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.

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.

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.

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.

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.

Specific questions and suggestions:

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

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

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.

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

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

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?

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.

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.