# Response to Editor

Dear Dr. Xichen Li,

Thank you for giving us the opportunity to revise the manuscript. We sincerely appreciate the constructive comments and suggestions from you and reviewers, which have significantly improved the quality and clarity of this work. Please see below for a summary of our revision:

1) **Introduction:** We revised the introduction to better highlight the theme and significance of this study.

2) **Methodology:** To clarify the rationality of the study area selection, we conducted a search analysis for the potential preferred area over the entire central Arctic Ocean.

3) **Results and Discussion:** We subdivided the ideal deployment areas and added deployment recommendations, removed the assessment of sea ice thickness from section 3.3 and merged section 4.3 and 4.4 to present more reliable and clear results.

4) **Additional Analysis:** We have extended the study period, which is now from 1979–1980 to 2022–2023, and added new analyses of precipitation and snowfall along the trajectories. For validation, we expanded the buoy dataset, conducted a closed-loop test using backward trajectory analysis, and added comparisons between the results derived from semi-Lagrangian and Lagrangian methods to further evaluate the reliability of our reconstructed result.

Below, we provide point-by-point responses to the comments, line numbers refer to the revised manuscript with track changes.

Thank you again for your time and consideration.

Best regards,
Ruibo Lei, Xiaoping Pang and co-authors

## Response to RC1

Thank you for your time and constructive comments on the manuscript "Estimation of duration and its changes in Lagrangian observations relying on ice floes in the Arctic Ocean utilizing sea ice motion product". We have considered comments carefully and incorporated practically all of them in the revised manuscript.

**Comments:**

This manuscript is interesting to me, in which the ideal deployment locations in the central Arctic Ocean for ice camp or buoy have been discussed by using SIM product, and the main standard is to ensure that Lagrangian observations can last a period as long as possible. The logic and structure of the manuscript are ok for me, but I still suggest the authors should address the following issues.

**(1)** L66, "The Arctic sea ice is mainly driven by wind and oceanic current stresses". Maybe you mean the drift of Arctic sea ice or sea ice dynamics is driven by….

**Reply:** Yes, we revised this sentence to "Arctic sea ice dynamics is mainly driven by wind and oceanic current stresses...". In the revised manuscript, as the introduction was revised to emphasize the main topic, we removed this sentence.

**(2)** L73-77 tell the motivation of this study. But I am not sure if there is any similar study on this topic? From the statement here, this study is the first one considering the ideal deployment location in Arctic Ocean.

**Reply:** In the introduction, we provided an overview and summary of the main achievements of previous studies on the ideal deployment location or logistical considerations in the Arctic Ocean (**lines 70-77**).

**(3)** The content in the introduction section is a little confusing. What exactly you want to summary in this part? Sea ice dynamics? Pervious ice stations? Or something else?

**Reply**: Thank you for pointing it out. Actually, we want to emphasize the history and challenges of long-term observations based on ice camps or buoys, as well as the importance of ideal deployment areas identification to ensure continuous and effective observations. We have reorganized and revised the introduction to improve the relevant expressions (**lines 65-67, 70-77**).

**(4)** L110. The definition of the rectangular area in Fig.1 is still somewhat arbitrary to me. Maybe you can put the EASE-Grid as the background and then select some from all of them. “The reasons for this diagnosis will be given later.” Please specify where you have discussed this problem.

**Reply:** Thanks to your suggestion. To demonstrate the rationality of the definition of the rectangular area, we conducted surveys for the potential preferred area over the entire central Arctic Ocean using sea ice motion mean field from 1979 to 2023. The results showed that 91.6% of starting points within the rectangular area allow ≥ 9 months of drift trajectories without drifting into the EEZ or beyond ice zone, exceeding other region within the central Arctic regions. Even in the last decade (2013–2023), with thinner and younger ice, the rectangular region had 25.6% of effective starting points, compared to an absence of such points in other regions. So it is extremely difficult to find suitable areas to deploy ice camps or buoys in the central Arctic Ocean outside of our defined rectangular region in order to maintain Lagrangian observations for a long enough period of time. The relevant result was added to Section 2.1 (**lines 117-128**).

**(5)** Section 2.3. If the ice floe is broken into pieces during drifting, is there any impact on the calculation of the survival time (ST)?

**Reply:** In reality, the breakup of an ice floe during drift certainly has an impact on survival time. However, in our calculations, we cannot be judged when and where the ice floes breakup would occur using the sea ice motion product. Such impact and challenges for maintaining observation of long time series using ice camps or buoys are not within the scope of this study. However, we still highlighted this impact in the conclusion section.

