REVIEWERS' COMMENTS and ANSWERS

Reviewer #1

General comment

My major concerns pertain to the clarity and novelty of the finding, rigour of the analysis and quality (and quantity) of illustrations.
As I understand it, the core message of the manuscript is that the seasonal cycle of speed (and shear) in the upper SAO is tightly linked to sea ice and has strengthened. The underlaying drivers are named (increased stratification in summer and increased ventilation during winter) but not shown in the manuscript (I understand that these are earlier findings by the same author(s)). Instead the focus appears to be on arguing that wind-driven near-inertial energy is distributed within the SML. This is in accordance with the over 50 year old concept of slab models, which the authors mention in passing but do not really incorporate in their analysis. Despite of the host of data presented, their argument remains largely qualitative and I find it difficult to decide how novel the insights actually are.

A: We sincerely thank the Reviewer for their careful reading of our manuscript and for the insightful comments provided. While we greatly appreciate the feedback, we respectfully disagree with what we perceive as a somewhat limited interpretation of our findings. As stated in the final paragraph of the Introduction, our objective was—and remains—to construct a cohesive picture of the spatiotemporal patterns and variability of currents and shear in the upper Siberian Arctic Ocean (SAO) over recent years, based on a comprehensive set of mooring data. We believe that the manuscript successfully meets this objective. To support this, we highlight below a list of key findings from our study, which we believe are both novel and quantitatively substantiated:
a. We document the spatial and temporal variability of currents and their vertical shear in the upper SAO, providing quantitative estimates such as means and standard deviations. These are novel results.
b. We assess the influence of sea ice on the seasonal cycle of currents and shear. While the broadening of the seasonal sea ice concentration (SIC) cycle is well established, our finding that upper-ocean currents and shear respond closely—and increasingly tightly over time—to changes in SIC is new and very important. This relationship is supported quantitatively by correlation analyses.
c. We quantified how winds influence the seasonal cycles of currents and shear in both the surface mixed layer (SML) and the halocline. Our results show that, in winter, weakened stratification due to Atlantification allows wind energy to penetrate much deeper than it could a decade ago—well beyond the SML. This leads to a wintertime peak in current speed and shear from deeper layers (quantified by providing estimates of means for current and shear). These are novel and significant results. The observed amplification of currents and shear below the SML during winter challenges the applicability of traditional slab models. This deep-reaching wind influence has significant observed and potential implications for upper ocean mixing and the regional marine ecosystem.

Thus, we believe our results are both novel and significant, and that they extend well beyond the more limited interpretation reflected in the Reviewer's comments. Nevertheless, we have taken all of the Reviewer's specific comments seriously and have addressed each one—either by revising the text and figures accordingly or by providing a clear rationale for why certain suggested changes were not implemented.

Specific comments:

SC1: The only indication for a systematic change in seasonal cycles of currents, shear and sea ice is in the qualitative comparison of the 6-year average 2004-2010 with the 10-year average 2013-2023. In the time series presented, there is no obvious systematic change.

A: We respectfully disagree with this statement. In the manuscript, we provide quantitative statistical estimates of the seasonal evolution of sea ice concentration (SIC) over the past two decades (ll. 292-293 in the reviewed manuscript version). Fig. 7 clearly illustrates a strengthening of the seasonal cycle of upper ocean currents and shear, as evidenced by the greater range of seasonal fluctuations in panels 7c–f for the period 2013–2023 compared to 2004–2010. Additionally, the text (ll. 301-302) presents a quantitative measure—a statistically significant increase in the correlation between SIC and both currents and shear—demonstrating a tightening of this relationship over time.

SC2: The method introduced to separate wind-driven inertial currents from tides is not sufficiently explained, nor tested (but I also don't think it is necessary to attempt this separation in the first place).

A: We have removed this section from the manuscript, as it was not central to the objectives of our study.

SC3: There are far too many figures (over 100 panels in total) that are unfocused, hard to read and contain sometimes odd, unexplained choices. Most of them are hardly referred to and many do not clearly support the reasoning in the text.

