

Review of the Ocean Science manuscript “Documenting the 2015-2017 freshening of the eastern Eurasian Basin of the Arctic Ocean and evaluating its drivers and consequences” by Dolly More and Igor Polyakov

In this manuscript, the authors present a truly unique and extensive observational dataset obtained from a multiyear array of oceanographic moorings in the eastern Eurasian Basin. The study documents a pronounced surface-layer freshening event that occurred between late 2015 and early 2017 and links it to anomalously strong river discharge from the Kara Sea approximately two years earlier. The manuscript further discusses how this freshening event may have influenced regional processes, including upper-ocean circulation, vertical mixing, and associated heat fluxes. The manuscript is clearly written and well structured. However, I have a few substantive concerns that I would like the authors to address.

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.

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.

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.

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.

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.

1) 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.

2) 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”).
- M1_{2a} lists “70–754” (MMP?), but according to Figure 2 these data appear to be unavailable.
- M1_{1a} and M1_{1b} are shown with identical SBE37 depths, whereas Figure 2 suggests that the shallowest instrument at M1_{1a} was deployed deeper.
- The coordinates of M1_{5a} and M1_{5b} 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.

3) 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.

4) 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.

5) Line #96: “MMPs profiled from ~40”

This statement is inconsistent with the data presented in Table 1 and should be corrected.

- 6) 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.

- 7) 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.
- 8) 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.

- 9) 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.

- 10) 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.

- 11) 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.

- 12) Line #237: “*strengthening the vertical density gradient.*”
It is preferable to use either “*strengthening the vertical density stratification*” or “*increasing the vertical density gradient*”.

- 13) 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.

- 14) 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.

- 15) 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?

- 16) Line #278:

“The (negative?) salinity anomalies...”

- 17) 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?

- 18) 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.

- 19) 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.

- 20) 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.

- 21) Line #327: “*when spread over the area covered by mooring observations (roughly 77-82°N and 110-140°E; $5 \times 10^5 \text{ km}^2$)*”

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.

- 22) 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.

- 23) 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.

- 24) 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.

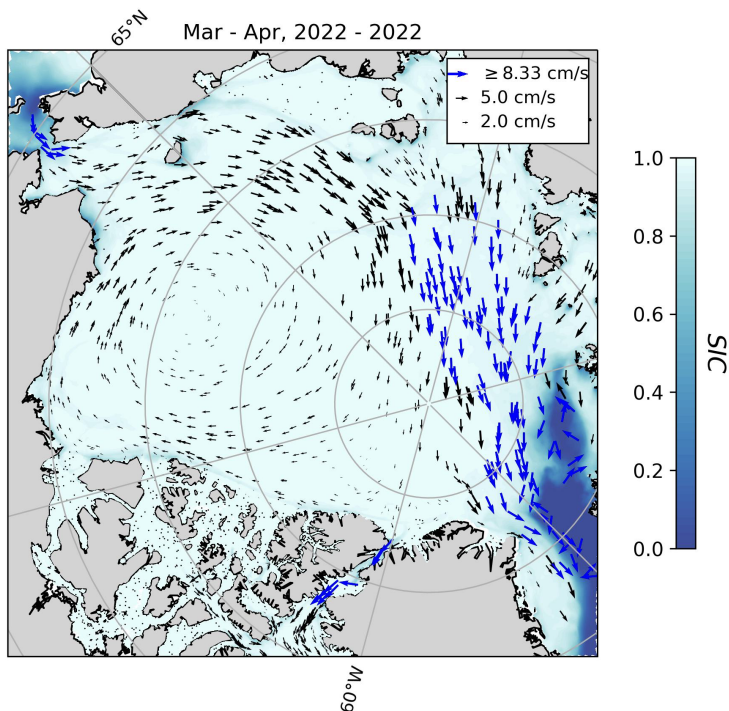
A minor point: there is a small inconsistency between the text (Line #354, “*all statistically significant at 95% level*”) and the figure (significance level $p < 0.001$). If all correlations indeed meet the $p < 0.001$ threshold, it is acceptable to state 99.9% confidence level in the text.

- 25) 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.

- 26) 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 $0.25 \times 0.25^\circ$ 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)



27) 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.

28) 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.

29) 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.

30) 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.

31) 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%.

32) 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.

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