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Ocean Science Discussions

Interactive comment on "Norwegian Atlantic Slope Current along the Lofoten Escarpment" *by* Ilker Fer et al.

Anonymous Referee #3

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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 Grinvoy 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

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Discussion paper

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 Grinvoy 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.

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.

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Discussion paper

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.

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".

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"?

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

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.

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.

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.

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.

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.

Page 5, line 116: please, add “2017” after “March”

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?

Page 10, line 171: you could either repeat the separation distance of 6 km here, or oth-

erwise provide readers with the size of the Rossby deformation radius at this location

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.

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 5°C isotherm to be constant across the entire width of the area used for calculating transports? Please, clarify as well.

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

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?

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.

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

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?

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

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.

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.

Page 15, line 270: please introduce WSC

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

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.

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?

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