A: We completely removed Figure 12 from the main text, moved Figure 11 to the Supplementary Materials, and entirely removed the former Figure S1.

These are my short notes:

SN1: Figures 1,2,3,4,6 all show seasonal cycle of upper ocean currents and shear in relation to sea ice. It should be possible to make this point with 2, max. 3 figures.

A: Fig. 1 does not show the seasonal evolution of currents in relation to sea ice; rather, it presents seasonal and annual mean current vectors and directional roses. Fig. 2 presents the original current data as a function of depth and time, while Fig. 3 displays their near-inertial component. These two figures form the basis for the subsequent analysis of currents in the upper SAO. Fig. 4 shows two-year averages of $|U|$ and U_z. Fig. 6, on the other hand, presents time series of $|U|$ and U_z in relation to sea ice concentration. Therefore, we believe these figures are distinct and each serves a different purpose.

SN2: Figures 7-10 basically do the same for the 2014-2023 period.

A: We respectfully disagree. The focus of the first six figures is on spatial variability, whereas Figs. 7-10 emphasize temporal variations.

SN3: Figure 11 has no clear message; the increase of shear at the bottom of the SML is not really visible in most panels.

A: We removed this figure.

SN4: Figure 13 shows much of the same information as figure 12, but apparently with an inexplicable 2018-2021 time average.
A: We have removed panels a-d from this figure.

SN5: Figure 14: what is the physical motivation for the averaging times in c? d is the same as the top right panel of figure 2 and e is for some reason only given for a depth range from 60m, even though a and b go up to about 45m, which would be a very interesting depth for buoyancy frequency.
A: We revised this figure, eliminating completely panel c.
Unfortunately, above 48m depth, we had two single-depth CTD sensors, whose data are sufficient for plotting temperature and salinity depth–time sections but are not suitable for estimating buoyancy frequency.

SN6: The list of references is not complete.
A: We revised the list of references.

Suggestions for revision:

SR1: Focus the manuscript on the core message of changing seasonal cycles from 2004 to 2023 (e.g. by showing full time series and deriving useful metrics).
A: We respectfully disagree with the reviewer's rather narrow interpretation of our objectives and main findings. As outlined in our responses above, their suggestion overlooks and unduly restricts the scope of our key results. We prefer to maintain the current focus and objectives of the study. Nevertheless, we have carefully addressed all of the reviewer's specific comments.

SR2: The point that there is little difference between all the MB moorings can be made much more succinctly.
A: The length of the manuscript is reasonable, and the result presented is both novel and significant.

SR3: Remove many of the figures (or at least put them into supplementary), revise and focus the remaining figures to directly and clearly support the argument.
A: We followed this advice and removed two figures (previously Figs. 10 and 11) from the main text. The current Figs. 10, 11, and 12 have been revised accordingly.
SR4: Perhaps add quantitative estimates using slab-models or at least incorporate the theory (and related findings) properly into the introduction.
A: We argued that slab models may be insufficient to simulate the complex processes observed in our mooring records. In particular, the wintertime amplification of currents and shear at the base of the ventilated layer—where strong vertical density gradients are present—may require sophisticated non-hydrostatic modeling experiments. However, we added some 1d modeling results emphasizing the role of wind and stratification in shaping upper ocean currents.

SR5: Decisions should be clearly motivated and transparent (why (and how) to use a 2-day window to separate tides from wind-driven currents, why the split at 2004-2010/2013-2023, why show 2018-2021 averages (fig. 13), why not show buoyancy in the 45-60m range (fig. 14)).
A: We removed the separation of tides and inertial currents from the text and added a section to the Methods to justify the time intervals we used. In the revised text, we also noted that the observations (formerly Fig. 14), obtained from single-depth instruments in the upper 50 m, are adequate for showing temperature and salinity, but are insufficient for estimating buoyancy.

Line-by-line:

LBL1: 23: Either expand on the "logistical challenges" or remove the sentence.
A: We removed this sentence. Thank you.

LBL2: 28: How can a cycle follow a coherence?
A: We edited this sentence.

