

Response to the comments of Martina Zapponini

I would like to thank the authors for taking into consideration my suggestion to extend the analysis regarding the selection of the meridional wind location. I think this addition provides a beneficial detail to the overall analysis. The manuscript is very well written, and the corrections and modifications made have improved its clarity and comprehensiveness.

I would support its publication with just minor additional comments reported here.

We would like to thank again the reviewer for the positive evaluation of this revised version and the suggestions, both at this stage and for the initial submission, that have contributed to improve the quality of our manuscript. Our responses to the comments are below in blue.

Minor comments

Line 142-143. Since the definition of the SAF is given, and its contour is included in Fig.1, it should be mentioned why it is not included in the analysis.

We mentioned lines 92-93 of the revised version that 'The SAF is located even farther north of the sea ice edge, and is therefore not expected to directly impact sea ice'. To make this point clearer, the text has been modified to 'The SAF is located even farther north of the sea ice edge. It is therefore not expected to directly impact sea ice and is not addressed here.'

Line 176-193. The author say that the datasets considered cover different periods, but it would be good to specify if the total period of availability for each of them (for observations/reanalysis) has been used in the study or, if not, which period. It would give a more comprehensive idea of the period of overlapping among the datasets.

For each of the dataset, we use the full period of availability at the time of our analysis. Several records have a small overlap or even none. For instance, the front definition of Orsi is based on in situ observations available up to 1990 while the definition of Park et al. uses satellite observations between 1993 and 2012. However, the mean signal on the mean position of the winter ice edge is robust and stable enough through time to identify links between the different variables even if different periods are selected. To make this more explicit, we insist more on the exact period covered by the observations in the revised version.

Line 294. "The main element that explains these changes in the distance." I think would sound better

Thanks. This has been modified in the new version.

Line 299. The correlation for the SBdy front following the definition of Orsi et al. (1995) is almost always giving offset values with respect to the other two fronts and even to the same front following the definition of Park et al., (2019). Is there a possible explanation for that?

The SBdy is the front that displays the largest differences between the two definitions (Orsi vs. Park), likely because its signature is weaker than the one of the other fronts (see the second paragraph of section 3a for instance). In the definition of Park, the SBdy position is more parallel to the one of the other fronts, probably because of the selected definition based on SSH. This could be the reason of the higher correlation but this is just a hypothesis and a more detailed comparison of the two definitions and an analysis of the limitation of the observations at those high latitudes would be required to verify this.

Figure 8/Table 3. It would be interesting to include in this analysis the other two fronts to see if the results are similar in terms of sea ice edge prediction.

The position of all the fronts is highly correlated with the position of the ice edge (Table 2) so by construction similar results can be obtained using any of them. The numbers could slightly change but this would not modify our conclusions. To avoid a too long discussion, we prefer to include the analysis for only one of the fronts (i.e. the PF).