Response to the reviewers

We thank Andrey Pnyushkov and Wilken-Jon von Appen for their thorough and constructive review of our manuscript. Their comments have been very helpful in revising and improving the manuscript. Below, their comments are shown in black arial and our responses in blue calibri.

During the revision process, and while extending the analysis of the model output, we identified an error in the de-tiding step of the model data processing. The model fields have therefore been reprocessed, and the affected figures and the corresponding text have been revised. Identifying and correcting the error, reconducting the analysis, and improving the study with additional analyses delayed the revision process substantially, unfortunately. We apologise for the delay and we appreciate the patience of the reviewers and the editor.

Response to Andrey Pnyushkov's comments

The manuscript investigates the mechanisms of baroclinic and barotropic instabilities in the Atlantic Water boundary current north of Svalbard and their relationship to the variability of eddy kinetic energy (EKE). Using year-long mooring observations combined with output from an eddy-resolving ocean model, the authors examine the seasonal evolution of EKE and associated energy conversion rates. While the topic is scientifically relevant and the dataset is valuable, the manuscript requires major revision to improve clarity, strengthen the connection between observations and model results, and more convincingly support the conclusions drawn.

Thank you for taking the time to provide such a thorough review of our manuscript. We greatly appreciate your clear and concrete suggestion for improving clarity and strengthening the connection between the model and the observational results. Below, we outline how we implemented your recommendations.

General comments:

1) The presented analysis of the mechanisms of baroclinic and barotropic energy conversion is thorough and certainly of interest to the oceanographic community. However, I find the study somewhat incomplete, as it does not provide sufficient insight into the consequences of these instabilities, which are potentially manifested as sub- and mesoscale eddies. The authors themselves point out that the link between the barotropic and baroclinic conversion rates and the eddy kinetic energy is rather weak, raising questions about the actual importance of these processes, at least within the specific region

where the mooring array was deployed. Furthermore, the model simulations employed in this study do not appear to substantially advance our understanding of how the diagnosed energy conversions translate into eddy generation or eddy-induced heat transport. Given this weak connection with eddy processes, I would appreciate it if the authors could further elaborate on why understanding barotropic and baroclinic instabilities remains important in this context and what broader implications their results may have for the local or regional ocean dynamics.

We are pleased that you found our analysis thorough and of interest to the community, and we thank you for raising important points that aided us in further analysis and revising the manuscript.

In our observations, the direct link between the conversion rates and EKE variability is indeed weaker than expected, except for agreement on a seasonal time scale. The model results provide some insight into this: point-based estimates of conversion rates must be interpreted with caution, as they are not directly representative of larger volumes. However, they can serve as order-of-magnitude indicators.

Also in the model, the connection between EKE variability and conversion rates was weaker than anticipated. We do not interpret this as evidence that the processes are unimportant, but rather as an indication that there are open questions regarding how best to analyse and quantify their influence. We expanded the analysis of the model fields to better quantify this connection and found that temporal correlations between the conversion rates and EKE were significant in areas with considerable conversion rates. We have added a paragraph in Section 3.3.2 with a summary of this analysis (lines 296–300):

"While there is not a conclusive, direct correspondence between local conversion rates and EKE variability, several segments along the isobath exhibit statistically significant correlations (not shown; Pearson's correlation coefficient, r, ranging from 0.65 to 0.92). In particular, BT and EKE are significantly positively correlated in regions where BT is mainly positive (directly upstream and 20 km downstream of the western array, and across a broad region surrounding the eastern array (Fig. 7b). Near the western array, the predominantly positive BC also shows a positive but weaker correlation with EKE, (r from 0.51 to 0.67)."

