REVIEWER'S COMMENTS and ANSWERS

Reviewer #1

General comments

Q1: While the freshening event and the observational dataset are of clear interest, I find that the current analysis remains somewhat limited in depth, and that several conclusions appear stronger than what is fully supported by the presented evidence at this stage. In addition, some arguments would benefit from clarification or revision, as certain interpretations appear inconsistent or insufficiently justified.

A: We hope that our revisions have addressed the reviewer's concerns and improved the manuscript.

Q2: The overall quality of the figures is also a significant concern. Several figures lack axis labels, have readability issues, or are difficult to interpret in their current form. Substantial effort is needed to improve figure clarity and consistency before the manuscript can be considered publication-ready. In addition, some figures appear redundant and do not always add new information. For instance, Figures 2–7 show overlapping aspects of the same signal, Figure 8 is extremely difficult to read, Figure 12 adds limited insight, and Figure 15 duplicates information already shown in Figure 14. Some of these figures could likely be removed or moved to the Supplementary Materials.

A: We followed the specific advice regarding figures and addressed most comments. We also provide justification where suggested changes were not implemented. In particular, we (using the original figure numbering) (1) moved Figures 5 and 15 to the Supplementary Material, (2) added axis labels to Figures 7, 8, and 10, and (3) revised Figures 8, 10, 11, 12, and 14.

Q3: From a scientific perspective, the proposed link between river discharge and the observed freshening is likely, but several aspects of the observed signal remain unexplained and would benefit from discussion. In particular:

● What is happening in the surface layer, and is it possible that the anomaly was already present there in summer 2015?

A: We hope that we have presented a compelling argument that our records capture the major freshening signal.

● Why does the freshwater anomaly appear to arrive abruptly down to ~100 m at some moorings, while the meteoric water fraction increase is confined to the upper ~25 m (Fig. 8)?

A: We often observe signals arriving at mooring locations abruptly over an extended depth range. This likely reflects two factors: 1) the signal is often barotropic and 2) vertical mixing is enhanced along the continental slope.

The apparent discrepancy arises because salinity is a directly measured bulk property, whereas meteoric water (MW) fraction is a derived quantity based on end-member analysis (e.g., salinity, AW/PW contributions, and $\delta^{18}O$). As a result, MW estimates carry substantially larger uncertainties, typically on the order of ±0.01–0.02 in fractional units. While the freshwater anomaly remains detectable in salinity down to greater depths, the corresponding MW fraction becomes small relative to its uncertainty and is therefore not well resolved. In addition, vertical mixing and dilution reduce the distinct meteoric water signature with depth, even when the total freshwater anomaly persists. Consequently, MW increases appear confined to the upper ~25 m, where the signal-to-noise ratio is highest. The positive MW anomalies nevertheless indicate an enhanced riverine contribution to Arctic surface waters, concentrated primarily within the upper ~25 m. We now emphasize this uncertainty more clearly in the text.

● Why does the anomaly appear weaker in summer 2016 compared to the preceding and following winters?

A: We believe that this is due to the seasonal signal. Although we de-seasoned our records, a residual seasonal signal likely remains. This is because the seasonal cycle varies from year to year, and subtracting a mean seasonal climatology does not fully remove it. In addition, seasonality is expressed not only in the mean but also in the variance (e.g., Arctic air temperature exhibits much stronger variability in winter than in summer). This component is not removed by traditional de-seasoning methods. We have added these arguments to the Methods section.

● Why is the freshwater anomaly associated with different (and sometimes opposing) temperature signals at different moorings?

A: We addressed this reviewer's question in our responses to their specific comments. Briefly, this arises from the contrasting thermal regimes of the shallow and deeper parts of the continental slope, which are separated by a hydrographic front.

While definitive answers to all of these questions may not be possible, acknowledging and discussing these features would improve the physical interpretation of the results.

A: We hope that the reviewer finds our responses satisfactory.

Q4: The discussion of the impacts of the freshening event would also benefit from clarification. In particular, the link between freshening and changes in surface currents is not entirely clear, and it is difficult to assess whether the freshening alone can explain the increased sea-ice cover observed in summers 2016 and 2017, given the potential influence of other factors.

A: We added a clarification to the text regarding the connection between stratification and current speed (see also our response to specific comments). Regarding the relationship between stratification and sea-ice state, the conclusions of this study are highly consistent with those of Polyakov et al. (2025). We have therefore added this reference to the manuscript.

Q5: Finally, some aspects of the analysis are unclear. For example, the calculation of $\bar{V}.\Delta S$ does not seem to provide information about a potential cross-slope shift of the Atlantic Water core, as it compares a mean-state salt flux with a temporal salinity anomaly. This interpretation likely needs to be reconsidered.

A: We have revised the text and hope it is now clearer. Please see our responses to the specific questions below.

Specific comments:

l. 32–34: "*exemplifying the phenomenon of Arctic amplification*"

The logical connection in this sentence is unclear. The numbers in the paragraph show that the Arctic is warming, but they do not illustrate Arctic amplification.

A: We edited the sentence removing the part related to amplification.

l. 47–53: Consider moving this to l. 38, where you introduce the presence of freshwater in the Arctic.

A: We prefer to keep this paragraph as is. The preceding discussion addresses freshwater changes across the Arctic, whereas this paragraph focuses on the Siberian Arctic.

l. 90–93: "*indicating that interpolation of SBE data introduces negligible error*."
You have not yet mentioned that the data are interpolated. I suggest moving this sentence to the end of the subsection.

A: We revised the sentence to remove referencing to interpolation.

l. 149: Is this wavelet-based method more accurate than simply analyzing anomalies relative to the seasonal cycle?

A: To calculate anomalies, it is necessary to define transition points between positive and negative phases. In the presence of noise, this is not straightforward; wavelet analysis provides a more objective way to identify these transitions.

l. 159–170: I would shorten this paragraph: simply define zl and z2 , and state that using the depth at which the anomaly becomes statistically indistinguishable from zero would yield similar results.

A: We have shortened this paragraph.

Definitions of freshwater content, Q, and APE: Is z1 the surface or the depth of the shallowest reliable salinity record zl defined earlier? You state that it is the surface for APE, but this is not clear for freshwater content and Q.

A: z_1 is the shallowest depth with available data (or as otherwise specified) and is adjusted to be consistent with the definition of APE.

Analysis of Figure 2: The abrupt arrival of the fresh anomaly at moorings M14 and M3 in January 2016 is intriguing. The anomaly then weakens during the following summer before strengthening again in winter 2016–2017. This variability is also visible in Figures 4, 5 and 6. Why do the anomalies appear stronger during the two winters? Is it because part of the freshwater anomaly resides closer to the surface in summer (in a layer not resolved by the moorings) and is transferred to deeper layers by winter mixing? Or is there another explanation?

A: This signature is seen in all records (see Fig. 5). It is a fingerprint of the seasonal cycle. Freshening is due to mixing which makes water saltier in the SML and fresher in the halocline. This is a very well-known feature. It is discussed in the text.

l. 286: "*For instance, the freshwater content averaged over four deeper moorings increased* *by 0.67 m*."