In this study, we mainly use sea ice concentration and exclusive economic zone boundaries to determine survival time of ice camps or buoys. We have already explained such limitations of this study in **lines 543-547, 550-552**. Based on our analysis and diagnosis, we wish to identify potential ideal areas for buoy or ice camp deployments. In actual operation, the operating time of buoys or ice camps also depends on the breakup or collapse of the ice camp or buoy and its supporting ice floe, the formation of melt ponds and the intrusion of polar bear, etc. Therefore, what we infer should be the maximum potential survival time. Nevertheless, we argue that such analysis is still necessary for the selection of deployment areas for buoys or ice camps, especially when we hope to obtain a sufficiently long time series of longer observations.

**(6)** L197, “FDD(TDD) refers to the integral of near-surface air temperatures below…”. What kind of air temperature? hourly average or daily average?

**Reply:** We used the daily average air temperature for the integration of FDD and TDD, as explained in **line 226**.

**(7)** Section 3.2. Air forcing such as temperature and long-wave radiation have been investigated here. How about the precipitation? Snowfall poses an important impact on sea ice growth and decay.

**Reply:** Following this suggestion, we calculated the precipitation and snowfall along the trajectories reaching the BG and TPD regions, respectively, and added some discussions on their regional differences (**lines 302-309**).

**(8)** Section 3.3. The ice-wind speed ratio in Fig.8 is also overall lower than the typical values in free-drift analytical solution. You can also discuss this difference, maybe relating to sea ice concentration.

**Reply:** Thank you for the suggestion. We discussed why the ice-wind speed ratios are lower than typical values in free-drift analytical solutions in **lines 340-343**.

## Response to RC2

Thank you for your time and constructive comments on the manuscript "Estimation of duration and its changes in Lagrangian observations relying on ice floes in the Arctic Ocean utilizing sea ice motion product". We have carefully considered all comments and made corresponding changes in our revised manuscript based on these suggestions.

The paper focuses on improving Arctic Lagrangian observations by analyzing long-term sea ice motion data (1979-2020). The study evaluates suitable deployment zones for ice camps and buoys by using sea ice motion products and incorporating atmospheric circulation patterns like the Arctic Oscillation and Arctic Dipole. The authors highlight the declining survival time of ice floes and the increasing challenges for Lagrangian observations due to climate change. This research is highly relevant, given the rapid transformations in Arctic sea ice dynamics and the growing need for precise observational data for climate modeling. By integrating trajectory simulations with EEZ constraints, the study provides actionable insights for future observational campaigns.

I appreciate the exhaustiveness of all sea ice thermodynamic and dynamic throughout the manuscripts. However, I have several concerns regarding the Area Of Interest (AOI) , the methodology protocol, and some logic explanations. Therefore, I recommend that the paper undergo major revisions before it can be considered for publication.

### General Comments:

**1.** I realized that the aim of the work is to provide the reference for the ideal deployment locations in the central Arctic Ocean (in Line 73), but I don't understand why author choose the starting points region just within the rectangular area instead of within the EEZ boundary since EEZ anyway is divided into BG and IPD? So, I am not sure the motivation, is it just want to find the ideal deployment region within the rectangular only?

**Reply:** Following this suggestion and the comments from other reviewer, we conducted surveys for the potential preferred area over the entire central Arctic Ocean using sea ice motion mean field from 1979 to 2023, and added a statement of the study area in the revised version to further demonstrate the rationality of our defined rectangular area (**lines 117-128**). Details are given below:

Using the 1979-2023 climatology field of sea ice motion, the ice trajectories were reconstructed for the period starting from October 1 to September 30, i.e., one year, using a total of 4289 points over the entire central Arctic Ocean as starting points. With a threshold of the trajectory being in the central Arctic Ocean beyond the exclusive economic zones (EEZs) >= 9 months, we found that 91.6% of the defined rectangular area is eligible, while only 36.0% of the peripheral area is eligible. Even during 2013–2023, a period facing more severe challenge due to the thinner and younger ice, the rectangular region contained 25.6% of effective starting points, while the other regions had no such points. So it is extremely difficult to find suitable areas to deploy ice camps or buoys in the central Arctic Ocean outside of our defined rectangular area in order to maintain Lagrangian observations for a long enough period of time. This suggests that considering the computational cost, the definition of our defined rectangular area is reasonable.

