**Response to reviewers for manuscript "Long-term eddy modulation inhibited the meridional asymmetry of halocline in the Beaufort Gyre" by Lu et al.**

September 2023

The author comment is presented in the following sequence: (1) itemized comments from the reviewer in black, (2) author's response in blue, (3) quotations from the revised paper in indented *blue italic*.

**Anonymous reviewer #1:**

The manuscript has improved significantly from the previous version, both in writing and the overall flow. Although the authors have addressed most of my comments, some clarifications are still needed. I list those comments which require further clarifications below, together with some new thoughts.
Thank you very much for taking the time to review our manuscript. We agree on these constructive recommendations to clarify our scientific results. The manuscript is revised again according to these helpful recommendations.

[1] Halocline depths are used throughout the paper, in context with APE, Eady timescale, and EKE. These variables are more relevant to isopycnal steepness in the halocline (halocline strength as put in the manuscript), not the depth. Halocline depths are associated with both its steepness and water redistribution. It would be clearer if the authors discriminate these terms and use the right term in different sections/paragraphs.
Thanks for pointing it out. We carefully checked sections in the paper to distinguish these variables. The right terms of halocline depth and steepness are used in the revised paper according to your proposal. The variables like APE and isopycnal slope are relevant to halocline steepness. The deepening of halocline depth and its bowl-shaped structure is associated with halocline thickness, depth, and freshwater redistribution.

[2] Paragraph 2-5 in the Introduction section: the authors listed many findings from previous literature, but these paragraphs lack a logical flow which connects the previous work. I recommend restructuring these paragraphs for clarity.
Thanks for your recommendation. We have restructured these paragraphs to enhance clarity and coherence. We have now revised the introduction section by organizing the information in a more logical sequence. We have provided a concise summary of each previous finding and have highlighted how it relates to our study.
Paragraph 2 demonstrates halocline layer within Beaufort Gyre, especially on asymmetrical stratification. Paragraph 3 introduces eddy vertical distribution and horizontal patterns. Paragraph 4 retrospects previous literature about temporal variability of eddy activity under the background of variation in BG physics, like FWC and gyre strength. Paragraph 5 is about the role of eddies in the halocline dynamics.

[3] Line 152: the authors focus on the PWW part of the halocline in the paper. However, the introduction reads like the entire halocline (from ~30m to ~250m) will be explored. It would

be clearer if the authors can clarify their major research topic in the introduction.
Thanks for your recommendation. PWW is a main component of the halocline. To address this concern, we have revised the introduction to explicitly state that our research primarily focuses on the entire halocline. We emphasized the entire halocline that our study aims to examine.

[4] Line 240: Can the authors explain why a symmetrical shaped halocline implies a state of equilibrium? Isopycnal steepness can still change even after halocline becomes symmetric.
Thanks for pointing it out. “a state of equilibrium” is inappropriate here and it is replaced by “a state of stabilisation”. The related sentences were modified in this paragraph.
In the previous period, the asymmetrical halocline was much steeper close to Beaufort sea southern slope and it was shallowing to the northern deep basin, which means stronger baroclinic instability in the south (Manucharyan et al., 2017; Manucharyan and Isachsen, 2019). The asymmetrical halocline influences that the halocline thickness in the south is much thicker than in the north. In the third period, the halocline structure was transformed to a symmetrical shape, with similar halocline depth and thickness surrounding the gyre center. The halocline is transformed to bowl-shaped with flatter isopycnals near the central gyre, which is consistent with the inflated halocline layer (Zhang et al. 2023), meaning a relatively stable state than before (Zhang et al. 2016). In this state, isopycnal steepness can still change with steeper halocline in the edge. This structure is directly driven by a surface Ekman convergence that creates a bowl-shaped halocline (Manucharyan and Spall, 2016).

*(Line 261)*

*……, the halocline depth and thickness tended to be meridionally symmetrical accompanied by flattened isopycnal slope surrounding central gyre, shaped like a horizontal bowl under the forcing of surface Ekman convergence (Manucharyan and Spall, 2016), indicating that it had reached a state of stabilisation (Zhang et al. 2016).*

[5] Figure 5: Days of eddies are strongly related to how fast eddies were translated by background currents. This makes this metric less reliable to represent the eddy activities. Can the authors comment on this?
Thanks for pointing it out. When we discerned days of eddies, the background currents, from velocity field without eddy activity, were removed. This metric is just to examine the eddy lifetime or duration every year to represent eddy activeness.