The statement that freshwater content increased by 0.67 m when averaged over four deeper moorings is difficult to interpret meaningfully, given surface-layer limitations (see also l. 330–332).

A: In winter, the absence of data in the upper 30–50 m is not critical because the layer is well mixed. In summer, however, missing near-surface data may be more important. To assess this effect, we use Ice-Tethered Profiler (ITP) data extending to 10 m and simulate the removal of data from the upper 30 m and 50 m layers. The impact on freshwater content (FWC) estimates is limited (Fig. S2 using new numbering). For example, removing data above 30 m—available at three of the six moorings during the freshening event—changes the annual mean FWC (10–175 m) by only 2.9%, increasing to ~10% when the upper 50 m is excluded.

Figures 3 and 4: There is some redundancy between these figures; choosing one would likely be sufficient. The qualitative results (lower salinity corresponding to larger freshwater content and higher APE) are obvious, and quantitative estimates are not possible because the moorings do not cover the surface layer.

A: We consider both Figures 3 and 4 essential, as they depict different water properties and together help build the overall narrative. We therefore prefer to retain both. It is possible that the reviewer was instead referring to redundancy between Figures 4 and 5; in that case, we agree and have moved Figure 5 (APE time series) to the Supplement.

We disagree with the reviewer's statement that quantitative estimates are not possible using the available data. First, for vertically integrated quantities such as FWC, we provide quantitative estimates for specified depth ranges. Although these do not cover the upper ocean to the very surface, they still represent robust measures of freshening. Second, as shown in our response to the previous comment, these estimates are not substantially different from those obtained when data extend to 10 m depth. Third, the strong consistency of FWC anomalies across different mooring records—including those with observations extending to 30–40 m and shallower—demonstrates that even records lacking near-surface data provide meaningful estimates of freshwater anomalies during 2015–2017.

l. 281–288: The contrasting tendencies in temperature and OHC between the moorings are difficult to interpret. At mooring M11, for instance, the average anomalies have opposite signs (positive temperature anomaly and negative salinity anomaly), yet temperature (or OHC) and salinity (or −FWC) appear correlated during 2015–2017. Do you have an explanation for this?

You also state that "*The potential underlying mechanisms for these contrasting signals are discussed in Section 6*," but there is no Section 6 in the manuscript. Such a discussion would improve the interpretation of the results.

A: We revised the text to clarify that these contrasting regional differences should be considered in the context of the hydrographic front, located near the ~750 m isobath, which separates the shallow $M1_1$ and $M1_2$ moorings from the deeper ones. The shallower and deeper domains exhibit markedly different thermal regimes (e.g., Baumann et al., 2018).

Figure 7: The figure is difficult to read and would benefit from improved resolution.

A: The original figure is very clear and readable, and we expect it will remain clear upon publication.

l. 297–310: This analysis is unclear to me. As I understand it, $\nabla.\Delta S$ is calculated from time-averaged velocity and salinity at each mooring location, representing one component of the mean salt balance. You then compare it with salinity anomalies during the freshening event, which represent temporal changes. I therefore do not see why these two quantities should be related. One possible alternative might be to examine changes in $\nabla.\Delta S$ between the pre-2015 period and the freshening event.

A: In the "normal" state (with no freshening), the cross-slope salinity distribution exhibits a maximum at mooring $M1_3$. Displacement of this maximum in either direction (closer to the shore or toward the deeper ocean), following the meridional shift of the AW jet, would produce salinification at some mooring locations, which contradicts our observations in 2015–2017. To illustrate this more formally, we assumed that near-slope salinity changes due to the cross-slope (meridional) migration of the AW jet can be described by a reduced salinity conservation equation, in which we retain only two terms: the temporal change of S and the meridional advection of salt. Under this framework, freshening (negative $\partial S/\partial t$) should be balanced by meridional advection of the corresponding sign. If this balance is violated—as indicated by our estimates—the observed freshening cannot be explained by a cross-slope shift of the salty AW core.

To make our analysis clearer, we edited this section.

Table S2: Please add units to the column headers, or remove the table if you agree with my previous comment on this calculation.

A: The original table caption included units, but the unit for the advective term was inadvertently omitted and has now been added. These estimates help clarify the potential role of cross-slope meandering in shaping the observed freshening event.

Figure 8: This figure is difficult to read. It would benefit from improved resolution, enlarged caption, and axis titles and units. Also, it would be good to specify the months over which these profiles were computed. This may be important for your analysis if the increase in meteoric water fraction was already present in summer 2015.

From what I can see, the main changes in meteoric water occur in the upper 25 m, which are not covered by the moorings. It would be useful to propose an explanation. Could this initially be a surface anomaly starting in summer 2015 that is not detected by the moorings and is later transferred to deeper layers by winter mixing?

A: We edited this figure and its caption.

Yes, this is our interpretation: the anomaly, originating from anomalous riverine discharge, forms initially as a thin surface layer near river mouths. It is then advected toward the mooring locations as a shallow fresh layer, where it is subsequently mixed into the ocean interior. Because five of the six mooring records used in this study begin within the upper 50–55 m during the freshening event (49 m at $M1_1$, 31 m at $M1_2$, 65 m at $M1_3$, 38 m at $M1_4$, 41 m at $M1_5$, and 30 m at M3), they capture most of the freshening signal, as shown in the new Figure S2, using new numbering.

l. 326: It is not clear from the figure itself that Yenisey discharge was lower in 2013 than in subsequent years, the calculations provided at l. 327-329 are necessary to say that.

A: We edited this paragraph stating that although the Yenisey exhibits a higher *instantaneous* peak in 2013, this peak is short-lived. In contrast, the 2014–2015 discharge is sustained over a longer period, resulting in a larger *integrated* runoff.

l. 327–330: Are these anomalies calculated relative to 2013 only? Why not use the 2013–2018 average as a baseline instead? Also, it is not clear at this stage of the article why the Lena River is not discussed, while it is also shown in Figure 9.

A: We aimed to illustrate the anomalous state of the system during the freshening event relative to its undisturbed state, with 2013 representing the baseline year before the freshening occurred. We also added a discussion of Lena River discharge to this paragraph.

l. 330–332: The estimate of 0.60 m of additional freshwater content is difficult to interpret, given both spatial variability among moorings and incomplete vertical coverage. Besides, summing the anomalous discharges from 2013 and 2014 implicitly assumes that freshwater from both years arrives at the mooring line simultaneously, which may not be the case.

A: The reviewer is correct, and we have revised the sentence accordingly.

l. 342: "*The downstream impact of this anomalous freshwater input is evident…*"
The downstream propagation of a single freshwater anomaly is not obvious from the presented maps, which show a mix of local and basin-scale patterns, including local freshening near river discharge regions and broader large-scale anomalies extending into the Eurasian Basin. I would suggest either showing only one or two maps illustrating the spatial extent of the anomaly, or presenting a lagged regression map of surface salinity anomalies onto Yenisey and Ob discharge anomalies.

A: We have retained a single map to illustrate the spatial pattern of the freshening event and have revised the text accordingly.

Figure 11: This figure is useful for illustrating the link between river discharge and the freshening anomaly. You state that all correlations are significant at $p<0.001$. What statistical test was used, and how were the degrees of freedom estimated? I would expect the correlation with M11 ($R = 0.39$) to be significant at $p<0.05$, but probably not much more.

