I have reviewed the revised manuscript entitled *"Documenting the 2015–2017 freshening of the eastern Eurasian Basin of the Arctic Ocean and evaluating its drivers and consequences"* by More, Polyakov and Pnyushkov, along with the authors' detailed responses to the previous review.

The authors have made a substantial effort to address the comments raised in the first round, and several aspects of the manuscript have improved. However, I still have a few concerns that require further clarification, as outlined below.

Response to C1:

I understand the authors' point that a surface-intensified negative salinity anomaly could be mixed downward to greater depths. However, the manuscript also states that layers deeper than 50 m were advected along the slope. This would imply relatively limited time for vertical mixing over the continental slope (compared to the much longer transit time from the Yenisey River and Ob River to the Laptev Sea slope), although such mixing remains possible.

I would recommend clarifying this mechanism more explicitly in the text, with reference to Fig. 12. In particular, it would be helpful to state whether the simulated trajectories crossed the continental slope (especially for M14 and M15) or followed it for some distance (as appears to be the case for other moorings). This would support the interpretation that surface anomalies could have been mixed into deeper layers during these periods.

Response to C2:

Satisfied

Response to C3:

Satisfied

Response to C4:

I am partly satisfied with the response, but I still believe it is important to note that the high correlation between sea ice concentration (SIC) and ocean heat flux — both of which exhibit a strong seasonal cycle — may be partly driven by a third seasonal factor. For example, seasonal wind forcing could simultaneously influence ice drift and upper-ocean currents (and associated shear), thereby contributing to the observed relationship. A brief acknowledgment of this seems to be worth.

Specific comments that might benefit from further clarification or revision (the rest were adequately addressed):

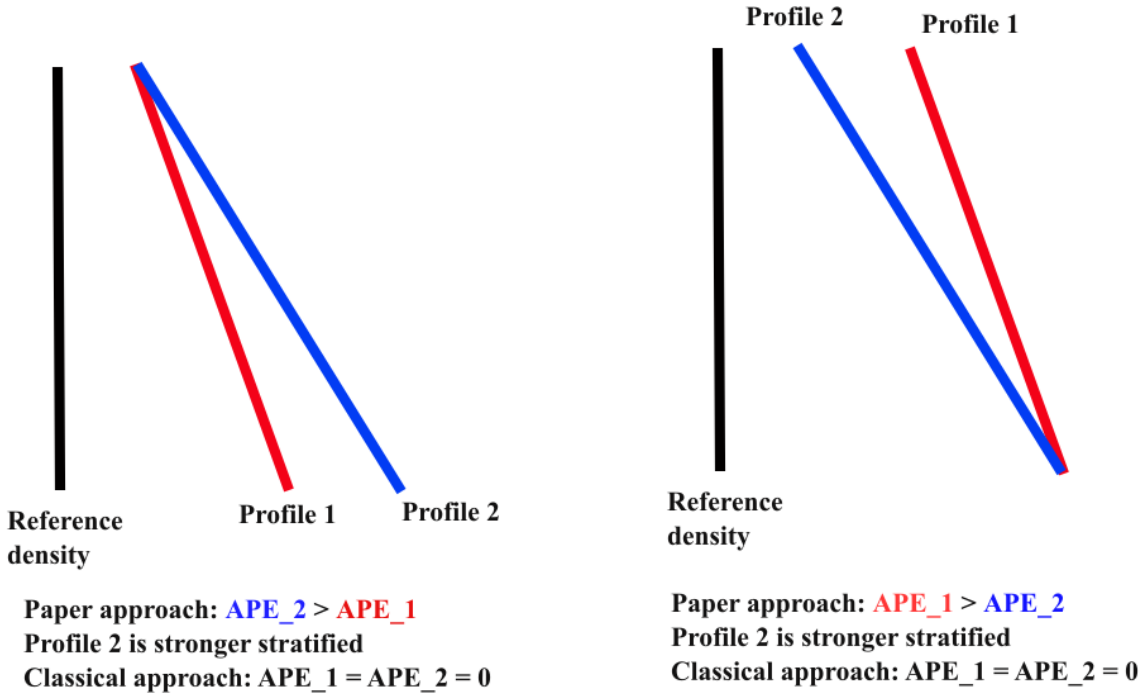
Response to Q13:

Unfortunately, I still have significant concerns regarding the use and interpretation of Available Potential Energy (APE) in the manuscript.

APE, in the sense introduced by Edward Lorenz, measures the departure of a fluid from a minimum-energy (adiabatically sorted) reference state. It is therefore not, strictly speaking, a direct measure of stratification strength. For example, a water column may be strongly stratified yet have low APE if it is close to its sorted state, or exhibit elevated APE due to vertical displacements even if the background stratification is weak. In this sense, equating APE with stratification strength (Line 206) is conceptually misleading. So, the interpretation in Line 266 - "This enhanced stratification was reflected in the pronounced increase in estimated available potential energy across all moorings" - is not strictly correct.

In addition, APE - both in its full definition and in its small-amplitude approximation - is fundamentally tied to perturbations about a *background stratification profile*, not a vertically uniform reference density. The formulation used in the manuscript, based on density anomalies relative to a constant reference density does not correspond to the standard small-amplitude APE framework even though the result may sometimes correlate with a real APE. In the present form, the used equation can be interpreted as a qualitative proxy for stratification-related potential energy. I would therefore recommend revising the terminology and interpretation of this metric to avoid referring to it as APE.

I provide a simple example for clarity. Consider two linear vertical density profiles without vertical inversions, where the second profile has stronger stratification. According to your formulation, the available potential energy differs between these two cases (note that, depending on the vertical structure and the choice of reference density, it may be either larger or smaller for the second profile). In contrast, both the classical APE framework and the small-amplitude approximation would yield zero for both profiles because of no inversions. This example highlights that the quantity used in the manuscript cannot be interpreted as APE and should not be referred to as such.



From these considerations, and given that the primary purpose of introducing “APE” in this study appears to be to characterize changes in stratification, I would strongly recommend using the mean Brunt–Väisälä frequency instead of this energy-based metric. The Brunt–Väisälä frequency provides a direct, well-established, and unambiguous measure of stratification strength, and would avoid the conceptual ambiguities associated with the present formulation.