Referee #3, Anonymous

This is our preliminary response to the reviewer's comments to encourage discussion when the discussion period is still open. We will provide a more complete response in our final response shortly after the discussion closes. The reviewer's comment is reproduced in black Calibri font **followed by our response starting with Re in red, bold Arial font.**

Overview and general recommendation:

The manuscript describes the outcome of a mooring effort, carried out between Jun 2016 and August 2017 across the Norwegian Atlantic Slope Current off the Lofoten Islands at the so-called Lofoten Escarpment. The authors exploit the data from a mooring array that consisted of three deep sea moorings. Two of them, moorings MN and MW, were located about 6 km apart from each other across the slope current. A third mooring, MS was located almost 30 km further upstream close to the Gimsøy hydrographic repeat section. A fourth mooring, MB, was located in the interior of the Lofoten Basin and was not part of the analysis. The authors use the mooring records, mainly velocity data obtained from Longranger ADCPs as well as T/S information from MicroCATs or temperature loggers, to address the Atlantic Water (AW) layer within the Norwegian Atlantic Slope Current that is captured by the moorings. While there are already descriptions of the Slope Current from the sections located upstream, the authors state that they provide the first mooring based description for the Gimsøy region off the Lofoten Islands. The authors describe the general nature of the velocity structure in the upper water column and find the strongest velocities in the winter period. This timing coincides with the time of the warmest temperatures observed in the AW layer. The authors furthermore infer transport time series for the two moorings MN and MW, explain their choice of a respective area over which the transport is calculated and finally quantify the volume transport for the AW layer. The authors furthermore address the forcing and find a correspondence between the along-stream wind forcing and the along-stream current component. Finally, the authors infer energy conversion rates from the mooring records, in particular baroclinic and barotropic conversion rates that describe the transfer of mean potential energy into eddy kinetic energy and the transfer of mean kinetic to eddy kinetic energy. The baroclinic conversion rate can only be estimated for the first three months of the deployment period due to otherwise missing data. The authors find conversion rates with magnitudes similar to estimates inferred for the East Greenland Current and the West Spitsbergen Current. Due to limitations in the mooring data set the authors have considered output from a high-resolution ROMS model. The respective analysis is part of an appendix to the paper. Therein, the authors aim at verifying how representative the mooring-derived energy conversion rates actually are. They conclude from the model analysis that the baroclinic energy conversion dominates over the barotropic energy conversion.

Re: We thank the reviewer for the detailed reading and constructive comments. We addressed all comments as detailed below. We do not respond to the above text, which is a nice summary of our paper (note we edited Grinvoy to Gimsøy in the above copy).

In general, the paper is written well enough. But I personally found it sometimes a bit tiring to read all the abbreviations. This is probably a matter of personal taste. I did wonder, however, why the model analysis was somewhat "hidden" in the appendix. The authors draw important conclusions from this model analysis. Any reader might easily miss the respective discussion by simply ignoring to read the appendix. Therefore, I think, this analysis deserves to be built into the main text.

Re. Thank you for this suggestion. We agree. We integrated the Appendix to the main body of the revised manuscript. We also attempt to remove some abbreviations.

The study of the authors contributes to improving the knowledge of one of the major currents transferring the warm and saline Atlantic Water towards the Arctic. I find that the manuscript addresses interesting scientific outcome on the nature of this current off the Lofoten Islands that is of interest to the readers of OS. The figures are generally of high quality. However, I partly missed information regarding the methods applied to the mooring time series. For example, it was several times mentioned that the mooring succumbed to “knock-down” events. But how these events were eliminated from the data remained unclear. There are other minor requests for clarification that I think will help to improve the manuscript further. Therefore, I recommend a minor revision of the manuscript.

