Review of "Estimation of duration and its changes in Lagrangian observations relying on ice floes in the Arctic Ocean utilizing sea ice motion product"

The manuscript presents an analysis of potential durations and changes in Lagrangian observation trajectories in the Arctic Ocean using sea ice motion (SIM) products from 1979 to 2020. The authors propose a reconstruction method to track synthetic buoys and analyze long-term trends in the survivability of sea ice-based platforms. The work is particularly relevant in the context of planning future Arctic field campaigns, where floe lifetime and trajectory uncertainty are critical.

The manuscript includes a substantial amount of data analysis, including validation against real buoys and exploration of relationships between trajectory duration and climate indices. However, many of the core scientific and methodological concerns raised in the first round have not been sufficiently addressed. Below, I detail unresolved issues, expanded requests, and additional suggestions for a more rigorous and impactful revision. I recommend **minor revisions**.

General Comments:

1. The manuscript continues to define synthetic deployment points within a fixed rectangular AOI in the central Arctic Ocean. This choice is not convincingly justified, especially given that the study emphasizes EEZ constraints and international boundary considerations later in the paper. Why not define initial locations based on EEZ boundaries or current common deployment areas (e.g., MOSAiC, N-ICE2015)? The reader is still left wondering whether the motivation is to assess *ideal* deployment zones within political constraints, or simply to map climatological drift patterns in a limited domain. These are fundamentally different objectives and should be clearly separated and addressed.

2. The manuscript still does not adequately explain how sea ice motion vectors are interpolated onto buoy positions during Lagrangian tracking. Is the interpolation linear? Bilinear? IDW? Are velocity fields regridded before or during integration? Moreover, the paper should comment on whether the use of a fully Lagrangian approach (i.e., step-by-step advection) could be compared to semi-Lagrangian methods, or if any correction is made to account for accumulated drift bias over long periods.Reliance on low-resolution, outdated SIM data undermines the validity of conclusions.

3. The study relies solely on the NSIDC Polar Pathfinder 25 km product, which is known to underestimate short-term variability and to smooth dynamic features relevant for station-keeping and observational fidelity. No comparison is made to higher-resolution products (e.g., OSI SAF 6.25 km, MEEREIS), nor are any bias assessments provided. Without this, conclusions about changes in survivability or trajectory characteristics over time are difficult to evaluate.

4. Trajectory validation continues to rely on a small number of buoys, mostly post-2014. The use of earlier IABP buoys or additional campaign data (e.g., SHEBA, DAMOCLES) could significantly strengthen the credibility of results. Moreover, distance between reconstructed and real trajectories is reported, but no error metrics such as RMSE, angular deviation, or trajectory similarity (e.g., Fréchet distance) are provided. This is especially important since the method is used to infer survivability duration over multi-month periods. Also, backtracking analysis of long-duration (>9 months) trajectories would provide insight into which ice origins produce the most stable paths. This could help refine AOI definitions or identify zones of persistent ice retention. Unfortunately, this suggestion remains unaddressed, despite being a straightforward addition that could substantially improve the paper's utility.

5. Section 3.3 remains somewhat abstract and disconnected from the core analysis. The discussion of drift trends and sea ice circulation patterns is mostly descriptive and lacks linkage to either the survivability calculations or campaign implications. Consider condensing or more tightly integrating this section with the trajectory duration results.

6. Section 4.1: no sensitivity or uncertainty analysis. The survivability results are presented without any robustness testing. How sensitive are results to the time of year, small changes in starting position, or minor variations in drift vector? Even a basic bootstrapping or ensemble test would help confirm that the observed patterns are not artifacts of the interpolation method or limited starting conditions.

7. Table 1: interpretation of CAI correlations still vague. The correlation analysis between duration and atmospheric circulation indices is repeated from the original version, with no added clarity. For example, why does autumn CAI correlate with longitude in BH but not IPD? What does the correlation in latitude for both suggest about the spatial mode of influence? Without physical interpretation, the correlation analysis is not actionable and feels disconnected from the rest of the study.

Specific Comments:

Eq. (1-3): Clarify the interpolation technique and its potential accumulation of drift error over time. Describe how errors in SIM data are propagated in trajectory reconstructions. Include justification for using 25 km resolution and for excluding any higher-res or recent products.

Table/Figure 3: Add quantitative error metrics. Distance alone is insufficient, what are direction errors, turning biases, etc.? Expand comparison beyond one buoy trajectory (e.g., include MOSAiC or additional IABP floats).

Why limit the results fixed the time span between 1979-2020 instead of 1979-2024?