A: The t-statistic from the Student's t-distribution is used for these estimates. For the 5yr record, the *p*-value just slightly above 0.001. We corrected the p value from "<0.001" to "<0.05" in this panel. Thank you for catching this.

Figure 12: Captions and titles are cropped, and overall presentation needs improvement.

A: We have replaced this figure with a new version.

l. 365–372: This explanation is unclear. Winds during the event appear to drive Ekman transport that is along-slope or even toward the coast, whereas the moorings are located in the basin interior. Such winds would be expected to limit freshwater transport toward the moorings. While it is likely that winds influenced shelf–basin freshwater exchange, it might be more appropriate to focus on wind patterns during the months surrounding the onset of the event (summer–fall 2015) rather than over 2015–2017 as a whole.

A: As advised, we have replaced the original figure with a revised version and updated the text accordingly. The new figure illustrates the wind pattern during the onset of the freshening event, which is conducive to the eastward transport of freshwater from the Kara Sea into the Laptev Sea.

l. 381–382: "*the resulting trajectories reveal that upper eastern Eurasian Basin freshening originates in the Kara Sea*." This argument should appear much earlier. All analyses from Figure 9 onward rely on the assumption that the anomaly originates in the Kara Sea.

A: We added an introductory statement at the beginning of Section 3.2.2 to clearly state that the subsequent analyses (from Figure 9 onward) are based on the premise that the freshening originates in the Kara Sea.

Figure 14: The font, style, and overall appearance differ from the other figures. Please ensure a consistent visual style. The time-axis ticks in panel (a) are not visible.

A: We appreciate the reviewer's comment. The figure was produced using standard, widely accepted software, and we note that the choice of software does not influence the scientific content or conclusions. As such, we have not modified the figure. However, we added external ticks to time axes.

l. 395–397: Are you suggesting that the freshening caused the decrease in surface currents? If so, what physical mechanism would explain this?

A: These results do not represent a suggestion of current decrease on our part; rather, they are based on mooring observations showing a reduction in measured surface currents. We interpret this as being associated with a strengthening of upper-ocean stratification, which likely inhibited the downward propagation of wind energy into the ocean interior, confining the response to a very thin (<10m) near-surface layer. Unfortunately, due to surface reflection and associated contamination, ADCP-based measurements are not reliable within this thin surface layer. We added a clarification to the text.

l. 403–405: "*the divergent heat flux across the halocline decreased from 20 W/m² to 3 W/m²*."

Where do these values come from? Are they taken from Polyakov et al. (2020b)? Do they apply to a specific mooring or represent an average?

A: These values are taken from Polyakov et al. (2020b) and are based on the M3, $M1_2$, $M1_3$, and $M1_4$ mooring records, representing a multi-mooring estimate of the divergent heat flux across the halocline. We have revised the text accordingly.

l. 408–414: You argue that the freshening led to increased sea-ice cover in summers 2016 and 2017, but Figures 2, 4, 5, and 7 suggest that the freshening event ended in spring 2017.

It is therefore not obvious how it would affect sea ice during the following summer. Is this due to delayed effects of reduced winter mixing in 2016–2017? In any case, I would moderate the strength of the claimed link.

A: The reviewer is correct that there is a delayed sea-ice response to changes in stratification, mixing, and winter sea-ice formation, with impacts becoming evident as reduced sea-ice extent in the Siberian Arctic the following fall. This is consistent with a positive ocean heat–ice albedo feedback (e.g., Polyakov et al., *2025*). We have moderated the statement accordingly.

Figure 15: This figure does not add new information (the SIC time series at M1 already appears in Figure 14). I suggest removing it.

A: This figure shows that this process occurred not only at a single mooring site but across the broader region, as indicated by sea-ice records from all mooring locations. We have moved this figure to the Supplement.

l. 435: Why describe this event as "extreme"? It is clearly significant, but no evidence is provided that such events are rare.

A: We removed this word from the sentence.

l. 440–441: "*the observed freshening cannot be explained by cross-slope shifts of the AW salty and warm core*."

The demonstration of this point is unclear to me.

A: We hope that our clarifications have made this point clear. We reiterate that when a salinity maximum is located over the middle part of the slope, uniformly northward currents cannot produce spatially uniform freshening, as a shift of the Atlantic Water core would instead lead to salinification elsewhere.

l. 443: "*exceptional increase*."

The data show an increase, but the time series are too short to justify the term "exceptional."

A: We moderated the tone of this statement.

l. 446–448: The timing is not fully consistent: if Kara Sea anomalies in spring 2015 take nearly two years to reach offshore moorings, they would arrive in spring 2017, which you identify as the end of the event.

A: The timing estimates based on lagged correlations (Fig. 11 in the original notation) are not sufficiently precise to accurately determine the arrival of the fresh anomaly from the Kara Sea to the Laptev Sea, as evidenced by the broad correlation peaks. We have added a statement to this effect in Section 3.2.2 and revised the Discussion accordingly. Moreover, the lag corresponding to the maximum correlation does not indicate the onset of the process; rather, it represents the time at which the freshening signal from the Kara Sea reaches its peak expression in the Laptev Sea.

l. 452–453: "… which aligns with the varying start dates of freshening observed at the moorings (Fig. 3)." I would nuance this statement. Differences in the onset of the freshening event among moorings are on the order of 3–4 months, not more than a year.

A: We edited this text.

l. 456–457: “Indeed, Polyakov et al., (2020b) showed that the divergent heat flux…”
This was already mentioned and is a result from another paper. I suggest removing it.
A: We removed this sentence.

l. 485–487: “Consequently, the maximum freshening that often resides in the very top layer cannot be monitored, and the overall magnitude of freshening may be underestimated by the available mooring records.”
This is an important limitation. Given it, I would place less emphasis on quantitative estimates of freshwater content and APE. An alternative approach could be to analyze the freshening in the ORAS5 reanalysis to estimate the fraction of the anomaly captured by the moorings versus that residing near the surface.

A: We hope that our additional analysis in Methods clarified the impact of this limitation. We modified this part of discussion.

Technical corrections:

l. 206: Remove one parenthesis.
A: Done. Thank you.

Figure 2: The caption does not fully correspond to the figure. In particular, there is a confusion between solid and dashed red lines and between red and black lines delimiting the event and the preceding and following periods. The black dashed lines separating the different years could also be mentioned.
A: The caption was edited.