LBL3: 29: Can the absence of ice "drive" something?
A: We edited this sentence.

LBL4: 54: Missing a "the".
A: We reviewed the sentence and found that it was missing a comma, which we added. However, we do not see where the article 'the' is missing in this sentence.

LBL5: 54-56: I think there should be references.
A: We added references, as requested.

LBL6: 56-58: Same here.
A: We revised this sentence to include a statement noting that, to our knowledge, there have been no observations of currents in the Makarov Basin of the Arctic Ocean. This type of statement is difficult to support with a citation.

LBL7: 60-61: No brackets necessary.
A: We believe that placing the references in brackets is appropriate in this context.

LBL8: 62-65: If this is to suggest a connection between inertial currents and Arctic Ocean circulation it should be explained here.
A: We believe that these two sentences are clear in meaning and do not require any changes.

LNL9: 69-71: References should be provided for this statement.
A: We added references as requested.

LBL10: 94: Is this no problem for the magnetic compasses in the ADCPs?
A: We revised this statement adding that ADCP measurements may be affected by the same issue.

LBL11: 95: Odd title for the section.
A: We modified this title.

LBL12: 123: I don't understand how this method could separates tides from wind-driven near-inertial oscillations. Where does the 2-day window come from? How is it implemented? What is the sensitivity? The explanation is lacking and the figure S1 does not provide useful information. In the context of this story, I think the authors could just proceed with NIC that contain wind driven and tidally driven currents (i.e. just do a 10-14h bandpass).
A: We removed this piece from the text.

LBL13: 153: Specify which moorings are considered to be on the shelf.
A: We added that MB8 and MB9 moorings are on shelf.

LBL14: 157: This is only (ever so slightly) visible for MB9.
A: We added clarification.

LBL15: 159-161: Sea-ice also provides a friction barrier for tidal currents.
A: We have revised the wording of this sentence—thank you.

LBL16: 192: The error bars appear to be exceedingly small; are they correct?
A: Yes, they are correct, given the number of observations used to compute each annual or seasonal mean.

LBL17: 203: Velocities in figure 2 mostly do not look surface intensified.
A: We added a clarification that the surface intensification is mostly in summer. Figure 12 offers a clearer visualization of surface-intensified currents. During the winter months, however, surface currents are only slightly stronger than those at deeper layers. Summer surface intensification is evident in Fig. 12. Thus, there is no contradiction in our statement.

LBL18: 221: Connection instead of connectivity?
A: We revised this sentence.

LBL19: 223: In the figure it is not possible to see if it is before, at the same time or after.
A: We edited the sentence.

LBL20: 225: How is the significance calculated?
A: We used a standard approach based on estimating t-statistics. We added a sentence in the text about that.

LBL21: 227: This does not appear to be the case for MB6.
A: We edited the text reflecting this fact. Thank you.

LBL22: 229: The differences often appear to be negligibly small.
A: The consistent nature of this relationship across most mooring records suggests that it is not a random occurrence.

LBL23: 234: Figure 7: Figure title talks of M1, should be MB1? Why split at 2010?
A: We renamed the mooring in Fig. 7. Figure 2 in Polyakov et al. (GRL, 2020) uses the same MB1 mooring records from 2004–2018 and clearly shows a shift in the upper ocean current regime beginning with the 2013 record (noting a data gap from 2010 to 2012). We extended the record with several recent years and found that the currents have remained relatively stable since 2013. This is the basis for our chosen separation. A description has been added to the Methods.

LBL24: 242: Wind speed is not the only meaningful variable; what about the wind direction or wind spectra?
A: Since most of our moorings are located in the deep, open ocean, the direction and shape of the wind speed spectra are less important in determining upper ocean currents.

LBL25: 247: In line 244 data from 2004-2023 is mentioned; why is only 2013-2023 considered here?
A: We partially addressed this in our response to LBL23. However, to compare the 2013–2023 data with earlier years, Fig. 9 includes the mean currents and shear from 2004–2009, shown as dashed lines.