[6] Line 291: The mooring measurements go up to ~50 m so EKE is not surface-intensified. Section 5 implies that the authors define upper ~100 m as surface (which includes halocline). It would be better if names of different layers are defined in the beginning to avoid confusion.
Thanks for pointing it out. In section 2, the names of the mixed layer and halocline layer are defined. The related texts were modified.

*(Line 143)*

*To investigate the variation in the overall halocline and understand the shifting of oceanic stratification, we consider the depth of the potential density surface $\sigma$=27.4 (25) kg·m$^{-3}$ to approximately represent the base (top) of the entire halocline layer (Timmermans et al., 2020). Accordingly, the depth of the surface mixed layer is also* *identified by the halocline upper boundary (Bourgain and Gascard, 2011; Polyakov et al., 2018).*

[7] Figure 8b: The major topic of this section is interannual variability (seasonal cycle is only mentioned in one sentence). Showing monthly EKE makes it harder to identify the trend and interannual variability. I suggest showing a panel with annual mean EKE.
Thanks for this suggestion. Figure 8b with added annual mean EKE was modified accordingly.

[8] Line 381: I'm still confused how the probability is estimated. Is it the frequency of days with eddies in a period?
The probability is estimated by the frequency of area mean time series of EKE and geostrophic current in each period. We calculated the percentile of these values in every period to estimate here.

[9] Line 419: mixed layer is only the upper ~30 m. As in my comment [6], please define terms in advance to avoid confusion.
Thanks for pointing it out. The terms, including mixed layer and halocline layer, are checked thoroughly and defined. According to our definition, the depth of the surface mixed layer is less than 70 m in the central BG region.

*(Line 174)*

*The depth of the surface mixed layer is less than 70 m. The entire halocline layer underneath the mixed layer, including upper and lower halocline, is mainly at 70–250 m.*

[10] Line 432-433: Can the authors explain why low-salinity water is at the subsurface? With a bowl-shaped halocline, at the same depth, water at the edge should be saltier than that in the basin interior.
With a bowl-shaped halocline, water at the edge still be saltier than in the interior at the same depth. The occurrence of low-salinity water at the subsurface means local salinity decreased at that time, which contributed to deepened halocline depth and increased thickness, so the halocline structure was transformed.

[11] I understand that surface EKE in AL/BSS regions are closely related to halocline eddies inside the basin, but I'm still not clear about the reason of analyzing surface EKE in basin interior. The authors in their previous response explain that surface EKE also impacts halocline, but I don't find the consistency. I suggest the authors add a sentence or two to motivate their analyses of surface EKE in basin interior.
Thanks for pointing it out. We supplemented an explanation to clarify the consistency in the context.

*(Line 359)*

*Based on the previous works, surface eddy activities are directly responding to the extra wind energy input (Armitage et al., 2020), while the subsurface EKE is related to from baroclinic instability and APE release (Manucharyan and Spall, 2016;*

*Manucharyan et al., 2016). Eddy activities at the surface and subsurface are both linked with BG stabilisation, contributing to increased energy dissipation.* ……

[12] I'm convinced with the authors conclusion about the role of eddies in modulating halocline shapes and depths. Besides, wind pattern is an important factor which can directly alter the center of the surface sea level dome and thus the area of Ekman pumping. It's important to at least discuss atmosphere's role in the discussion part.
Thanks for this suggestion. The surface Ekman convergence related to wind pattern is a crucial role in the BG system (Manucharyan and Spall, 2016). We added the influence of atmospheric activity in the discussion part to make results more comprehensive and credible.

*(Line 491)*

*Many studies support that BG system is strongly affected by atmospheric dynamics that contribute to deeper halocline in the interior gyre. The center of the surface sea level dome and wind-forced Ekman pumping area are also highly sensitive to wind patterns (Manucharyan and Spall, 2016; Regan et al., 2019; Timmermans and Toole, 2023).*

**Anonymous reviewer #2:**

Second review of "Long-term eddy modulation inhibited the meridional asymmetry of halocline in the Beaufort Gyre" by Lu et al.

I appreciate the efforts the authors have gone to in order to address the reviews of the previous iteration, and I believe that the structure is now clearer than before. However, the manuscript still has some major problems that, in my opinion, make it unsuitable for publication in its current state. In particular, I do not believe that one of my major points has been adequately addressed.