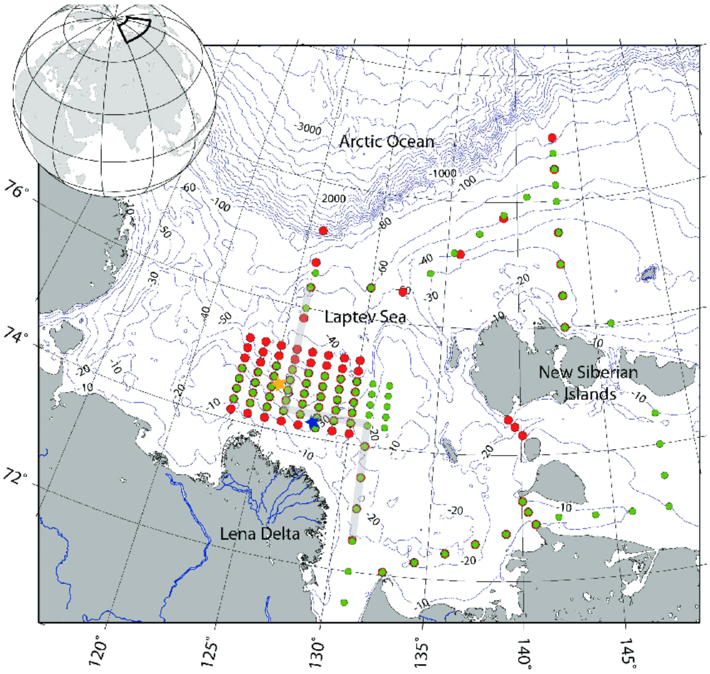
l. 403: Remove parentheses around the reference.
A: Done, thank you.

Reviewer #2

General comments

C1. My primary comment relates to the simulated trajectories used to support the proposed origin of the freshwater anomaly. It is well known that different ocean models can produce substantially different spreading pathways for freshwater and water masses in the Arctic. As I am not a modelling specialist, I cannot confidently assess the reliability of the specific simulations presented here. However, the authors themselves note (Line #57) that numerical models often struggle to accurately reproduce freshwater redistribution. A key limitation of the trajectory figure is the absence of information about flow pathways at different depths. The observed freshening signal is clearly detectable at least within the upper ~150 m. Therefore, it would be highly informative to show simulated trajectories at several representative depths (e.g., 50, 100, and 150 m), rather than only at the surface (0–5 m). In particular, do the modeled pathways at these depths indicate transport through Vilkitsky Strait? If not, and if the simulations instead suggest that water at these levels primarily arrives from the northwest along the continental slope, this would weaken the argument that the entire freshening signal originates from Kara Sea river discharge. In that case, the key conclusion stated in Line #19 of the Abstract may require qualification. At present, the anomaly is traced in the surface 0–5 m layer only, whereas the origin of the anomalies observed at greater depths may have been different.

A: Extensive mooring and shipborne observations and reanalyses data used in our study suggest that salinity anomalies are surface-intensified and originate from Siberian River mouths. As a result, anomalies associated with anomalous riverine discharge initially form as a thin surface layer near the river mouths. They are then advected toward the mooring locations as a shallow freshwater layer, where they are subsequently mixed into the ocean interior. As the attached map (sourced from Google, ignore color dots, see depths near Vilkitsky Strait) illustrates, the shallow bathymetry limits vertical mixing to greater depths along most of the pathway, such that significant deepening of the signal is unlikely until it approaches the continental slope and the mooring sites. Therefore, in the original analysis, we performed Lagrangian tracer simulations for the surface layer (0–5 m).



However, following the reviewer's suggestion, we conducted additional tracer simulations for layers deeper than 50 m. These simulations (not shown in the revised text) suggest that tracers are predominantly advected along the continental slope, from the areas west of the eastern Eurasian Basin with climatologically much higher salinities, highlighting the essential role of waters of the Atlantic origin in shaping water structure at the intermediate layers. Thus, we think that our conclusion regarding the origin of the observed freshening is robust.

C2. I also share the concerns raised by the other reviewer regarding the analysis intended to demonstrate a potential cross-slope shift of the Atlantic Water core. In its current form, this approach is not sufficiently convincing. First, the method does not account for the vertical nonlinearity of either salinity or current velocity, both of which may significantly affect the resulting estimates. To obtain physically meaningful values, the quantity $V\Delta S$ should be calculated over smaller vertical intervals, and only thereafter integrated to derive a depth-averaged effect. Averaging first and integrating afterward may obscure important vertical structure and lead to misleading conclusions. Second, I believe there is an issue in the interpretation of the calculated values presented in Table S2. The table provides estimates of $V\Delta S$ between adjacent mooring pairs. However, these values represent the lateral salt flux between the moorings, not the divergence or convergence of salt flux at a given mooring location. For example, to determine whether advection-driven freshening or salinification occurs at mooring M12, one would need to compute the difference between the fluxes across M11–M12 and M12–M13 (−0.32 and +0.25, respectively). This difference would indicate the sign of the expected salinity tendency. However, such an analysis is not feasible for the boundary moorings (M11 and M15), as there is no information on lateral salt flux outside the array. Consequently, convergence or divergence cannot be estimated at the array edges. In my view, this part of the analysis currently introduces more ambiguity than robust insight. I would therefore strongly recommend either substantially revising this section with a physically consistent framework or considering its removal. In its present form, the approach does not allow one to assess whether lateral shifts occurred upstream at different depths, nor how such potential shifts might have contributed to the observed salinity anomalies.

A: We have revised this section to address the reviewer's concern. In the revised text, we first provide a holistic description of our approach, followed by a more formalized formulation. In response to the concern that the method neglects the vertical structure of currents, salinity, and fluxes, we note that the analysis is intentionally highly simplified. This is motivated by our objective, which is not to quantify the exact contribution of individual terms to potential cross-slope shifts in the AW jet, but rather to assess whether this mechanism could plausibly explain the observed widespread freshening in the eastern Eurasian Basin in 2015–2016. We therefore consider the chosen approach appropriate for this purpose. At the same time, we agree that robust estimates require considering flux divergence rather than fluxes alone. We have accordingly updated Table S2 and revised the text to base the analysis on flux divergence. Thank you for this helpful suggestion.

C3. Figure 11 is arguably one of the strongest figures in the manuscript. However, the reported significance level of the correlation coefficients ($p < 0.001$) appears unexpectedly high. I encourage the authors to carefully verify these calculations. Such a strong level of statistical confidence warrants explicit clarification. In addition, panel (b) does not seem essential to this figure. The rectangle indicating the Kara Sea region could instead be incorporated into Figure 1, which would improve the overall clarity and reduce redundancy.

A: We have identified an error in the estimation of the p-value for the top panel ($M1_1$ mooring) and corrected it. For the given length of the monthly time series and the reported correlation values, all other p-values are correct. We have made changes adding the rectangle in Fig. 1.

C4. I also find the proposed impact of oceanic heat flux on sea ice concentration (SIC) to be insufficiently substantiated. The correlations shown in Figure 14b and 14c appear to primarily reflect the seasonal cycle, rather than demonstrating a robust physical relationship between SIC and velocity shear. I refer the authors to my detailed comment on Line #403 for further elaboration.

A: See our answer to the Reviewer's specific comment (l. 403).

Despite the criticisms outlined above, I view this as a high-quality and potentially impactful contribution that is suitable for publication in Ocean Science. The manuscript is generally well organized, presents a unique and valuable observational dataset, and develops several convincing lines of evidence. Nevertheless, there remain a number of issues that require clarification or revision to strengthen the robustness and interpretation of the results. Below, I provide a detailed list of specific comments and suggestions.

A: We thank the reviewer for his time and effort in evaluating our manuscript. We hope that the revised version addresses most (if not all) of the reviewer's concerns.

