

## EDITOR AND REVIEWER'S COMMENTS and ANSWERS

### Editor

#### **Public justification (visible to the public if the article is accepted and published):**

The revised version of your manuscript has been substantially improved but there is still a number of issues, related to description of methods (calculation of errors) and visualization (too many and too complex panels) that need to be resolved. Therefore, the manuscript can be published subject to minor revisions as recommended by the referee.

Additional private note (visible to authors and reviewers only):

Dear Co-authors,

The revised version of your manuscript has been substantially improved but one of the reviewers still have a number of remarks that must be taken into account. In particular, the way of calculating errors and the uncertainties in the correlations should be properly addressed. A few panels or figures that are not directly discussed in the manuscript could be moved to the supplement to enhance the clarity and readability of the paper.

These remarks are rather minor, and I hope that the next revision will be the final one.

Kind regards,

Agnieszka

**A:** We have addressed all of the reviewer's comments. In particular, we expanded the description of our error calculation methods and reduced the number of figures in the main text.

### Reviewer #1

#### **General comment**

I appreciate the authors' extensive response to my previous comments. I can see that in my attempt to distill the core message of the paper (and its vast amount of panels), I was too narrow. I appreciate the authors' explanation and their efforts to remedy some of the points I raised. At the same time, I still believe there is room for improvement in the manuscript and the illustrations.

Note: line numbers are based on the tracked changes version. (As a side note: Since the tracking of changes apparently did not work consistently, it is difficult to know how much the authors actually changed in the manuscript...)

**A:** We sincerely thank the Reviewer for their careful reading of our manuscript and for the insightful comments provided.

### Major Comments:

**MC1:** The choices of calculating and presenting statistical confidence levels is unclear. Significance testing of the correlations is not explained/shown, apart from the mention that it involves a t-test. It seems odd that correlations as low as -0.19 are significant in this context (figure 6). Did the authors correct for autocorrelation in the time series (Chelton modified method)? How high does a correlation have to be to be above the confidence level in figure 6 (in figure 12, the level of 0.45 is given)? Revising the confidence intervals of the correlations may have ramifications for the conclusions drawn from figure 6.

**A:** We re-calculated the cross-correlations using the suggested approach, but the results remain unchanged: even the lowest correlation,  $R = -0.19$ , is statistically significant at the 0.05 level ( $p = 0.002253$ ). An explanation has been added to the text.

**MC2:** The "error bars" in figure 9 are all collapsed onto a point. Clearly this is not a useful way to display variability/uncertainty around the averages. Perhaps the standard deviation would be more useful than standard error in this case.

**A:** For the error bars, we used the same scale as defined by the vertical axes for the mean values. Due to the large number of observations used in calculating the means, the standard errors are very small. This is reflected in the narrow width of the whiskers in Figure 9. We are not aware of a clearer way to illustrate that the errors are minimal and the means are well defined.

**MC3:** In their effort to make a persuading argument, the authors at times use strong wording that does not appear to be supported by the evidence referred to. In lines 312-316, the authors claim that an increase in correlation from -0.73 to -0.94 (or -0.89, when using similar periods) shows a "tightening of relationship", with currents and shear following now "much" closer the seasonal cycle of sea ice. I am sceptical that this subtle change of correlation coefficients can be used to indicate a physical change of processes and it certainly does not support the strong wording used by the authors.

**A:** We have softened the tone of our presentation.

**MC4:** Along the same line: While running correlations in figure 12c seem indeed to be generally larger in late winter, there is no sign that they are (consistently and significantly) negative during summer (as claimed in line 385).

**A:** We added seasonal means, which clearly demonstrate that the sign of the correlations changes from one season to another.

### Minor comments:

The authors still show figures with a great many sub-panels and details that are hardly (if at all) referred to or discussed. In particular, figures 2 and 3 are still only there to get a general impression of increased currents during the summer, that are (sometimes) gradually deepening. The difference between moorings is not really discussed in the text. So at least figure 3 could for example be moved to supplementary without loss to the story since quantitative differences between total and near-inertial currents are much easier seen in figure 4.

**A:** We moved Fig. 3 to Supplementary.

Figure 6 is very complicated, with lots of details and measures showing great variability between years and moorings. None of these measures or variability are discussed in the manuscript (there is one sentence in the discussion acknowledging that there is variability in figure 6, line 358). On the other hand, some of the same measures (correlations) done for

figure 7 are discussed in the text, but not shown in the figure.

**A:** Nearly the entire paragraph (lines 230–240) is dedicated to discussing this figure; therefore, we prefer to retain it in the main text.

#### **Line-by-line:**

129: Why is the wind time series in figure 5 split at 2006? That does not fit into the story at all.

**A:** We tested different time periods and obtained practically the same result; a comment has been added in the text.

235-246: It varies a lot in how far this simple relationship is visible at the different moorings and at different years. This is not mentioned at all. Also, correlations between -0.19 and -0.71 for total currents indicate that this relationship may be more subtle.

**A:** We added a sentence reflecting this fact. Thank you.

314: A modest increase in correlation does not indicate a "much" closer relationship. Consider adding the correlation values to figure 7 (as in figure 6).

**A:** We revised the wording of this sentence. Due to limited space in Fig. 7c, we decided it is sufficient to include these estimates in the text.

325: Residual currents should be total currents I believe... At least there are total currents shown in figure 10.

**A:** The sentence correctly identifies these as residual currents.

374: I think the wording is confusing here. Strongest shear is visible during summer, not early winter. What is visible is a deepening of the layer with maximum shear.

**A:** We edited the sentence.

376-377: This is visible for 2022, but in 2023, the maximum remains between 60-80 m.

**A:** Ventilation reached >100m in 2023 as well – see yellow color at these depths in March-April of 2023.

392: I can see that the figure is in agreement with the point being made, but since it shows neither wind nor stratification I wonder how it can provide evidence for their role.

**A:** In the text, we referenced our earlier papers that argued for stronger stratification in the earlier years.

426-427: Is there any evidence for this?

**A:** We added a reference to Fig. 8, which compares current speed and shear between 2004-2009 and more recent years.

432-422: The relationship was already very strong before (-0.74); changes are probably hardly significant in the statistics.

**A:** This is not just a statistical observation; visual inspection clearly shows a tighter connection between SIC and  $|U|/U_z$  in recent years compared to the past.

Figure 5: Why is the wind time series split at 1991-2006 and 2007-2022?

**A:** As indicated in our response above, the results are robust regardless of the averaging interval, with 2006/2007 used to split the 1991-2022 period into two equal parts.

Figure 7: In figure 6 there are correlation coefficients between ice and speed/shear; why not add them to figure 7 as well (instead of just mentioning those for shear in the text)?

**A:** Please see our response to the comment to line 314.

Figure 8: Name M1 should be changed to MB1

**A:** [We removed this name from the figure.](#)

Figure 12: I don't think the wavelet analysis adds anything to the story; it can be removed without loss.

**A:** [The wavelet clearly highlights periods dominated by positive and negative correlations \(see Reviewer's question MC4\); therefore, we prefer to retain this panel.](#)

Figure S2: X axis of b does not show correctly in the pdf.

**A:** [We have corrected that – thank you.](#)