I have outlined the main points below. Note that some of these have appeared since the previous review. This is either because of wording changes, or because the text was made clearer which made it possible to assess the scientific content without being as affected by difficult sentences. Note that there are still some places where the wording needs to be fixed.

Thank you for your valuable feedback on our paper. We are very grateful for your time and effort in reviewing our work. We have carefully considered your comments and have made these necessary revisions to address the concerns raised in responses to specific comments. The overall manuscript has been carefully checked and the incorrect wording was fixed.

**Major points**

**1) Gyre asymmetry**

I believe that the gyre symmetry found in the manuscript is not a valid interpretation of the data available. In my previous review, I questioned how the northward limit of the datasets (northernmost being 80N) affects the results. Specifically, if the gyre centre is more south, the halocline to the north of the centre – both within, and perhaps further north than the northern

extent of the gyre - can clearly be seen. If the centre is located more north along the section, though, then it is closer to the northern limit of the data and less information is available about what happens up to the northern limit of the gyre and beyond. It is important to acknowledge this as a large caveat, since the gyre has been shown in the literature to move northwards and westwards during some of the time period in question. The authors have provided an additional set of transects along 140W to demonstrate "similar shifts along these two sections". In my opinion, the section along 140W after 2014 demonstrates my point; when the gyre has moved westwards, the deepest part of it occupies less of the transect than it does along 150W, and there is a clear asymmetry in isopycnal slope either side of the deepest part, much like in 150W before 2008. I suggested that the authors could check if the northward limit is making them draw the conclusion that the gyre is now symmetric by analysing the SODA dataset further north than the data extends to. This has not been done.
Thanks for your suggestion sincerely. I have checked the impact of the BG northward limit in the results to improve scientific results. Analysing SODA and CTD, the annual meridional isopycnal slope (north to 90°N) in the halocline layer that represents halocline steepness was added in the supplement. Especially, the slope of the halocline base that varied remarkably within BG box in the first and third periods was added in Fig. 4c in the revised paper. Figure 11a with northmost halocline depth was accordingly modified northwards to 90°N.

Under the background of BG movements westwards and northwards (Regan et al. 2019; Bertosio et al., 2022), halocline variability and its asymmetrical structure are influenced. As shown in Figs. S1, the northward slopes of all isopycnal surfaces, with opposite slopes on the either side of gyre center show typical structure like a bowl under the forcing of surface Ekman convergence (Manucharyan and Spall, 2016). From the view of steeper isopycnals σ =27.4 and 27, the steepest part of the halocline along this transect is occupied in the south of approximately 82°N (some part beyond BG region) over years 2004-2020. When the position of BG center is much closer to the southern slope (before 2008), the halocline lower boundary is steepest on the south side and it is shallowing from central gyre where isopycnal slope is near zero up to ~ 82°N (Fig. 11a), with meridional symmetry, which can be explained by the intrusion of Altantic water and strong Ekman downwelling in the central BG (Karcher et al., 2007; McLaughlin et al., 2004; Timmermans and Toole, 2023). To the further north of the BG region, the halocline is relatively flatter and its slope is negligible (Fig. 4c). When gyre location is to the north after 2014, the northern thickness increased more and thickness was similar surrounding gyre center. In the meantime, the main part of halocline where halocline is deepest and its slope is obvious is well captured in BG region. Using these available data including SODA and observations, although low-salinity anomaly exists in central Arctic just near 90°N (Fig. 11b), we find the variability of halocline structure and eddy activity in the area we focus on is much more remarkable than in the northmost extent. Though observation is just to about 80°, variability on the main part of halocline with time varying is still discerned within the fundamental BG region.

In previous manuscript, we provided an additional set of transects along 140°W to demonstrate there is a clear asymmetry in the isopycnal slope induced by deeper halocline in the south, much like in 150°W before 2008. The deepest parts of halocline along two transects demonstrate that the BG movements influence the halocline. Comparing the views in the first and third periods, the similar deformation of halocline structure with halocline deepening more on the northern side of central gyre along two transect supports our results.