Specific comments:

Q1. Line #68:
All river-driven freshenings are seasonally episodic by nature. It would therefore be more appropriate to refer here to an anomalous freshening event rather than implying episodicity as a distinguishing characteristic.

A: Valid point. We have corrected this to "anomalous."

Q2. Line #87 (Table 1):
There are several formatting inconsistencies and potential errors in Table 1 that should be carefully checked.

- Some dates contain odd empty spaces (e.g., "26 .08. 2013").
- M12a lists "70–754" (MMP?), but according to Figure 2 these data appear to be unavailable.
- M11a and M11b are shown with identical SBE37 depths, whereas Figure 2 suggests that the shallowest instrument at M11a was deployed deeper.
- The coordinates of M15a and M15b are identical to the last digit, which may indicate a mistake.

In addition, the MMP on M15 is reported to have profiled between 88–754 m and 172–806 m. However, Figure 2 shows data availability beginning at approximately 50 m and 45 m. Based on Figure S3, it appears that during the first deployment the MMP started from at least ~77 m (not 88 m as indicated in the table), and during the second deployment from approximately ~185 m (rather than 172 m). Figure S3 also suggests that a MicroCAT may have been deployed at ~77 m during the second deployment.
I strongly recommend carefully verifying all information presented in this table and ensuring full consistency with the data shown in the figures.

A: We have corrected the table: all spacing issues in the dates have been removed, several instruments that were missing in the original version have been added, and deployment depths have been corrected. In particular, the $M1_5$ mooring information previously omitted two SBE37 instruments, which explains the reviewer's question.

Q3. Line #90-93:
This statement may be valid for the MMP profiles specifically, but it does not appear to hold for the discrete sensor measurements at other moorings. Based on Figure S3, I have significant doubts that the vertical sampling configuration at moorings such as M11 and M14 would result in only a ~1% error. The vertical distance between the shallowest two loggers at these sites is substantial and could lead to considerable under- or overestimation of freshwater content if linearly interpolated. The authors should clarify how that small potential error could be transferred to the moorings with measurements at discrete and sparse levels.
A: We have removed this statement from the text, as it could not be independently verified.

Q4. Line #94:
It would be useful to include information on ADCP instrument depths and measurement ranges in Table 1 to improve clarity and allow reproducibility.

A: We added this information.

Q5. Line #96: "*MMPs profiled from ~40*"
This statement is inconsistent with the data presented in Table 1 and should be corrected.

A: We corrected this sentence.

Q6. Lines #149-153:
Using wavelet analysis to extract and remove the seasonal signal does not appear to work consistently, as seen in Figure S1. In many locations, the seasonal signal appears artificial and potentially misleading. Conceptually, the seasonal component should correspond to a 12-month period. While this periodicity is reasonably clear at moorings M11, M12, M13, and M3, it is not evident at the other two moorings.

I suggest the authors reconsider the terminology used to describe these anomalies. Rather than labeling them strictly as "seasonal-adjusted" or "anomalies," it may be more accurate to describe them as the high-pass component of salinity, with the low-frequency (periods ≥12 months) signal removed via wavelet decomposition. My concern is primarily terminological rather than methodological. Additionally, I note that the red line in the upper panel of Figure S1 is absent before 2015.

A: We tend to disagree with the assertion that wavelets identify an "artificial" seasonal signal. Rather, we interpret the seasonal signal as inherently variable in time and space (as the reviewer also noted). In our experience, wavelet analysis is a powerful tool for decomposing time series into their constituent components. We also disagree that the method applied here constitutes low-pass filtering; instead, it is more appropriately described as a band-pass filtering approach. However, we agree that the original sentence (ll. 151–153) was not sufficiently accurate and have revised it accordingly. We also added a red line to highlight the early part of the M11 record.

Q7. Line #179:
In the integral, dz represents the differential element of depth, not the layer thickness. It would be clearer to simply state that z is the vertical coordinate.

A: We have removed this statement regarding the definition of dz, as it was not necessary.

Q8. Line #187: "*Here, Q can be defined as the relative heat content, which measures the amount of heat that must be removed to create ice crystals at a given salinity and pressure.*"
It would be more accurate to define Q as the heat that must be removed to lower the water temperature in the entire layer to its freezing point at a given salinity and pressure. The phrase "to create ice crystals" implies removing additional latent heat beyond this amount. For consistency with the freshwater content (FWC) formula, consider adding a subscript *z* to *T* (and possibly ρ) in the ocean heat content calculation, and replace the subscript *i* with *z* in the APE formula.

A: The sentence is edited.

Q9. Line #210:
It would be helpful to clarify in the text that the Kara Sea data are derived from the model, while the Laptev Sea data come from the mooring array, to avoid potential confusion.

A: Added.

Q10. Line #228 and Figure 2: "*... and the freshening extended to depths up to 175 m ...*"
The statement that the freshening extended to depths of up to 175 m requires clarification. During the early-2016 surface freshening, MMP measurements indicate that the anomaly was mainly confined to the upper 60 m at M15 and 70–80 m at M13. On other moorings, the apparent deeper extension (~110 m at M11, ~120 m at M12, ~100 m at M14) results from the discrete depth levels of the sensors (53 & 140 m, 67 & 138 m, 38 & 107 m, respectively) and the linear interpolation between them.
Figure S3 illustrates this effect clearly: interpolation can artificially extend the freshening signal to greater depths, particularly at M11, M14, M15 (where MMP started at 188 m), and M3. This is not a critical problem, but the text should at least mention these potential artifacts. In particular, it should be noted that the calculated freshwater content (FWC) carries additional uncertainty at moorings with sparsely spaced MicroCATs. Therefore, the previously stated maximum error of 1% may not apply to these sites.

A: We agree and revised this paragraph.

Q11. Line #229:
It would be clearer to write "*evolution of the spatial (or cross-slope) pattern of salinity and temperature anomalies*" to better convey the intended meaning.

A: We edited the sentence.

Q12. Line #237: "*strengthening the vertical density gradient.*"
It is preferable to use either "*strengthening the vertical density stratification*" or "*increasing the vertical density gradient*".

A: We edited the sentence.

Q13. Line #238: "*This enhanced stratification was reflected in the pronounced increase in estimated available potential energy across all moorings*"
There appears to be a conceptual issue here. Based on the formal definition, APE is the portion of the total potential energy of a stratified fluid that can be converted into kinetic energy. It represents the energy associated with deviations of density surfaces from a state of minimum potential energy. Critically, the correct calculation of APE requires a reference density profile corresponding to the minimum potential energy, which is obtained by adiabatically sorting all vertical layers by density. In this framework, APE is zero when there are no vertical density inversions. It definitely does not use a constant reference density in the formula!
From the description in the manuscript, it appears that the formula currently used may not represent true APE. Instead, it seems to calculate the energy required to mix a given water column into a fully uniform state with some mean (reference) density. This distinction is important, as the two quantities represent fundamentally different physical processes.
The authors may wish to clarify this point and, if necessary, revise the text and formula to accurately reflect the concept of APE.