**2**. Data and method part: How do you interpolate the 25 km ice motion when employing Lagrangian methods, linear or inverse distance weighting? Do you apply the Lagrangian method from start to end without any regridding during the period? How do the results compare to a semi-Lagrangian approach?

**Reply:** Thank you for suggestion. We have revised the relevant sentences and added the discussion. The details are as follows:

(1) We used the bilinear interpolation method to interpolate ice motion speeds (**lines 180-181**);

(2) The original grid of sea ice motion products is the EASE-Grid, and the grid of the study area is the polar stereographic grid, so before applying the Lagrangian method, the original sea ice motion is regridded at the study area grid point, and then bilinear interpolation is used to obtain the Lagrangian sea ice motion speed;

(3) We added a comparison of the semi-Lagrangian method with the Lagrangian method, and validated the ice trajectories reconstructed by the semi-Lagrangian method using buoy trajectories in Section 4.1. The results showed that the reconstructed trajectories from the semi-Lagrangian method are highly similar to the results from the Lagrangian method, however the results derived from the Lagrangian method have a relatively high accuracy compared to that obtained from the semi-Lagrangian method. (**lines 374-385**)

**3**. The validation of buoy trajectories seems to focus on data after 2014. Are there additional buoy datasets available from earlier periods? If not, are the selected buoys representative and exhaustive for this study?

**Reply:** As suggested, we collected earlier buoy data spanning from 2010 to 2023, obtaining the data from 15 buoys for each region of BG and TPD for validation, and

**Table A1** (**line 557**) shows the basic information of all 30 buoys. We revised the corresponding result in Section 4.1.

**4**. Another interesting point to explore could be backtracking trajectories instead of forward tracking. For trajectories with >9 months survival time (ST), does the backtrack reveal that their starting points are mostly within the rectangular AOI? This may provide valuable insights into uncertainties and trajectory origins.

**Reply:** Following the suggestion, we conducted a closed-loop examination by reconstructing backward ice drift trajectories from endpoints of the original forward-reconstructed trajectories. Using trajectories with a survival time (ST) exceeding 9 months, we obtained the spatial distribution of the endpoints of these forward trajectories during the study years, extending to 1979/1980–2022/2023. We then used the Density-based Clustering tool to identify the hotspot region of the endpoints. After identifying the main hotspot regions, we reconstructed backward trajectories from the grid points (795 points) within the hotspot region to check whether the terminations of the backward trajectories can reach the recommendation deployment areas. Results showed 66.3% of backward trajectories returned to recommended regions, which demonstrated the high confidence in the reconstruction method of ice trajectory. We added some text in **lines 386-393**, as well as **Figure 9** to describe this closed-loop examination to further evaluate the reliability of the reconstructed ice drift trajectories.

**5**. When using 2m air temperature for calculating Freezing Degree Days (FDD), how was the daily value derived - was it simply a mean of hourly data? Providing clarity on this calculation is crucial for reproducibility. How about the bias in ERA5 temperature.

**Reply:** We revised the description for the FDD/TDD calculations and added sentences in the data section describing the evaluation of the ERA5 variables used in this study (**lines 158-161, 227-228**). The details are as follows:

(1) For trajectories with a 90% probability of reaching the BG and TPD regions, the difference between the 2-m temperature calculated from ERA5 hourly data and the daily mean 2-m temperature obtained directly from ERA5 ranged from $-2.66 \times 10^{-4}$ K to $2.65 \times 10^{-4}$ K, with an average difference of $7.18 \times 10^{-7}$ K. This indicates that the discrepancy between them is negligible. Figure 1 below illustrates that the FDD and TDD of 2-m temperatures derived directly from daily ERA5 data and those calculated from hourly ERA5 data exhibit nearly identical trends from 1979 to 2022. Notably, the increasing trend of FDD in the BG and TPD regions is consistent, with both datasets showing equal values.