*(Line 238-243,249-255,258-264)*

I understand that much of the gyre is contained within the "BG box", and that it has been used in previous studies. However, there is a difference between computing integrated properties in a region (as is done in previous studies) and analysing vertical structure within different parts of the region. I believe that the latter is more heavily affected by the position of the centre and spatial variability, and so such analyses carried out in a static region must be very conservative in their conclusions. As it stands, in the later period it could equally be interpreted that only the central portion of the gyre is being shown. There is no evidence to demonstrate that this is not the case.

Thanks for pointing it out. We consider the spatial variability of BG (Regan et al., 2019), we put halocline structure to the north of BG box into comparison. For analysing vertical structure within different parts of the region, we compared isopycnal slope within and beyond BG region from 70°N to 90°N in the supplement. As interpreted in the upper response, halocline steepness in the further north is much smaller than in the south from SODA reanalysis. The variability on main part of BG halocline is well captured in the BG box. Considering the influence of the position of the centre and spatial variability on the halocline structure, we determined the position of gyre center by isopycnal slope of near zero in the interior of BG region. Results show gyre center moved and expanded northwards and the entire halocline inflated, corresponding with previous studies (Bertosio et al., 2022；Zhang et al., 2023). From the supplement, the halocline structure tended to be bowl-shaped with flatter halocline surrounding gyre center. Therefore, to the some extent movement and expansion of gyre center also influenced isopycnal slope on either side of central gyre and reduced the asymmetric structure of halocline, which demonstrates our results complementally.

The authors' response that the APE (and thus the halocline slope) flattens, and therefore the gyre becomes symmetric, is not so relevant as an argument, since it is computed within the same region as the section in question, and therefore does not contain information from further north.

I further note that Manucharyan and Isachsen (2019) suggest steeper isopycnal slopes in the south due to bathymetry; with a southern bound very close to the continental slope, the isopycnals will naturally be steeper there. Perhaps when the gyre centre is further from the continental slope, it allows for flatter isopycnals south of the centre away from the slope? This is not discussed.

Thanks for pointing it out. This inappropriate interpretation was modified in revised manuscript. We could not conclude the halocline becomes symmetric directly from APE evolution. APE within BG box, associated with eddy genesis and halocline strength, was analysed to explore BG stability here.

The influence of bathymetry in the south was added through previous literature. Here we take the position with isopycnal slope of near zero as gyre centre. With gyre centre further from the continental slope, partial isopycnals are flatter near BG center (Fig. 4c), while isopycnals are steeper on the edge due to enhanced eddy-induced convergence and BG strength.

> *(Line 249)*
> *Initially, the location of central gyre that is determined by isopycnal slope of near zero in the interior, was very close to the vicinity of continental slopes with the largest isopycnal steepening occurring to the southern side and stronger baroclinic instability (Manucharya and Isachsen, 2019).*

The continued emphasis of the gyre becoming symmetric, which is attributed to eddies by the authors, without discussing how the changing shape/size/centre location of the gyre in itself affects the halocline structure, is a significant oversight. Being limited by the data range is not an excuse for this, especially since the authors could try to verify their conclusion is valid by looking at the SODA data.
Thanks for pointing it out. Sorry for the inappropriate texts related to asymmetry. These were modified. The main point in this study is asymmetrical halocline not gyre asymmetry. As shown in the current study (Regan et al., 2019), the shape of Beaufort Gyre is always asymmetrical. Now study on deformation of the halocline asymmetry is fewer. We just found that the asymmetry of halocline structure reduced in recent years. In the final period, it was more like a bowl shape. We understand changing shape, size, and centre location of the gyre all affect the halocline structure. We took the influence of BG into account but we did not carry out a detailed analysis, due to exploring how much impact on the halocline beyond the scope of this paper. As described above, according to isopycnal steepness in the halocline layer, the location of BG center and fundamental area of halocline were involved in our revised manuscript using SODA and observations in Section 3.2. The influence of gyre center location in halocline structure was specified in the revised manuscript. The eddy modulation in the oceanic stratification and changing gyre center location both influence halocline structure becoming symmetrical. We would discuss both of these factors in Section 6.