A: We thank the reviewer for this important clarification. We agree that the formal definition of available potential energy (APE) requires the construction of a reference state obtained by adiabatic sorting of the density field, as originally formulated by Lorenz (1955). In the present study, we do not compute this full APE. Instead, we use a linearized diagnostic metric based on density anomalies to characterize changes in stratification strength. This metric is proportional to the small-amplitude approximation of APE under the Boussinesq assumption and constant background stratification, and therefore should be interpreted as a proxy for stratification energy rather than a rigorous estimate of Lorenz APE. We have clarified this distinction in the revised manuscript.

Q14. Line #259 (Figure 3):
I have a concern regarding the placement of M3 in this plot. Although M3 is slightly closer to M15, it may be more intuitive to position it on the left side of the figure. From both a topographic perspective and considering the general flow path along the slope, it would visually align more naturally with M11. Doing so would likely reduce the apparent contrast in temperature and salinity between M3 and M11, which currently appears stronger than the contrast between M3 and M15.

A: Our rationale was to (1) show all cross-slope moorings $M1_1$–$M1_5$ without interruption, (2) include the M3 records, and (3) place them as close as possible to the most comparable cross-slope moorings ($M1_3$ and $M1_4$, given their similar depths). It is evident that the M3 signal shares many similarities with those observed at these two moorings. Therefore, we prefer to keep the figure unchanged.

Q15. Line #273 (Figure 6):
The caption does not specify the depth range over which the mean salinity was calculated, and this information is also absent from the main text (Lines 245–247). Although it can be inferred from the winter panels, it would improve clarity to explicitly state the depth range in the text. Additionally, it is unclear why the summer and winter data are presented separately. Would it not be clearer to show them as bars (with the same altering seasonal colors) on a single panel for each mooring, allowing direct seasonal comparison?

A: The depth ranges were presented in the original figure. The requested changes have been implemented.

Q16. Line #278:
"*The (negative?) salinity anomalies...*"

A: Yes, thank you – we have added this word.

Q17. Line #288: "The potential underlying mechanisms for these contrasting signals are discussed in Section 6."
There is no Section 6 in the manuscript. Additionally, it is unclear how ocean heat content was applied or interpreted in the context of this study. Is it worth removing OHC part at all?

A: We edited this sentence.

Q18. Line #309: "*Thus, the 2015-2017 freshening cannot be explained by the advective cross-slope shift of the salty jet's core.*"
See my general comment at the beginning regarding the validity of the approach used to assess cross-slope shifts.

A: See our answer and edits.

Q19. Line #324: "*3.2.2 Kara Sea as a driver of salinity change*"
A geographical entity such as a sea cannot act as a "driver" of change. It would be more precise to refer to the Kara Sea riverine discharge as the driver of salinity variations.

A: We revised the title.

Q20. Line #325: "*we indeed found that the Yenisey and Ob rivers showed an anomalously high runoff peaking between 2014 and 2015 relative to 2013*"
It is difficult to draw robust conclusions based solely on the plotted daily series. The seasonal peaks in the figure may represent single-day maxima rather than the high total (cumulative) discharge for the season. I recommend adding the mean seasonal discharges for each summer (as a text) above the timeseries in Figure 9 to provide a clearer quantitative context.

A: We agree that it is difficult to fully discern the entire story from this plot alone. Therefore, we have revised the text to make the interpretation as clear as possible and to provide sufficient detail for understanding.

Q21. Line #327: "*when spread over the area covered by mooring observations (roughly 77-82°N and 110-140°E; 5 x 105 km2)*"
This appears to be a very strong assumption. The authors should justify why this specific region

accurately represents the domain over which Kara Sea riverine water becomes eventually distributed. The actual affected area could easily be larger or smaller, and the estimated freshening in meters would strongly depend on this choice.

A: To clarify this point, we have revised the text to explicitly state that the region is only loosely defined and is used solely as a representative area for estimating freshwater distribution. This clarification has been added in the revised manuscript.

Q22. Line #327-330:
There appears to be a minor arithmetic or rounding error. The sum of 0.19, 0.16, 0.14, and 0.26 cannot equal 0.78 under any reasonable rounding scheme. Even allowing for maximum trimmed decimals of 0.0049(9) for each member, the total would not exceed approximately +0.02 (that would give 0.77), not +0.03 (for having 0.78). I know this is not a big issue, but it caught my eye.

A: We edited this paragraph.

Q23. Line #349 (Figure 10):
It is unclear whether the presented anomalies are temporal or spatial. I attempted to follow the tracks shown in Figure 13 to trace the modeled spread of freshwater anomalies, but this proved difficult, likely due to the different color scales used in each panel. While varying scales can help highlight distributions within individual panels, they make it challenging to track the evolution and movement of anomalies across panels. Additionally, the zero value corresponds to different colors in each panel, further complicating careful interpretation. Overall, I am not certain that these modeled anomalies add substantial insight to the narrative as currently presented.

A: Following the advice of Reviewer #1, we have removed all but one panel from this figure and modified the text accordingly.

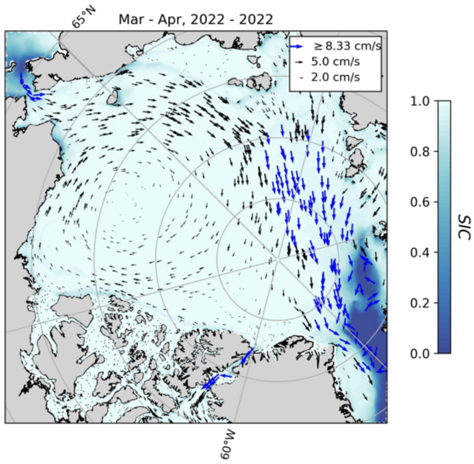
Q24. Line #360 (Figure 11):
For M11, the maximum correlation was reported at a 7-month lag, whereas the graph appears to show a maximum around 22 months, similar to M13–M15 and M3. The authors should clarify why the 7-month lag was chosen. For M12, there is a relatively flat plateau between 9 and 23 months, indicating some uncertainty. I recommend adding confidence intervals (e.g., shaded areas) to each correlation panel to better convey this uncertainty. Overall, it would strengthen the figure and interpretation to acknowledge that a lag of ~22–23 months likely applies across all mooring records.
A minor point: there is a small inconsistency between the text (Line #354, "*all statistically significant at 95% level*") and the figure (significance level $p<0.001$). If all correlations indeed meet the $p < 0.001$ threshold, it is acceptable to state 99.9% confidence level in the text.

A: We

Q25. Line #368: "*These changes can alter the direction of Ekman transport, shifting the accumulation of freshwater along the slope rather than driving it into the basin and contributing to the observed freshening buildup.*"
I suggest adding a comma after "*basin*" for improved readability.

A: We have edited this sentence.

Q26. Line #374 (Figure 12):
Please check the ordering and layout of all labels, as some text is currently obscured by overlapping graphics. While not critical, I also recommend improving the figure style. Instead of plotting vectors on a regular spherical grid, consider weight-randomizing their positions with weighting each vector according to the actual area of the corresponding 0.25 × 0.25° ERA5 cell. This approach is relatively straightforward to implement and can produce much cleaner and more visually informative plots (e.g. like an example below)



A: We have corrected labels in this figure. The reviewer's suggestion may be important for regions close to the pole; however, we find that the presentation of vectors in our domain using a spherical grid remains clear and informative and is not affected by the convergence of meridians. We therefore prefer to retain the original presentation. Note that the figure has also been revised following the advice of Reviewer #1.