We also recognise that the analysis of the model data did not fully demonstrate how the energy conversions translate into eddy generation or heat transport induced by eddies. Our main goal for using model data was to provide context for the observation-based estimates and to assess how representative the conversion rates were. Motivated by your suggestion, we investigated eddy-band temperature fluxes in the 100 m – 500 m layer covering the AW inflow and the connection to the conversion rates. Although the link between EKE and conversion and temperature fluxes / -divergence was not clear, we identified some regions in which higher average conversion seem to affect the eddyband heat transport, and that the highest integrated eddy temperature flux divergence (as a measure of eddy-induced heat transport) occur in winter. We added a new panel in

Fig. 7 and corresponding text in Section 3.3.2 (lines 310–318):

"The divergence of eddy temperature flux integrated along the 1400 m isobath (Fig. 7e) has pronounced temporal variability, ranging from −1.8 °C cm s^{-1} to 2.3 °C cm s^{-1}, with a mean value of 0.3 °C cm s^{-1}. The five months with the largest integrated eddy temperature flux divergence occurred between November and February. Averaged in time, the cumulative integral of eddy temperature flux divergence increases starting from near the western mooring array to until 10 km downstream of the eastern array (Fig. 7e, upper panel, left axis). The largest contributions to the cumulative integral are from a broad zone extending downstream from the western mooring array (35 km to 70 km along the isobath) and a narrower zone centred on the eastern array (125 km to 140 km; Fig. 7e, upper sub-panel, right axis). The broad zone starting from the western array coincides with areas where both BT and BC are positive on average (Fig. 7b, c), while the more localized smaller maximum near the eastern array is approximately co-located with the local BT maximum there."

2) Even if the observational dataset and its processing have been previously described and published elsewhere, I would recommend that the authors include a concise but sufficient description of the data and methodology within this manuscript to ensure it is self-contained and understandable to the reader. In particular, information about the vertical coverage and resolution of the instruments should be provided. For example, it is not clear how the distributions of current speed, temperature, and density in Fig. 2 were obtained, given that a substantial portion of the section is not covered by ADCP or T,S measurements. Please clarify how these data gaps were handled and indicate the potential uncertainties this may introduce into the analysis. Including these details would significantly improve the transparency of the study.

We have added a clarification at the end of Section 2.1 on how the sections of mean current speed, temperature, and density in Fig. 2 were obtained (lines 86–95):

"The analysis is based on data from the deeper mooring pairs which provided current profiles with a 5 m–10 m vertical resolution from the seafloor to approximately 300 m depth. Here, only three levels of density measurements at approximately 300 m, 500 m, and 700 m depth were available. Our analysis is therefore based on layer averages over 300 m to 700 m (Section 2.3.3), focusing on the levels close to the CTD sensors to limit the errors from interpolation. For context, we present seasonal mean sections of velocity and hydrography at both mooring arrays (Fig. 2). For computing mean structure of the current velocity and hydrography across the slope, a spline-Laplacian routine was used for interpolation, described in detail in Koenig et al. (2022b). While current measurements cover the water column at W1, there were no temperature and salinity measurements available, and the hydrographic structure over the upper slope was supplemented by historical hydrographic data. The details on the gridding and the associated uncertainties are described in the Appendix of Koenig et al. (2022b). The uncertainties in constructing the sections do not affect the analysis in this paper."

To increase transparency, Fig. 2 has also been revised. We now show the vertical coverage of the ADCP for both seasons. The 90 % shading was adjusted to 80 % because one mooring was several hundred metres with 89 % coverage in one season. The 50 % shading was reduced to 20 % to avoid suggesting that no data were available higher in the water column. We have also added the distribution of all the temperature sensors in Fig. 2.

Regarding the data gaps and related uncertainties, we used only levels where measurement coverage in time and across a pair of moorings was sufficient and near instrument target depths (to allow calculations of BC). A more detailed explanation is provided under the point about Section 2.1 below, and a brief clarification is included in the paragraph from the revised manuscript pasted above.

3) Taking into account that a portion of the observed EKE likely results from the advection of mesoscale eddies past the moorings, as indicated in the text, I am uncertain whether the selected cut-off period of 35 hours is appropriate for capturing the relevant variability. Assuming that the lateral scale of the advected eddies is approximately twice the local baroclinic Rossby radius of deformation (~7 km) and that the typical boundary current velocity is around 15 cm s^{-1}, the corresponding advection time scale would be about 24 hours. Therefore, the applied filtering threshold might artificially attenuate the contribution of advected mesoscale features, leading to an underestimation of EKE and, consequently, of the lateral eddy-induced heat transport. It would be helpful if the authors could assess the sensitivity of their results to the choice of the band-pass filter parameters. Given that model output is available, it should be possible to estimate the typical advection timescale directly from the simulated flow field and thereby verify whether the adopted cut-off period adequately captures the relevant eddy variability.