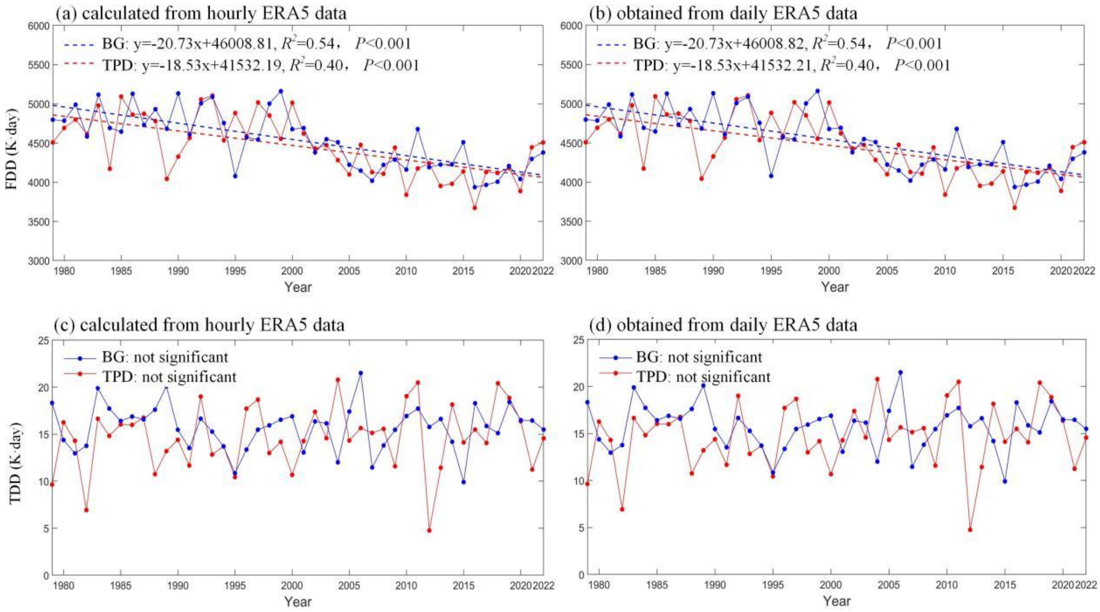


Figure 1. Comparison of FDD/TDD using daily 2-me temperature calculated from hourly ERA5 data versus daily 2-m temperature directly from ERA5.

(2) We consulted the literature on ERA5 temperature assessment in the Arctic. The correlation between ERA5 2-m temperature and N-ICE2015 observations is above 0.9 in January-May, but drops to 0.57 in June. In January-June, the 2-m temperature data exhibit a warm bias, with a bias range of about 0.8 to 3.4°C (Graham et al.,2019). Compared with the IMB and Snow Buoy observations, the ERA5 2-m air temperature also exhibits a warm bias ( < 4°C), with a larger warm bias in September-May and a smaller warm bias in June-August (Wang et al.,2019). Overall, the warm bias in ERA5 2-m temperature may lead to relatively small values in the FDD calculations, but our study mainly focuses on the difference in the trend of FDD in the BG and TPD regions. The influence of warm bias of 2-m temperature on the trend is relatively trivial compared to its absolute value.

Graham, R.M., Cohen, L., Ritzhaupt, N., Segger, B., Graversen, R.G., Rinke, A., Walden, V.P., Granskog, M.A. and Hudson, S.R.: Evaluation of Six Atmospheric Reanalyses over Arctic Sea Ice from Winter to Early Summer, Journal of Climate, 32(14): 4121-4143, https://doi.org/10.1175/JCLI-D-18-0643.1, 2019.

Wang, C., Graham, R.M., Wang, K., Gerland, S. and Granskog, M.A.: Comparison of ERA5 and ERA-Interim near-surface air temperature, snowfall and precipitation over Arctic sea ice: effects on sea ice thermodynamics and evolution, The Cryosphere, 13(6): 1661-1679, https://doi.org/10.5194/tc-13-1661-2019, 2019.

**6**. I am more interested in Figure 5, which is more pratically in the future. Shouldn't you further add more recommendation on the deployment for the future based on the 2007-2020 analysis (and also, could you longegate the time span from 1979-2023), and further make some uncertainties or high-recommend and mediate-recommend about the region? Since now for me, the all materials somehow distract me about the whole motivation. Incorporating uncertainty estimates and differentiating regions into high-recommendation and moderate-recommendation zones would greatly enhance the practical utility of the paper. As it stands, the extensive materials somewhat distract from the core motivation of the study.

**Reply:** Based on this suggestion, we extended the study period, is now from 1979–1980 to 2022–2023 because the sea ice motion data for 2024 are not yet available.