Q27. Line #388: "*Instead, the large Yenisey and Ob River discharge between 2014 and 2015 (Fig. 9) was probably the dominant source of the freshening observed in the eastern Eurasian Basin.*"
See my general comment at the beginning regarding the limitations of the approach used to fully attribute the freshening to Kara Sea river discharge.

A: We thank the reviewer for this comment. We have revised the text to clarify that the Kara Sea river discharge is a plausible and likely important contributor to the observed freshening. We hope that in the revised version we made our arguments stronger.

Q28. Line #403: "*It shows that during the winters of 2015 and 2016, around the onset of the freshening event, the divergent heat flux across the halocline decreased from 20 W/m2 to 3 W/m2. This weakening of oceanic heat flux was reflected in the sea ice concentration observed at all moorings during the following summers of 2016 and 2017 (Fig. 15).*"
I am not convinced that the reduction in sea ice concentration can be directly attributed to the decreased oceanic heat flux. The correlation between normalized SIC and velocity shear timeseries is largely driven by the strong seasonal cycle in both variables. The apparent relationship could also result from a third factor, such as seasonal wind forcing, which affects both ice drift and surface currents. Filtering out the seasonal signal would likely reduce the correlation considerably.
If Polyakov (2020b) has already discussed the effect of reduced turbulent heat flux associated with the 2015–2016 freshening on ice cover, then Section 3.3 — at least its latter part — may be redundant.

A: Yes, Polyakov et al. (2020b) discussed this effect in detail. In the present study, we argue that the observed sea ice changes can be attributed specifically to the impacts of ocean stratification and heat release during the freshening event. Therefore, this discussion is highly relevant, and we have retained it in the text.

Q29. Line #404:
The term "*divergent heat flux*" is unclear in this context and should be explicitly defined. It is important to clarify whether it refers to the net upward heat flux across the halocline, a horizontal (vertical) divergence, or another component.

A: We clarified its meaning.

Q30. Line #417 (Figure 13)
I am skeptical that the tracks shown in this figure represent 23 linear monthly segments. The description of the calculation and the reference to Polyakov et al. (2023) do not clarify this. I assume the simulated velocities were interpolated in both space and time with much smaller time steps than one month, which would produce such smooth trajectories.
Regarding the "Pre" and "Post" trajectories: rather than keeping these, it may be more informative to include panels showing trajectories at depths other than the 0–5 m range. The observed freshening in the mooring data occurs over a thicker layer, so it would be valuable to illustrate where the signal originates at, for example, 50, 100, and 150 m depths.

A: The Reviewer is correct that the monthly current velocities were interpolated in time to obtain a much shorter time step. We have added this information to the Methods section. Regarding the inclusion of additional panels at deeper levels, we refer to our response to the Reviewer's general comments, where we explain that the deeper layers are not directly relevant to the main narrative of this study.

Q31. Line #429: "*are statistically significant at 0.05% level*"
I assume the authors intended to indicate significance at $p = 0.05$ rather than 0.05%.

A: We corrected this sentence.

Q32. Line #439: "*nearly doubling the available potential energy*"
I again have concerns related to the definition of APE and the assumption that freshening and stratification automatically increase APE, as discussed in my comment for Line #238. The authors should clarify whether the quantity they compute is truly APE or rather the energy required to mix the water column.

A: We have clarified this in the Methods section.

Summary:

While I have raised critical concerns regarding individual pieces of evidence, the overall body of observations — correlation analyses, trajectory simulations, and the consistency of anomalies across multiple moorings — strongly supports the main conclusions of the paper. I fully agree with the authors' statement that: "Despite these limitations, the core findings, including the anomalous freshening of the eastern Eurasian Basin and northern Laptev Sea driven by Yenisey and Ob river discharge, and its impacts on sea ice and upper-ocean currents, are robust and well supported by extensive observations." With some targeted clarifications and minor revisions addressing methodological details, figure presentation, and terminology, this manuscript can be considered suitable for publication in Ocean Science. The study

presents a unique and valuable dataset and offers important insights into Arctic freshwater dynamics.
Sincerely, Sergei Kirillov

A: We thank the reviewer for his careful and constructive review, which has significantly helped us improve the manuscript.

Reviewer #3

General comments

Q1: The manuscript presents a combination of hydrographic data from moorings and from model outputs to investigate the origin and fate of a freshwater anomaly on the Siberian slope. The in-situ observations (form the NABOS moorings) are unique and highly valuable, as they witness the fate of the Atlantic water in the Arctic Ocean, and capture the interannual variability of the Atlantic Water Boundary Current over the last 10 years at least. The processes leading to the presence of the freshwater anomaly in the observations is analyzed using model outputs. The authors suggest that this anomaly is driven by the increased river discharge that year, while the wind pattern help maintaining this anomaly concentrated on the shelf. The results of this manuscript are for sure valuable and give important insight on the interannual variability of the upper water column in the Arctic. The manuscript is overall well written but is quite lengthy, and I think some figures could be merged/could be made smaller and should be edited for sake of clarity. See my comments below. This manuscript should be accepted for publication after some revisions.

A: We thank the Reviewer for their valuable comments, which significantly improved the manuscript. We have moved several figures to the Supplementary material and modified others, which we hope improves readability.

Q2: I do agree with the 2 previous reviews that the link between the river discharge and the increase freshening is not so strong for me with what is shown in this manuscript, see some of the 'minor comments' below, especially regarding the one on how the wind is discarded very quickly as a driver of the freshwater anomaly.

A: We hope that our revisions have improved this part of the manuscript, particularly by clarifying that winds are the primary drivers of upper-ocean currents and, therefore, of freshwater redistribution from the Kara Sea into the Laptev Sea.

Q3: My other main comment regarding the manuscript concerns the use of the model. The model is used to define the processes behind the freshwater anomaly. However, it is not mentioned if the model even represents appropriately the ocean in the region. It seems hard to explain an anomaly with the model outputs without some explanation on how it performs. Some kind of model evaluation should be added to the manuscript to give some weight to its use.

A: In previous studies, the quality of this reanalysis was evaluated and it was found the model performance to be satisfactory. We have added some references to the Methods section 2.4a.

Minor comments:

L.131: It is in this section that a model evaluation should be added.

A: We have added reference to our previous papers, in which we evaluated the quality of the reanalysis to section 2.4a.

l.145-146: How reliable are the wind field reanalysis reliable in this region?

A: ERA5 is widely considered a high-quality reanalysis. However, specific evaluation information for this region is not available. We have emphasized this point in the text.

l.162: the first measurements of salinity are around 60m if I am not mistaking (Table 1). Which means that a significant part of the surface waters is not observed in the moorings. While I don't think this is changing the content of the analysis, I think this limitation should be acknowledged, especially that it means that neither the mixed layer nor the depth of the Transpolar Drift is actually covered by the mooring array. There is a full sensitivity study done on the definition of the surface layer for compute the freshwater content, but nothing is mentioned about this lack of data at the surface.