We have explored the effect of decreasing the high-frequency cut-off period (and sensitivity to the filter type) and summarized the results in a new Appendix C. To avoid contribution from the diurnal tidal band, particularly the energetic K1 constituent, and ideally also the O1 constituent (25.8 h), we applied a sharper FIR Kaiser filter instead of the original Butterworth filter. This allowed us to reduce the cutoff to 27 h without including significant diurnal energy. Advection time scales for different current speeds can be estimated in the range of 24 h to 40 h, so a 27 h cutoff captures much of this variability. The differences were small. As an example, November–December mean EKE at 400 m depth increases from 52 to 56 $cm^2 s^{-2}$ after changing the cut-off from 35 h to 27 h.

We have included a figure in Appendix C of EKE, BT, and BC at one depth to display the effect of changing the passband. This includes the original Butterworth filter with passband 35 h to 14 d, Kaiser FIR filter with the same passband, with 27 h to 14 d, and with 48 h to 30 d that was used in von Appen et al., 2016.

In the main text, end of Section 3.2, we have added this paragraph (lines 238–245):

"Advection of eddies past the moorings complicates the interpretation of our estimates in several ways. A fraction of the calculated EKE may be attributed to eddies advected by the mean flow, adding to the local conversion from mean to eddy energy, potentially leading to an overestimation of locally generated EKE. In addition, the use of a band-pass filter to estimate fluctuations may attenuate contributions from mesoscale features when the filter cutoff exceeds the eddy advection time scale. The advection time scale is 24–40 h, based on along-slope velocities of 10 cm s^{-1} to 15 cm s^{-1} and an eddy lateral length scale of twice the baroclinic Rossby radius of deformation (~7 km). This range overlaps with, and partly exceeds, the 35 h cutoff used in the EKE calculations. Alternative calculations using a shorter cutoff of ≈24 h, excluding the diurnal tidal band while retaining most mesoscale variability, show no significant differences (Appendix C; Fig. C1a, b)."

4) Before undertaking an in-depth analysis of the barotropic (BT) and baroclinic (BC) energy conversion rates, it is essential to demonstrate that the model reliably reproduces the mean structure of the boundary current and its baroclinic structure in agreement with observations. I recommend that the authors include direct model–observation comparisons and provide quantitative statistics of the mismatch. Useful additions would be: cross-slope sections of mean velocity and temperature/density from the model as well as vertical profiles of mean velocity at mooring locations. These comparisons should be shown for relevant seasonal subsets used in the conversion analysis. Demonstrating that the model adequately captures the observed mean flow and stratification is a prerequisite to attributing conversion rates and interpreting mesoscale energetics with confidence.

Agreed. We amended the revised manuscript with a new appendix with two sections on model validation. In Appendix B1, we have included a summary of the validation done by Frank et al., 2025 (DOI: 10.5194/os-21-2419-2025) for which the model setup was originally used. They validated the model setup with data from Isfjorden in Svalbard and from the West Spitsbergen Shelf. These data included three autumn cross-slope sections across the shelf break along 78° N and two years of mooring data at hourly resolution at the mouth of Isfjorden. The simulations showed temperature biases (colder surface waters, warmer deep water over the slope) and salinity biases (too low in the surface layer over the shelf and in the Atlantic Water over the slope). These biases largely compensated, resulting in good agreement in the density structure. Also the regional circulation patterns were reproduced at the cross-slope section. The density time series also had good agreement between mooring observations and the model. The inflow pattern into Isfjorden was well represented, though inflow speeds were slightly over-estimated.

In Appendix B2, we have included across-slope sections of along-slope current velocity and hydrography for the autumn–winter (September to February) and spring–summer (March to August) seasons. These sections cover the slope with the virtual moorings, and we have compared these sections with the observed sections (Fig. 2) for both seasons. We have also included the temporal average of the current velocity as well as the 2 °C isotherm in the 100 m – 500 m depth layer in a new panel in Figure 6. These comparisons demonstrate that the model adequately captures the observed mean flow and stratification.