LBL26: 254: Why is the data from 2004-2009 not shown?
A: They are shown as dashed lines in Fig. 9.

LBL27: 286: No obvious consistent change is visible in figure 10.
A: We observe that, in general, the summer minimum in sea ice concentration lasts longer in recent years, which broadens the summer peak of near-inertial (NI) currents. This is confirmed by the mean seasonal cycles shown in Figure 7.

LBL28: 314: This is hardly visible (if at all) in most panels of figure 11.
A: We removed this figure; instead a reference to Brenner et al. 2023b paper is included.

LBL29: 319: Should be Figure 13a-d.
A: Corrected, thank you.

LBL30: 322: Reference formatting.
A: We removed this reference from the text.

LBL31: 320-339: Isn't this the premise of all slab-models since about 1970?
A: That is correct. However, our findings extend well beyond the surface mixed layer, reaching depths greater than 100 m. Slab models—and ocean models in general—struggle to accurately capture the ocean's response to the complex interplay of wind forcing and stratification, particularly as it is influenced by Atlantification.

LBL32: 334-336: How are near-inertial wind signals "transformed" to non-inertial residual currents?
A: We revised the sentence to clarify that the near-inertial component refers to the currents, not the wind.

LBL33: 346: Why not show the full data set?
A: We have added the early years (see revised figure in the text), but there is little change in the monthly mean current profiles.

LBL34: 352: Where does the 2018-2021 average come from?
A: This analysis requires high temporal resolution current data. Such data are limited for the deeper layers (below the SML) by these years, therefore we used the available records to produce this figure. These years are representative of the last decade (e.g., Fig. 9). A detailed explanation is provided in the main text.

LBL35: 362-363: This is very confusing. Where am I supposed to see stratification decrease in winter? In figure 14c, buoyancy frequency tends to be highest in winter. Neither can I see greater currents or late winter shear in figure 14d.
A: We have revised this paragraph and updated Fig. 12 (previously Fig. 14). In particular, we now emphasize that temperature and salinity clearly indicate deep (>150 m) seasonal

ventilation, as shown by the cooling and freshening of the 150–200 m layer in late winter (February through May) in both 2022 and 2023. The seasonal evolution of the surface mixed layer (SML) can be traced by strong shear at its base (upper 30 m) during the early winter period (Fig. 12e), and elevated buoyancy extending to ~80 m later in the season (Fig. 12f). By April–May, the ventilation reaches significantly deeper, exceeding 100 m in depth.

The connection between currents and stratification is illustrated by their running correlation at 100 m depth, revealing a complex pattern of interactions within the Arctic halocline (Fig. 12c,d). Notably, the correlation exhibits a clear and intriguing seasonal cycle: it is negative during summer and becomes strongly positive during the peak of the ventilation season. We have added this discussion to the main text.

LBL36: 365: a and b are never discussed, it is unclear how the averaging periods in c are defined; why is buoyancy only shown from 60m, when T and S go up to 45m?
A: We edited this paragraph, incorporating references to Fig. 12a,b. Now, instead of averaging we used wavelet to show seasonality of interactions between stratification and currents.
In the text, we added a description noting that continuous, high vertical resolution MMP-based profiles of water temperature and salinity—suitable for calculating buoyancy frequency—were limited to depths below 48 m. Above this depth, we had two single-depth CTD sensors, whose data are sufficient for plotting temperature and salinity depth–time sections but are not suitable for estimating buoyancy frequency.

LBL37: 395: This is not visible in figures 9 and 10.
A: The mean 2004–2009 currents and shear are shown as dashed lines in Fig. 9, so this statement can be easily verified using these materials.

LBL38: 402: Where and how is this shown?
A: We added discussion to the text and updated Fig. 12 to include the correlation between current speed and stratification.

LBL39: 410-412: This is not shown.
A: We revised the text to reflect the uncertainty surrounding the nature of the winter forcing. Additionally, we noted that addressing this uncertainty may require sophisticated model experiments beyond slab-based approaches.

LBL40: 421-423: Is this new?
A: We revised the sentence to emphasize the new findings.