A: The shallowest observations during the freshening event (2015–2016) varied (49 m at $M1_1$, 31 m at $M1_2$, 65 m at $M1_3$, 38 m at $M1_4$, 41 m at $M1_5$, and 30 m at M3). We have made several corrections to the table. We acknowledge the limitation posed by the lack of observations in the uppermost ocean layer. To assess its impact, we conducted a test experiment using ITP data (which extend to 10 m depth and provide year-round coverage in the eastern Eurasian Basin in 2009–2010). These tests show that our estimates capture the major part of the freshening signal.

l.171: I was surprised by this value of 77m for the first measurement of salinity, as I thought that the shallowest one was 54m according to table 1. Please see my comment below regarding the clarity of the table.

A: We corrected the table including several missing instruments.

l.200-201: When reading the manuscript, it was not clear for me why the cross-slope shifts of the AWBC core can cause freshening. The explanation does come later in the text, but it would be valuable to have it already here.

A: We have significantly modified this part of the manuscript. We hope it is clear now.

l.201: What do you mean by 'advective terms'? please define?

A: We defined this term in the text.

l.202: how many pairs of mooring do you define then?

A: For the cross-slope mooring section, we identify four such pairs (see Table S2 for clarification).

l.202: please precise that you use meridional velocities as the slope is oriented East-West (or refer to figure 1 at minima).

A: We edited the sentence giving orientation of mooring section and slope.

l.210: what does 'deseasoned' mean? Does it mean that the seasonal average has been removed?

A: We have defined this in the text.

l.210: 'surface salinity time series': is that from in situ observations or from the mooring observations? Please clarify. This is a comment valid for the entire manuscript. It is not always clear if the data presented/used are from the moorings or from the reanalysis of from the model outputs.

A: We have clarified that in the text.

l.230: as mentioned before, it is a bit hard to follow the depths of the different instruments. Please clarify the text and table 1 (see comment below).

A: We have clarified this issue by revising Table 1. Thank you.

l.234-236: I know it is explained later, but at first glance it is very clear that the freshwater anomaly is divided in 2 events. This should be mentioned already here.

A: We have added this statement to the sentence.

l.287-287: I was first confused reading this sentence: 'the freshwater content and the ocean heat content showed a strong opposite tendency'. I understand now (if I am right) that this sentence refers to the average per year, and not to the actual trend of the curve. Please clarify in the text.

A: We have corrected the sentence.

l.288: there is no section 6.

A: We edited this text.

l.321: the meteoric water fraction is mentioned, while the upper water column is not sampled by the moorings. How is it possible to conclude something about the sea ice melt without the upper water column being sampled?

A: These were shipborne observations with water sampling extended to the very surface. We have added this information to the text.

Section 3.2.2: There is no mention of advection patterns on the shelf, while one could explain that they could explain at least part of the freshwater anomaly, and not only the river discharge.

A: Our trajectory analysis specifically addresses this, focusing on the advection patterns over the shelf that lead to the redistribution of freshwater from the Kara Sea into the Laptev Sea. This analysis also indicates that Lena River discharge does not play a role in shaping the freshening event.

l.344: '4-month seasons': isn't it rather 3-months seasons?

A: We have edited this text.

l.345-346: What about the DJF&MAM 2016? Again, it seems that it could be differentiated in two different events, and this anomaly within the anomaly is only mentioned later while I think it should be mentioned earlier.

A: We have stressed in the text the importance of seasonal signal in establishing this pattern.

l.346-347: the last sentence: 'this pattern… easter Eurasian Basin'. I understood this sentence after reading the full manuscript, but here it seems too early to have this claim. Please remove.

A: Following Reviewer #1's criticism, we reduced the number of panels in this figure to one, in order to show only the spatial pattern of the freshwater anomaly. The discussion of a potential link between this freshening event and seasonality is provided elsewhere (e.g., lines 263–267 in the revised manuscript).

l.377: is the model representing correctly the river discharges? Does the model actually show the freshwater anomaly?

A: This is a simulation using a simple algorithm that computes trajectories of passive tracers, rather than a physical model. The algorithm uses currents from the ORAS5 reanalysis and does not include river discharge or other processes. The confusion likely arose because this approach was previously referred to as a "model" in the Methods section. We have revised this section for clarity.

l.387-389: I was a bit lost with the argumentation about the wind not having a significant impact on the freshwater anomaly, as the section before concludes that the wind is important. Please clarify in the text.

A: We agree with the Reviewer that this sentence was not accurate and have revised it.

l.494-496: these sentences should be earlier in the manuscript, in the method section to motivate the use of this model compared to another one.

A: We have revised the Methods section to include the appropriate information.

l.499-503: it would be interesting to investigate if this event of larger discharge was a one-time event, or if it is occurring frequently in the region. This will put this study in a larger temporal context and will provide a larger impact to this study. One question I was also asking myself was: what trigger this increased release of freshwater? Even though not fully detailed in this manuscript, some hypothesis could be advanced.

A: We agree, but since Dolly More has graduated and left the group, we do not currently have the capacity to continue or expand this research.

Table 1: all in all, I find table 1 hard to follow, to know at which depth are which variables. Perhaps add also the variables measured by each instrument. It will help non expert reader to find out what it available where.

A: We edited this table and hope it is much clearer now.

Figure 3 and figures thereafter: I suggest using g/kg and absolute salinity instead of psu for the Salinity units, following the latest recommendations from McDougall et al., 2012.

A: We prefer to keep psu as a unit for salinity since this is usually the most widely accepted choice.

Figure 3: the colorbar chosen is not colorblind-friendly. I suggest to rather a colorblind friendly one, as 'balance' form the cmocean toolbox on Matlab for example.

A: We have used this color palette consistently throughout, with no issues reported by readers. Importantly, the colormap places extreme values in blue and red, while using green—often problematic for color-blind readers—in the intermediate range, which enhances overall readability.

Figure 4 and 5: maybe these 2 figures can be merge by using a left and a right axis. It will also be easier to compare the different variables.

A: We moved Figure 5 to Supplement.

Figure 6: as the text mainly follow the chronological order for the description of this figure, I suggest to merge the winter and the summer panel of this figure, so that the bars are chronologically order, but to keep the color code (orange for summer and blue for winter) to help the comparison in between the same season.

A: The requested changes have been implemented.

Figure 10: same as for figure 3, the colorbar should be changed to something colorblind-friendly. Add also a label for the x-axis. This applies to figure 8 and in general to all figures where there is no label to the axis.

A: See our answer to the previous comment.

Figure 11: In the left panels in a) one curve has the lag applied to it if I understood correctly. However, the x-axis is the date. I would suggest showing the curves without adding/ subtracting the lag for sake of clarity and matching the x-axis label.

A: The reviewer's understanding is correct—we did indeed shift one time series relative to the other using a lag. The purpose of this figure was to illustrate the similarity between the red and blue time series when the lag is taken into account, so our approach is well justified.

Figure 12: the labels are not shown correctly. I would suggest having windroses instead, so that we could see also the variability and not only the average direction.

A: We followed Reviewer #1's advice and replaced the original figure with a single-panel version. The revised plot clearly illustrates that winds during the onset of the freshening event favor the eastward transport of freshwater from the Kara Sea into the Laptev Sea.