Specific questions and suggestions:

Table 1. What do the numbers following the dates represent?

They were the times of day. Thank you for pointing out that it was unclear. We have removed this detail from the table and now have only one column with temporal coverage from deployment date to recovery date.

L.84. Please make sure that your model indeed uses a terrain-following coordinate system, as stated, rather than a stretched one (s-coordinate).

The model was set up with stretched terrain-following s-coordinates, using stretching parameters theta_b = 0.1, theta_s = 8, and T_cline = 20. Vertical transformation equation 2 (Vtransform = 2) and stretching function 2 (Vstretching = 2) were used (see https://www.myroms.org/wiki/Vertical_S-coordinate). We clarified this in Section 2.2 by specifying "… 35 vertical stretched bathymetry-following levels".

Section 2.1. It is unclear why the analysis is limited to the 300–700 m depth range, given that the cross-section includes data for the entire water column up to the surface. Is this restriction related to the actual (non-interpolated) data coverage? This limitation excludes the most energetic part of the boundary current near its core, where the dynamics are likely most intense. Moreover, even though the shallowest moorings (W1 and E1) are separated by a distance exceeding the baroclinic radius of deformation, incorporating data from these sites would substantially enhance the analysis and provide valuable insight into the asymmetry of EKE and baroclinic/barotropic energy conversion.

The 300–700 m depth range was the best option for this analysis, mainly due to data constraints, especially for calculating lateral gradients. Temporal averaging required consistent coverage in time, and lateral gradients in BT and BC conversion rates required additionally good coverage at both moorings simultaneously. Limited ADCP coverage at the deepest moorings (W3 and E3) therefore restricted the depth range for BT and BC estimates.

We agree that including data from the shallower moorings can add insight, even though accurate conversion rate estimates are not possible there. In the revised version, we now include a new Appendix to provide details from all moorings. In Appendix A, we have added a figure with EKE, eddy momentum fluxes, and eddy density fluxes from W0, W1, and E1 (excluding eddy density flux at W1, where the temperature and conductivity sensors were lost). Furthermore, as lateral gradients are not required for EKE and eddy fluxes, we can also show them for a larger depth range at the deeper moorings. Although eddy fluxes alone do not directly quantify energy conversion, they provide additional context on the vertical and cross-slope structure of the energetics.

We have added a clarification of the depth range limitation and the inclusion of Appendix A in Section 2.3.3 (lines 148–153):

"Calculations of BT and BC require adequate lateral gradient estimations. First, the separation between two moorings must be sufficiently small, limiting the analysis to the deeper moorings pairs. Second, simultaneous measurements at both moorings are needed, constraining the analysis to the 300 m to 700 m layer because of substantial data gaps higher in the water column at the deepest moorings. EKE and eddy momentum and density fluxes can be calculated over a broader depth range at all seven moorings (Fig. 1), capturing more of the energetic part of the water column where the data loss was greatest. Their vertical structure is shown in Appendix A."

Figure 2. Please label the mooring positions directly on the section plots.

We have added labels ("W1", "W2", etc.) above the black triangles in Fig. 2.

L.185. Where are the BT conversion rates in the upper water column shown?

The wording in this sentence was unclear, and we have revised it. What we intended was "higher in the water column," i.e., closer to 300 m. This curve is not shown explicitly but is included within the standard-error shading in Fig. 4. We present BT with shading to reduce the number of curves in the plot.

Section 3.3. The temporal and vertical averaging applied to the model output appears inconsistent with that used for the mooring observations, making direct comparison difficult. Could this discrepancy explain the significant mismatch between EKE and conversion rates derived from observations and from the model?

The reviewer is correct. Volume-averaged EKE and conversion rates from the model output were obtained by averaging over large volumes and in regions of the slope where the conversion rates were substantial, typically deeper and further offshore than the moorings. This is by design, and provides a "ground truth" of sufficiently averaged conversion rates representative for the region, to compare with the undersampled in situ mooring coverage, as well as mooring output selected to mimic the observation-based analysis.