Re. Thank you for your assessment of our manuscript. We made minor revisions and clarifications to address your detailed comments below. We also improved the description of the methods applied to the mooring data. Fer (2020) (see also response below) is the mooring data set together with a detailed data report. The data set and the report are openly accessible with CC BY 4.0 license. Unfortunately, neither the URL, DOI or the fact that this is a data set was apparent in the citation. We did not notice this. This reference (data and the data report) was a reason why we kept the description of data treatment relatively concise. We now corrected the citation and also include the URL in the data statement.

My detailed comments are given below:

Page 1, line 25: the statement that “the front current is relatively poorly known” somehow contradicts the statements that follow in the next sentences. Therein, the authors quote several studies that provide transport estimates for the front current for various location. It might help to clarify what exactly is “relatively poorly known”.

Re. We replaced this opening sentence with “The front current, which is not addressed in this study, has not been measured in detail using current meter arrays, but geostrophic transport estimates are available from hydrography.”

Page 3, line 36: please highlight the location of the Lofoten Escarpment in Figure 1 by adding a respective label. Same sentence starting with “there might. . .”: there is a word missing, “be”?

Re. Done

Page 3, line 59: please add something like “based on the mooring records” at the end of the sentences

Re. Done

Page 4, Table 1: as there are different styles/cultures to write down dates, I suggest to write months using letters like May, Jun, Sep. This avoids that people mix up days and months.

Re. Done

Page 4, line 62: it remains unclear what kind of manuscript “Fer (2020)” actually is or where it can be assessed. The respective reference does not provide any relevant information. So, at present, any reader is not able to locate information on the data set other than the one mentioned here. Same holds for page 5, lines 84/85, where the same reference is mentioned.

Re. Thanks for noticing this. Unfortunately, Fer (2020) was not formatted properly. It is the mooring data set together with a detailed data report, openly accessible with CC BY 4.0 license from the Norwegian Marine Data Centre. Neither the URL, DOI or the fact that this is a data set, was apparent in the citation (because of the formatting). We did not notice this. We now corrected and also updated the data availability statement with the link.

Page 4, line 80: previously, it was said that the used ADCPs were of type RDI 75 kHz Longranger. Now, they are addressed as RDI 75 kHz Sentinel Workhorse. To my knowledge, such a device operating at 75 kHz does not exist. According to the Teledyne-RDI web page, ADCPs of type Sentinel operate at frequencies >= 300 kHz and thus have a much shorter range than the Longranger ADCPs. Please, clarify.

Re. The reviewer is correct. "75 kHz Sentinel" is a mistake and now corrected. In MN, MS and MW we used only Longranger ADCPs. (Additional 300 kHz Sentinels were also deployed in MB, but not reported in this paper.)

Page 5, line 88: please, provide more information on the observed knock-down events, e.g. how often did they occur, how deep did the moorings descend, and how was this effect eliminated from the considered data ? Very much later (page 15), it is mentioned in the text that the data set was actually interpolated and gridded. This information and related specifics are missing here.

Re. We provide some description now, but refer the reader to the data report for a detailed description. Specifically, pressure time series are shown in the report, and inform about the knock down events.

About interpolation and gridding: Data from all instruments are first averaged into one hour intervals (if the sampling rate was faster) and then interpolated to a common 1-hour time stamp. The time variable depth (pressure) records were constructed at each time stamp and for each instrument using vertical interpolation of the known target depth (of instruments with pressure sensor) and the measured pressure to the target depths of all instruments. Hourly profiles of temperature, salinity and horizontal current were then vertically interpolated to 10-m vertical resolution. A depth level with a data coverage less than 30% of the total measurement duration was excluded. This 1-hour-10 m vertical homogeneous, gridded data matrix was cleaned from short segments of data (especially in the outer ranges of the ADCPs) by filling with NaNs when a duration of segment with data was less than 3 days.

Page 5, line 100: please, refer here to the Copernicus Marine Environmental Monitoring Service (CMEMS) as the data provider, since there are still a number of papers out that still claim AVISO to be the data provider. Use of CMEMS data furthermore expects if not requires a proper credit of their data use, which is missing in this manuscript. My guess is that ECMWF expects something similar. Finally, as EKE is not a property provided as part of the used data product, how is EKE defined here ? Did you just consider the provided geostrophic anomalies ? The data set provides both, anomalies and absolute velocities. The present text is not clear enough on what kind of velocity fluctuations actually been used.

