shows the spatial picture of the centre of the cross (standard deviation cross) for the pale blue dots in panel a, which represents weak wind stress from north/northeast (small negative Ekman transport) and a (very close to) zero current anomaly.

Figure 12: ‘Sea ice concentration (black, left vertical axis) and sea surface temperature (red, right vertical axis)’ - from which product - write it here, also - add years on the subplots - it would be easier to look at.

Thank you for this suggestion. We have added the years on the subplots and added that the

data are from OSTIA.