EKE and conversion rates at the virtual moorings are calculated exactly as those from the mooring observations. In Fig. 8, EKE and conversion rates are shown for in situ moorings and virtual moorings, based on data at the same depths, using only the terms in the simplified Equations 4 and 5 (BT and BC), averaged over 300 m to 700 m (EKE and BT), and monthly averaged. This shows that there is both a discrepancy between observations and the model (that we partly attribute to different time periods covered and potential shortcomings of the model in representing the meso- and submesoscale processes) and between the virtual moorings and a more complete calculation using all terms of the conversion equations and volume averaging along the 1400 m isobath.

Section 4.3. This part of the discussion appears insufficiently supported by the results presented in the manuscript and relies largely on reiteration of findings previously reported by Koenig et al. (2022). It is unclear what new insights regarding lateral eddy-induced transport are provided by this study. Moreover, according to the authors' own conclusions, the substantial BC/BT conversion rates are not directly linked to EKE, and thus are not clearly related to mesoscale eddy activity—an essential mechanism for Atlantic Water ventilation. In its current form, this section seems exceptional and disconnected from the presented results; therefore, I suggest removing it altogether.

We acknowledge your concern regarding the limited link between our estimated conversion rates and EKE. While we do not find a direct causal relationship, there is agreement on a seasonal time scale. Offshore eddy temperature fluxes and flux divergence from the western array are larger in autumn and winter than in spring and summer. This was noted briefly in Section 3.1 but not discussed further in Section 4.3. Although temperature fluxes estimated from a few points must be interpreted with caution, as rotational and divergent terms cannot be separated, they suggest that lateral heat fluxes are enhanced during the same seasons when EKE and conversion rates are elevated.

To address your concerns, we have strengthened Section 4.3 (now Section 4.2). We now include the observed eddy temperature fluxes and their divergence estimated from the moorings explicitly in this Section. We further conducted additional analyses of the model outputs and expanded the discussion on seasonal lateral heat loss due to mesoscale eddy activity, and on the relationship between EKE, conversion rates, and lateral heat exchange in the model. We present results from the temporal evolution of energetics and eddy temperature flux divergence along the modelled boundary current path.

Our results complement the earlier estimations of along-path heat loss and atmospheric heat loss from the boundary current by Koenig et al., 2022 and provide further insight into the seasonality of heat-loss mechanisms. Although our results are not fully conclusive, they form an integral part of our study and help highlight the complexity of the processes involved.

Section 4.4. For the negative BT conversion rates, which indicate energy transfer from eddies back to the mean current, was such re-energization reproduced by the model? Do you observe a statistically significant intensification of the boundary current in the regions where negative BT values occur? Demonstrating this would help make the discussion less abstract and more focused on the underlying physical processes.

We appreciate this question, as negative conversion rates and their interpretation are, to our knowledge, rarely discussed in the literature.

We expanded the analysis of the conversion rates and EKE variability from the model fields. By analysing the correlation between BT and the along-slope velocity component at various locations along the isobath, we identified one zone where BT is predominantly negative, BT and the current speed co-vary negatively, and the current accelerates on average. We expanded the discussion of negative conversion rates in Section 4.1 (lines 382–395):

"Negative conversion rates, observed in both our mooring records (lasting weeks) and the model (lasting months in some regions), have also been reported in other regions (e.g., Spall et al., 2008; Håvik et al., 2017; Wekerle et al., 2020; Fer et al., 2023). Theoretically, negative BT implies a transfer of energy from eddies to the mean flow, while negative BC implies energy transfer that steepens isopycnals by lifting denser water toward the shelf. Our observations provide some support for these interpretations. For example, a period of negative BC in January–February 2019 was followed by an increase in isopycnal slope (Fig. 5c, a). In the model, a wide zone between 80 km and 110 km along the isobath with predominantly negative BT (Fig. 7b), shows a significant negative correlation (r -0.47 to -0.72) with the along-slope current velocity (not shown). Here, decreasing BT is associated with increasing current strength, consistent with a transfer of energy from eddies and to the mean flow. Correspondingly, the mean velocity transitions from below- to above-average values downstream (Fig. 7a, right axis of upper panel), suggesting a local strengthening of the boundary current. However, this relation between negative BT and an increase in $\bar{u}$ is not found in all regions. Due to uncertainties in the estimates from the moorings, we cannot determine whether the observed change in the isopycnal slope is a direct consequence of negative BC or a coincidental alignment. However, the occurrence of negative conversion rates in both observations and high-resolution models supports the interpretation that energy can be transferred to the mean flow or into available potential energy."