Re. We now refer to E.U. Copernicus Climate Change Service and E.U. Copernicus Marine Service Information as the data provider, and properly cite ERA-5 and ERA-Interim. We calculated the EKE using the geostrophic current anomalies obtained from the sea-level anomaly. This is now clarified in the revised version.

Page 5, line 116: please, add "2017" after "March".

Re. Done

page 7, line 135: it looks like the cross-component was also quite high in spring. In the upper part of the water column (Figure 4b) other seasons seem to be higher than winter or are of comparable magnitude. Could you comment on that?

Re. We revised this part as: "Cross-slope component was weak (typically ± 0.02 m s^{-1}) and increased in spring and winter, with largest 200-600m depth-averaged values in winter (0.05 m s^{-1} at MW) and an increased variability with depth (Figure 4b, d). In the upper part of the water column at MW, averaged cross-slope velocities in fall exceeded the winter values. This is consistent with

the increased EKE_g at MW location, calculated from satellite measurements in November 2016 (Figure 3c)."

Page 10, line 171: you could either repeat the separation distance of 6 km here, or otherwise provide readers with the size of the Rossby deformation radius at this location

Re. We repeat the separation distance (note that this is now reduced to 5 km; see response to reviewer 1).

page 10, lines 176/177: from figures 4 and 5 it is obvious that there isn't any velocity data at depth < 200m at mooring MW. At times, there are data missing as deep as 300 m. Also MN does show data gaps for z < 200m. So, please, clarify how it is possible to infer the transport for the 50-650m range.

Re. The shallowest available measurement is extended upward to 50 m. This is now clarified.

At MW, velocity measurements are limited in the vertical. The data gap is 18% at 250 m depth and rapidly increases to 60% and 85% at 200 and 190 m. Temperature at MW is better covered with 20% gap at 90 m, increasing to 70% at 80 m. Velocity measurements at MN have 35% gap at 150 m, increasing to 50% at 80 m.

Page 10, line 179: how did you treat the temperature information outside the mooring array or during those times, when there wasn't any temperature information for mooring MW? Did you consider the depth of the 5C isotherm to be constant across the entire width of the area used for calculating transports? Please, clarify as well.

Re. For full-duration transport calculations, we used the temperature record from MW, which is available for the entire deployment and is well-resolved in the vertical. We extended the uppermost temperature measurement to 50 m when there were gaps in the data (typically above 90 m depth, see above). These points are now clarified.

page 10, line 190: please, clarify; relative to what reference level? Same line: "currents peak" or "the current peaks"?

Re. clarified and corrected: relative to surface pressure; curent peaks

Page 11, line 200: as the summer season is covered twice, is "summer" meant here as the average of both summer seasons? Was there any difference between the two summers? One might guess so by looking at Figure 6.

Re. Yes, we averaged both summers. We now also report and discuss the differences between summer 2016 and 2017. We do not show the mean profiles separately because Fig 4 is too crowded. In summary (using only depth levels where there's more than 70% data for each season):

Summer 2016 and summer 2017 averaged temperature profiles at MW are very similar, equal to within 0.5C in the upper 600 m and identical in deeper layers.

At MW, u is about 1 cm/s larger below 300 m (a barotropic increase) in summer 2017, and shear is stronger in the top 300m, increasing by 6 cm /s to 200 m depth.

At MN, below 400 m (bottom 250 m) u is the same, but shear is stronger in summer 2017 higher in the water column, with u increasing by an additional 10 cm/s to 200 m depth.