**2) Misleading description of figures**
In a number of places, descriptions of figures do not match what the figures show. These ambiguities, and in some cases errors, make it hard to know whether the text is correct or not. I list some examples below:

Figure 2 and Table 1, and associated text (section 3.1) is very confusing. There are multiple places where statements in the text are not apparent in the figure and not backed up by Table 1. Firstly, there seems to be contradictions related to whether a negative trend in halocline depth is a decrease or increase in depth. “A negative trend of halocline depth is clear during 2008-14 in the southern sites of the basin (moorings A and D), but the former and latter periods both mostly exhibit positive trends in halocline depth and thickness” would suggest that positive is a shallowing depth, and decrease in thickness. But then, for mooring B, “the halocline depth continues deepening over the whole period” which has positive trends for each time period, suggesting positive is deepening. Likewise, in the response to review, “the negative trend means halocline depth is lifting during that period, which is not comparable with the deepening trend” suggests positive is deepening. Such inconsistencies make it hard to follow what is actually happening and when, especially since the general trends (increasing

or decreasing) are hard to tell in figures 2a and 2b due to the y-axis and short-term variability.
Thanks for pointing it out. These associated texts were checked for better clarification. A negative trend means a decrease in depth and a decrease in thickness. For better clarification, the original "negative trend" was replaced by "shallowing trend". Likewise, "positive trend" was replaced by "deepening trend". For comparison, the deepening trends of halocline base over the whole period were added in Fig. 2a. At four moorings, the depth halocline base are all exhibiting long-term deepening trends over the whole period 2003-2018, which means deepening in halocline depth.

Figure 2c also has poor text accompanying it. Lines 200-202 do not correctly analyse the figure - there is not a continuous increase before 2009, there is not a continuous decrease between 2010 and 2014 (it goes up between 2010 and 2011!), and it has not been relatively stable since 2015 (the increase from 2017-2018, for example, appears similar to the decrease between 2011 and 2014). This also appears in line 400.
Thanks for pointing it out. There was an error of x-axis in Fig. 2c. The text related with Figure 2c was revised accordingly.

> *(Line 203)*
> *As shown in Fig. 2c, there was a significant increase from ~50 to ~200 PJ before 2010. However, APE was continuously decreasing in the periods of 2010–2013, 2014–2016 and 2017–2021, meaning a flattening of isopycnals in the BG and APE release. Although there was a short-term increase in APE accumulation in 2016-2017, it did not lead to further accumulation, indicating an energy reservoir limit around 150-250 PJ.*

Figure 3 (line 220): after 2014, there is also a difference north-south (just with some fresher water in the far south). So I do not really see a gradual decline in spatial difference
Thanks for pointing it out. "a gradual decline in spatial difference" is not appropriate here. Associated texts were revised.

> *(Line 223)*
> *In the initial period, the halocline base maps highlight significant differences between the north and south. And then the north experiences a much more pronounced deepening of the halocline depth compared to the south closer to the Beaufort Sea slope, which also exhibits more fresher water.*

Figure 4: what is shown by the arrows? The caption says "the depths of halocline base on either side are marked" – either side of what? Is it the interannual mean of the centre? I worry that the two points are not comparing the same parts of the gyre on each plot
In Fig. 4a, we added the interannual mean centre position determined by isopycnal slope of near zero in the interior. The arrows were modified, showing the depth of halocline base on either side of central gyre.

Figure 6, and lines 291-293: many lines on the figure, but most notably the green lines for moorings a and d, are neither bimodal nor decaying directly from the surface.
Thanks for pointing it out. This sentence was modified.

*(Line 310)*
*The vertical structures of EKE in the basin and its marginal seas can be classified into two types. The first type is that EKE is up to ~ 0.01 m2/s2 under the surface mixed layer and decays with depth. The second type is with maximum value at the subsurface of approximately 70–250 m between the upper and lower halocline boundaries.*

Figure 7: you state on line 318 that "EKE was significantly enhanced compared with that in the previous period" – are you just talking about the area in the Alaska box? This should be stated here. There are parts where EKE clearly decreases over time in the rest of the figure.
Thanks for pointing it out. This sentence was modified.
*(Line 337)*
*In every period, the EKE field along the southern continental slope was significantly enhanced compared with that in the previous period, and the strong EKE gradually developed from coasts to offshore regions and the central basin with time.*