As for the relationship between conversion rates and EKE variability, the link between negative BT and boundary-current acceleration is not one-to-one, but may indicate that negative BT contributes to current intensification.

Response to Wilken-Jon von Appen’s comments

The paper uses methods previously applied to other mooring arrays and applies them to two mooring arrays north of Svalbard (other aspects of these mooring arrays had already been presented in other publications in the past). In particular, this allows to get an idea on the stability of the boundary current that carries warm Atlantic Water into the Arctic Ocean. If this boundary current loses water through mesoscale instabilities, that has impacts on the heat budget and thus e.g. the sea ice distribution across the Arctic Ocean. The paper is well-written and clear in all its statements. I find no major issues with it. Especially, the discussion regarding what the point estimates of the conversion rates as obtained from the moorings can actually be used for (what they are and are not representative for) is very useful for the literature. The comparison to the estimates from the numerical model is very insightful. As such I think this paper will make a valuable contribution to the literature and it fits the scope of Ocean Science. Hence, I recommend minor revision in order for the authors to be able to address the minor points below.

Thank you very much for your careful review of our manuscript and for the valuable and constructive feedback. We are pleased that you found the manuscript clear and well-written and that you consider the discussion of the representativeness of mooring-based estimates, as well as the comparison with model results, to be useful. We appreciate your helpful suggestions, which we address below.

L39 Are the references cited on this line really original references for these statements or did they not themselves just cite the supported statements from somewhere else?

Cushman-Roisin and Beckers, 2011 was the textbook reference for barotropic and baroclinic instabilities used in this paragraph in the introduction. Håvik et al., 2017 and von Appen et al., 2016 were referenced for their wording on barotropic instability in regions with strong topographic beta. We have revised this paragraph. We replaced the first reference with Killworth, 1980 (DOI: 10.1016/0377-0265(80)90013-5) for barotropic and baroclinic instability, which is a classical review paper on both types of instabilities.

When we discuss the conditions for instability later in the manuscript, we cited Vallis and Maltrud, 1993 (barotropic, Section 2.3.4) and Charney and Stern, 1962 and Spall and Pedlosky, 2008 (baroclinic, Section 4.4).

Tab.1 “… temporal coverage (UTC) of …”

Thank you for pointing out that the format was unclear. We have changed the column to “Temporal coverage” and only show the dates without time.

L77 “…E3 (Table 1) which were separated by a distance of approximately 9km.” Also, the sentence states that you use only data from the deeper pairs and not the shallower pairs. Why do you disregard this additional information? Maybe briefly comment on that in the main text.

We have revised this sentence to “In this study, we used data from the deeper mooring pairs W2–W3 and E2–E3 (Table 1) as the distance within each pair – approximately 9 km – is small enough for calculating lateral gradients (see Section 2.3.3).” to provide a brief context and to avoid ambiguity about which distances are being referred to.

In the revised version we now include a new Appendix to provide details from all moorings. In Appendix A, we have added a figure with EKE, eddy momentum fluxes, and eddy density fluxes (where calculations are possible).

L92 “April 2019” Why did you not use April 2018 to have the mooring deployment embedded in the model simulation time?

We agree that using April 2018 would have been ideal. However, the model had originally been set up and run for a different study upstream of our area. By the time we sought to incorporate model data into our analysis, it had already been initialized and run from April 2019.

L105 band pass filtering from 35 hours to 2 weeks: Note that von Appen et al (2016) used a filtering of 48 hours to 30 days. While you are free to choose your cutoff times (and they will hopefully not have a huge impact on the results), it is not clear that this will not result in some differences. Given that you later (L296) make direct comparisons to the quantitative results of von Appen et al (2016) and your study it might be worthwhile to be aware (also make the reader aware?) that this is an additional source of possible differences in the numbers.

We agree that different passbands can introduce additional differences between studies, although probably it is a smaller source of difference than, for example, the vertical placement in the water column or geographical location. We have explored the effects of adjusting the passband to quantify the sensitivity to different filter choices and included an additional appendix in the revised manuscript.