As shown in Figure 2, the probability distribution of ST > 9 months ranges from 0.09 to 0.91 in the study region, so we classified the regions with probabilities between 0.75 and 0.85 as moderate-recommendation zones, and those greater than 0.85 as high-recommendation zones. On this basis, we subdivided the ideal deployment areas in **Figures 4, 5, and 11** into moderate- and high- recommendation zones in the revised manuscript for further discussion. Furthermore, we also added discussions on uncertainty estimation and provided suggestions for future deployment based on the recommendation zones (**lines 246-249, 269-272**).

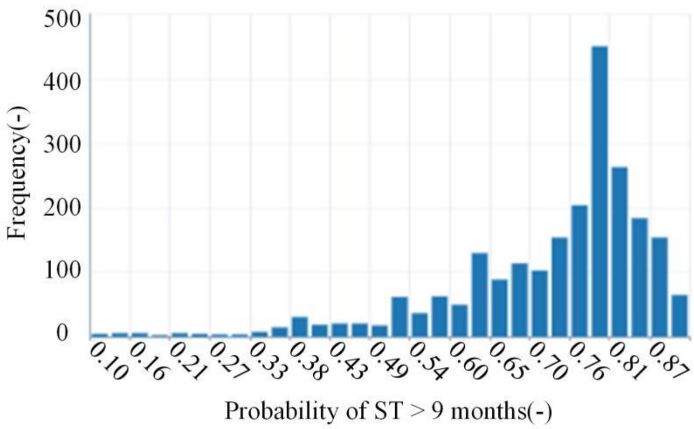


Figure 2. Probability distribution of ST for no less than 9 months in the study region.

**7.** Section 3.3, I'm not sure how much information related to the motivation can get obtained from here, please considering make them concrete.

**Reply:** Due to the significant uncertainty in the data on sea ice thickness, the results of the impact of changes in sea ice thickness on future deployment recommendations or the operation of ice camps may not be reliable. Therefore, we removed the section on sea ice thickness and relocated the content related to the ice-wind speed ratio to Section 3.2 (**lines 333-350**).

**8.** Section 4.1 requires further elaboration. In particular, I recommend adding an uncertainty analysis or sensitivity test to strengthen the robustness of the findings.

**Reply:** Thank you for the suggestions. To improve the robustness of our results, we have added an uncertainty analysis in Section 4.1, consisting of the following three aspects:

1) To improve the representativeness of the validation results, we have collected buoy data obtained from earlier years from 2010 to 2023 (**line 557, Table A1**), thereby having 15 buoys for each region of BG and TPD for validation;

2) We added a discussion on the comparison of the reconstructed trajectories using the semi-Lagrangian and Lagrangian methods (**lines 374-385**);

And 3) As mentioned earlier, for trajectories with ST> 9 months, we reconstructed backward trajectories from endpoint hotspot region and analyzed the uncertainty of the endpoints of the trajectories using a closed calibration assessment method (**lines 386-393**).

**9**. I don't fully capture the Table 1 concerning its physical mechanism, first of all, how to understand the autumn CAI only have the obvious significant correlation with longitude in BH, but more correlated with both IPD and IPD/BH in latitude.

**Reply:** Table 1 showed the correlation between the atmospheric circulation indices and the longitude or latitude of the ice drift trajectory endpoint. Actually, CAI represents the air pressure gradient difference between the east and west of the central Arctic (94°N, 90°W, and 84°N, 90°E), which could characterize the intensity of TPD. In the BG region, sea ice motion is mainly driven by the anticyclonic circulation, so CAI mainly affects the longitude of the sea ice trajectory. In the TPD region, sea ice mainly advects meridionally, so CAI affects the latitude of the ice trajectory more significantly. We further improved the expression to make it clearer in the revised manuscript (**lines 407-408, 419-420**).

**Specific Comments:**

Line 23: change to "as the sea ice thins"

Line 117: use "optimal" instead of "most optimal"

Line 347: "form" to "from"

**Reply:** We have revised them (**lines 23, 131 and 355**) according the comments.

Line 308-309: I am not sure about the statement since we don't know the casuality between ice motion, wind circulation, near surface ocean current/stress. It is truly that sea ice motion, wind speed, ocean surface stress increase with climate change, but correlation doesn't give us some ideas in who is the trigger and who is the influencer. Could you provide more evidence.

**Reply:** Thanks, we removed this inaccurate statement and revised the relevant text (**lines 343-348**).

Line 393-395, can you explain why?

**Reply:** We added text to explain why the BH does not reveal more effective interpretability in the BG region (**lines 422-424**).