Transport is stronger in summer 2017 relative to 2016. Average values, standard deviation and standard errors are now listed separately for each summer in Table 2. (Using the revised width calculations in response to reviewer 1, average Q (± standard error) increases from 1.4 (±0.2) Sv in summer 2016 to 1.9 (±0.3) Sv in summer 2017).

page 12, Table 2: Please write Q_N and Q_S in the same way as it is used in the text and in the table, i.e. with small letters for "N" and "S". As will also be my question regarding Figure 7: as "annual" refers to the entire time series, and as this comprises two summer seasons, does this enter the uncertainties? Or asked differently, what is included in the uncertainties mentioned here?

Re. Corrected (we are now using p(ositive) and n(egative)). The averaging durations, standard deviations, degrees of freedom and standard errors are now calculated and clarified in the table and in the text.

Page 13, equation 1: if EKE is inferred from along-stream and across-stream velocities, it makes sense to keep the previously inferred terms u_a and u_x. Here and later in the text, the authors switch to u and v.

Re. We now use u and v (along and across isobath components) throughout.

Page 13, line 241: please, provide a reference.

Re. Inserted Spall et al. (2008).

Page 14, lines 243/244: these lines need fixing. Furthermore, equation (2) does not contain a rho_0, which is part of equation (3), but a rho', which is not introduced. What reference density was used ? Shouldn't the right term of equation (2) be negative?

Re. Corrected, and all these points now clarified. We used ρ_0 = 1027 kg m^{-3}. The sign of Eq(2) is correct in the convention we described (it can also be obtained by simplifying Eq. A1; note the sign of the last term.).

Page 15, lines 256: four times use of the word 'obtain'

Re. Improved the text.

page 15, lines 260, sentence starting with "The conversion rates calculated from. . .": please remove this entire sentence and the following as the information is identical to the one given in lines 279ff. There, it fits much better.

Re. Done.

Page 15, line 269: the statement that the estimates from the Fram Strait are comparable to the Lofoten Escarpment is a bit tricky. The former values are O(100) mˆ2/sˆ2, the latter values are O(10ˆ-4) W/mˆ3. So, please, make the comparability more obvious to the reader.

Re. The comparison starts with the EKE density. The values are (50-200)×10^{-4} m^2 s^{-2} (both in Fram Strait and the Lofoten slope). Then we compare the conversion rates (BT and BC) which are O(10^{-4}) W m^{-3} both in Fram Strait and the Lofoten slope. Perhaps the transition from EKE to BT (and BC) is too abrupt? We smoothened this by rewording. We also inserted (in line 265) the following to better interpret the two parameters.

"For reference, a conversion rate of 10^{-4} W m^{-3} for 1 day accounts for ρ_0EKE of O(10) J m^{-3}, or EKE of O(100)×10^{-4} m^2 s^{-2}."

Page 15, line 270: please introduce WSC

Re. We decided to remove this abbreviation because it is used only a couple of times.

page 16, line 298: there is a word missing at the end of the line.

Re. Corrected.

Figures:

Figure 1. Labels like “Norway” in Figure 1a and “NO” in Figure 1b are really hard to see. Think about adding a text label highlighting the location of the Lofoten Escarpment. The unit in the EKE colorbar should read 10ˆ-4 mˆ2sˆ-2, not 10ˆ4 mˆ2sˆ-2.

Re. Done

Figure A1. Both subplots lack a frame, at least in my printed version. Also the grid is almost invisible in the printed version. Maybe the authors can improve that. To the southwest of the red box, there is something like an arc-like pattern of very small-scale features in Fig A1a that look totally different from the remaining parts of the plot. What causes this?

Re. Thanks for pointing it out. We improved Fig A1. Among other improvements (projection, zoom, representation of the transect), the figure now includes a frame and a visible grid.

Regarding the small-scale features mentioned, we observed that these only occur during fall and that they are related to noisy horizontal density gradients. The features are mainly confined between the 250 and 400 m isobaths, and particularly in the southern region of the domain, and only in fall. The isobath range and the region where this “noise” occur are not relevant for our conclusions; therefore we removed them without commenting in the text.