By comparing time series of EKE from W2 at 400 m, using our original filter with passband 35 h – 14 d and a similar Butterworth filter with passband 48 h – 30 d, we found that they agree fairly well in magnitude and variability. The percent difference (relative to their average) was on average -5 %. The 10th and 90th percentiles of the difference were -24 % and +17 %, respectively.

The BC maximum at 320 m at W2 increased slightly when using the 48 h – 30 d passband: the November average increased from 2.9 to 3.6 × 10^{-5} W/m^3. We have included a sentence to address this (lines 371–373):

"It should be noted that we used slightly different pass-bands than von Appen et al. (2016) to obtain fluctuations. A comparison between choices of pass-bands and filters is shown in Appendix C. If we use their pass-band of 48 hours to 30 days instead of our 35 hours to 14 days, our November-mean BC increases by 25%."

In line 369, where we directly compare BC maxima, we have revised the value 4 × 10^{-5} W/m^3 (estimated visually) to 2.9 × 10^{-5} W/m^3 (actual monthly average calculation).

L114 Please make it more explicit what the ^-1 refers to by putting brackets around the fraction (partial rho/partial z)$^{-1}$.

Done. We agree that this revision removes ambiguity.

L120 "with 20° (i.e. counter clockwise) … and -5° (i.e. clockwise) …"

Done.

Fig.2 The symbols for RCM and for 90% are both black squares/rectangles. They cannot be distinguished in the plot, so maybe choose some other symbols. Why do you not also show the sensor distribution of shallower moorings in the bottom panels?

We have changed the colour of the ADCP and RCM symbols to light green so that they stand out more clearly against the black shading.

We initially showed only the deeper pairs, as these were the ones we used in our calculations. The shallower moorings were used only for background information (Fig. 2). In the revised Fig 2 we added the sensor distribution for all moorings.

Fig.3. “v is offset by -10cm/s” How so? In which direction? The statement as it stands now is very ambiguous, but it is also unnecessary to do this offsetting, so I recommend to just not offset v.

In the original plot, we had subtracted 10 cm/s from v so that its curve would lie beneath that of u, with the aim of improving clarity. We agree with your suggestion and have updated the plot with no offsets, presenting u in a slightly paler gray to distinguish it more clearly from v.

L179 “remained low” Relative to what? Put this into context somehow.

By “remained low”, we meant relative to our BC estimates, to BT values reported in other studies, and to what would be required to fully explain the observed EKE. We agree that this needs clarification and revised as follows (lines 206–209):

“BT remained weak year-round relative to the conversion rate expected from the observed EKE variability (Fig. 4g, h). It was negligible in summer and otherwise fluctuated within ±0.5 × 10^{-5} W m^{-3}. The persistently low BT reflects both cancellation of oppositely signed momentum fluxes (Fig. 4e, f) and periods of weak velocity shear (Fig. 4c, d).“

We also added context to conversion rate numbers, please see our response to L221/232 below.

L196/210 320m is not “upper water column” in the strongly stratified Arctic Ocean.

You are right – this was not a good way to describe 320 m. We intended it to mean “relatively higher in the water column”. We have changed the wording here and in other instances “upper water column” was misleading.

L221/232 Maybe put these numbers into context: How long do the conversion rates need to be active to produce the observed EKE? This timescale compared to the residence time/advective time over the domain where the estimated conversion rates may be representative may tell one whether the conversion rates and the EKE are reasonably linked to each other, i.e. a parcel of water is subject to the conversion rates for long enough to produce the observed EKE.

A conversion rate of 10^{-5} W/m^3 active for one day produces an EKE of 10^{-3} J/kg = 10 cm^2/s^2. We agree that putting the numbers into context in this way is useful when interpreting the results, especially when discussing whether the observed EKE variability is linked to the estimated local conversion rates. We have added this clarification in the introduction of Section 3.2 (lines 202–204):

"To provide context for the magnitudes of conversion rates and their effect on changes in EKE, note that a conversion rate of 1×10^{-5} W m^{-3} sustained over one day would produce 1×10^{-3} J kg^{-1} corresponding to an EKE increase of 10 cm^2 s^{-2}."