Figure 8: Lines 340-355 say some incorrect things about the figure. For example, line 345 suggests SODA has been stable since 2010, but in the figure it reaches its highest points during strong variability from 2014 onwards, and this variability is comparable to the increase from the mid 1990s to 2010. Line 347 states EKE decreased in the two regions between 2010 and 2015, but it seems to increase in both SODA and altimetry, and in the case of SODA it is not "relatively weak" – it is higher than in all previous years before 2009.
Thanks for pointing it out. The associated texts were corrected.
*(Line 366-374)*
*Although the EKE from reanalysis is the highest estimate among them, it has also increased since the 1990s and remained at a relatively stable level in 2010–2013. In the BG region, subsurface EKE began to increase rapidly since 2003 and peaked in 2009, and it indicated a decrease until 2014, which was slightly different from that in the AL region. When EKE was relatively strong after increasing, its cumulative effect was contributing to plateauing of halocline depth and weakening of halocline steepness associated with APE release. These characteristic shifts of eddy and oceanic stratification were both related to the varying physics of the gyre in the upper layer that indicated a strengthening during the years before 2007 and a possible stabilisation since 2008 (Zhang et al., 2016). After experiencing a low ebb, especially from altimetry and MMP, since 2014/2015, EKE has presented some enhancements over time and remained at higher levels than in previous years before 2008 between the central BG and marginal AL regions.*

**Response to other revisions**

You discuss MKE in multiple places, and I asked why it was relevant. It would be useful to add another set of subplots showing the MKE, since it seems that the authors find the comparison with EKE important.
Thanks for this suggestion. A subplot of MKE was added in Fig. 7.

One of my comments asked for further justification that you are combining the mooring data, after focusing on how they are different. The new lines 335-339, which address this, should have a reference about which particular former research is relevant here. Additionally, "every mooring are thought equal to characterize the main features of eddy strength in the BG region" is not in itself a justification – do you have a reference for this? Combining the moorings and vertically averaging should be explained more here, in particular in how it affects the results. For example, 2013 and 2014 have very low EKE, but mainly have data from moorings A and D in them – is this a coincidence?

Thanks for this suggestion. The interpretation of combining mooring observation was added. From observations, the study on the long-term variability of EKE is few. The variability of eddy counts from MMP can well represent the variability in halocline eddies, for example, more eddies are detected in the southwestern sector of Canada Basin (Mooring A) in 2005, 2009 and 2013, which is well consistent with former research by ITP observations (Zhao et al., 2016). The results is verified in other sectors. Therefore, considering the difference in spatial distribution of eddy activity, we use horizontal velocities of all moored observations to quantify eddy strength. As shown by previous literature, eddy activity is most active in the southwestern sector of CB (Mooring A). Very low EKE in 2013 and 2014 can be supported by relatively fewer eddy days related to eddy duration.

> *(Line 355)*
>
> *As shown in the eddy detection from MMP, eddies are common in the upper and lower halocline of the CB (Zhao et al., 2014). The variability of eddy counts in four moorings are also consistent with former research by ITP observations in four sectors of the CB (Zhao et al., 2016), so EKE above the halocline base for different moorings is vertically averaged with depth to comprehensively characterize the main features of eddy strength over the years between 2003 and 2018.*

**Other comments**

Lines 63-67: time periods of changes of each variable should be provided, and the literature checked to ensure it is correct. For example, I believe Zhang et al (2016) discuss a stabilization rather than continued increase in some properties. The phrasing as it stands could suggest a continuous increase in all gyre properties over the last decades.

Thanks for this suggestion. We checked the related literature.

Line 236, 238: Be careful when talking about "lower" depth. Here it seems that you are using lower as deeper, but when you say "30m lower" it could mean "30m less". Prefer to use "deeper" or "shallower" to describe depth changes

Thanks for pointing it out. We checked related texts and used "deeper" or "shallower" to describe depth changes according to your recommendation.

Line 299: what is meant by "less valid observations"?

"less valid observations" means in third period there are fewer observations at mooring B (Fig. 5, thin grey lines). It was modified.

"before 2008" seems to mean different time periods in different parts of the manuscript. This

should be clarified everywhere, as otherwise it is hard to compare figures.
The years of "before 2008" were clarified in every figure for better comparison.

Line 347: it is strange to phrase a sentence as EKE lagging behind halocline changes, since I thought the point was to show how EKE was causing halocline changes. This should be clarified.
Thanks for your suggestion. This sentence was modified for explaining relationship between EKE and halocline.

> *(Line 362)*
> *When EKE was relatively strong after increasing, its cumulative effect was contributing to plateauing of halocline depth and weakening of halocline steepness associated with APE release.*

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