L279 Add the following to make the statement on L301 easier to understand: "… in February as determined from observations at 78°50'N..."

Agreed, we changed the sentence to "Upstream at the 78°50′ N mooring array, the WSC is most energetic …" (line 342–343).

L301 "…downstream of the WSC … from mooring records at ~80°N, ~6°E…"

Agreed. As we changed the order of some material in this section, we revised this sentence to (lines 377–378):

"On the southern flank of the Yermak Plateau at 80° N, further downstream of the mooring observations of von Appen et al. (2016), Fer et al. (2023) estimated conversion rates from mooring records."

L303 Space after "."

We have corrected the spacing after the period.

L307 The statement regarding the equivalence of the two numbers given is misleading. I think what you meant to say is that if the vertical average over the top 1000m of the water column of the conversion rate is 5×10^{-5} W m^{-3}, then the integrated conversion rate over the top 1000m of the water column is 5×10^{-5} m^3 s^{-3}. That is, the two numbers are related to each other by two factors of 10^3: the density of sea water (1025 kg m^{-3}) and the water depth of 1000m.

Thank you for noting that the original wording was misleading. We have changed the sentence to (lines (374–376):

"... magnitudes of 5×10^{-5} m^3 s^{-3} along the slope. Depth-integrated values in m^3 s^{-3} are numerically equivalent to depth-averaged values with units W m^{-3} for a ~1000 m-thick layer of seawater with density ~1000 kg m^{-3}."

L320 Consider adding "… north of Svalbard with implications across the entire Arctic Ocean."

We have added *"with implications for the broader Arctic Ocean."* (line 398).

L334 "earlier observations of eddies" Why would the presence of eddies there indicate conversion there? Was the eddy not just advected in the offshore direction after its formation?

We agree that this paragraph was misleading. Local conversion rates offshore of the mooring locations may not necessarily be connected to eddy observations there, as eddies may be advected from a shallower part of the slope. We have revised Section 4.3, and the relevant part of the paragraph now reads (lines 405–409):

"Limited observations north of Svalbard showed anticyclones carrying warm anomalies offshore from the boundary current (Våge et al., 2016) and other eddy features linked to baroclinic instability (Pérez-Hernández et al., 2017). Anticyclonic eddies carrying AW or warm anomalies have also been identified in high-resolution modelling studies both north of Svalbard (Crews et al., 2018) and upstream (Wekerle et al., 2020). The model results presented here also indicate substantial conversion rates both near the moorings sites and further offshore (Fig. 6)."

L350 "topographic vorticity waves" Is it not more common to call these "topographic Rossby waves"? If that is what you mean, then at least also mention the latter term to make it clear to the reader who might suspect that it is what you want to say, but who might not be sure.

We agree and have replaced the term with "topographic Rossby waves".

L351 "An ongoing study (unpublished)" Would it not be better to call this "pers. comm." and to then specify who this communication is with? As it is written currently, it is unclear who is responsible for this statement.

Yes, thank you for noting this. We have changed this part of the paragraph to (lines 425–429):

"A likely candidate for summer lateral heat loss is topographic Rossby waves, which have previously been linked to increased heat loss from the WSC (Nilsen et al., 2006). An ongoing study (F. Nilsen, personal communication) indicates that the shape of the continental slope near the western mooring

array, together with the reduced stratification and slower current observed in summer, could support topographic Rossby waves with diurnal periods, energized by the diurnal tide."

L359 topographic beta is not the easiest concept ever for readers to deal with. On L257 you call it "topographic beta" rather than "beta_topo" as on L359. I would recommend to also call it "topographic beta" on L359. In addition, it might even be helpful to explicitly refer to its definition on L359: "variation in bathymetry through topographic beta as defined in Section 2.3.4."

Agreed. We have changed the wording to "topographic β" here as well and now explicitly refer to the Section where it is defined (line 437):

"... which depends on spatial variations in bathymetry through the topographic β (defined in Section 2.3.4)."

L375 "…value of Ri in summer was …"

Thank you. This improves the readability of the sentence.

L410/411 Very nice and useful conclusion.

We appreciate the positive feedback – thank you!