We would like to express our sincere gratitude to both reviewers and the editor for their insightful and constructive comments. Your detailed feedback has been invaluable in identifying key areas for improvement and will significantly enhance the structure and impact of our paper. We greatly appreciate the time and expertise you dedicated to reviewing our work.

We have implemented several changes in our revised version, including a more focused data analysis, a new structure, and extended discussions on the points raised in both reviews.

The main changes in our manuscript can be summarized as follows:

The areas of investigation were expanded by analyzing also smaller regions, using a higher spatial resolution and putting more emphasis on coastal processes.

We also extended the target variable (lead frequency) by specific polynya frequencies using a published data set to account for potential deficits of the leads-only data set in representing coastal process and to assess potential benefits from using a combined data set.

In the predictors list, we removed 2m air temperature and sea-ice concentration as they mostly represent proxies for other predictors and do not really contribute to explaining individual forcings for lead formation.

Finally, structure and analysis have changed substantially in the current version, where we tried to account for all of the reviewer's comments and suggestions.

Response to Referee 1 Comments

We show referee comments in black text, our response in blue and changes inserted to the manuscript are put in *blue italics*.

This manuscript applies a random forest regression framework to reconstruct and interpret wintertime Southern Ocean lead frequency (LF) using a gap-filled monthly lead-frequency product (Dubey et al., 2025a) together with a set of atmospheric, sea-ice kinematic, and ocean current predictors.

The study of Dubey et al. (2025a) itself is highly valuable, and the dataset produced in that work represents an important contribution to the community. Compared with that work, however, the contribution of the present manuscript appears more limited and gives the impression of a preliminary or exploratory application of the method. The identified top predictors appear to be only apparent or indirect drivers, and the paper provides relatively limited new physical insight into the mechanisms controlling lead variability.

Using a random forest approach for this type of problem is potentially meaningful. However, for publication as a full paper, the study requires more in-depth analysis. The choice of predictors and the way they are treated should also be reconsidered. In particular, leads are small-scale phenomena, yet the analysis is conducted at a very coarse grid resolution (2° × 5°). In addition, further clarification and discussion are needed regarding the interpretation of the dominant predictor (2 m air temperature) and the regional classification based solely on longitude sectors.

For these reasons, I believe that substantial revision and additional analysis are required before the manuscript can be considered for publication in The Cryosphere.

We really appreciate your thoughtful comments and suggestions, which have helped us improve the clarity and overall quality of the paper. We have carefully considered and addressed each of your points.

Major comments

1. Need to address coastal polynyas explicitly

From reading the Introduction, the discussion includes not only leads but also the role of coastal polynyas. However, the term "coastal polynya" is not explicitly used in the manuscript, and leads appear to be treated in a way that implicitly includes coastal polynyas.

Although leads and coastal polynyas cannot always be strictly separated, the text should consistently refer to them as "leads and coastal polynyas..." if both are included in the analysis.

Furthermore, from examining Dubey et al. (2025a), it appears that coastal polynyas forming along ice shelves or along landfast ice may also be included in the lead counts. If this is the case, the manuscript should explicitly state that open water or thin ice regions forming along landfast ice, which are commonly referred to as coastal polynyas, are included in this study and treated as leads.

Some clarification on this point is necessary.

We thank the reviewer for this helpful suggestion. In the revised manuscript, we now explicitly make sure that the lead frequency (LF) target variable incorporates both pack-ice leads and coastal polynyas, and we consistently use the terminology "leads and coastal polynyas." In Sect. 2.1, we have added:

" To provide a more complete representation of wintertime open-water activity along the Antarctic coastal margins, the coastal polynya dataset of Lin et al. (2024), was incorporated into the LF target variable. This dataset provides daily records of coastal polynya areas around Antarctica derived from passive microwave satellite observations, and is used here to supplement the MODIS-derived LF, which does not capture larger open water or thin ice areas explicitly. A monthly polynya frequency is therefore calculated based on the daily binary data. The year 2012 is excluded from the analysis because the polynya dataset is not available for that year. The combined dataset includes both pack-ice leads and coastal polynyas, enabling a more physically complete analysis of wintertime lead variability across the Southern Ocean. We provide results, however, also for the leads-only data set to highlight the specific benefits that the polynya dataset can contribute "

2. Differences between coastal and offshore regions, especially the role of landfast ice

Based on Dubey et al. (2025a), many leads appear to occur near the coast, particularly along the edge of landfast ice. In the present study, regional divisions are made only by longitude sectors. However, it seems reasonable to expect that lead variability differs between coastal regions and offshore pack ice. Despite this, no such regional distinction is made in the analysis. I suggest reconsidering the regional classification. In particular, leads near the coast are likely strongly influenced by the presence and variability of landfast ice. However, the manuscript contains no discussion of the relationship between leads and landfast ice. The landfast ice dataset of Fraser et al. (2020) is openly available and could be used in the analysis. Even if the dataset is not used directly, some discussion of the potential role of landfast ice would be appropriate.

Fraser, A. D., et al., 2020: High-resolution mapping of circum-Antarctic landfast sea ice distribution, 2000–2018. Earth System Science Data, 12, 2987–2999.

We agree with this important observation. In the revised manuscript, we have added a dedicated coastal analysis section (Sect. 3.5) that explicitly addresses the coastal regions. Furthermore, we now include a distance-from-coast analysis (Sect. 3.6) that quantifies how model skill and the dominant predictor importance vary systematically across four distance categories from the Antarctic coast. Regarding landfast ice, we acknowledge its potential role in modulating coastal lead variability. While the direct inclusion of the Fraser et al. (2020) landfast ice dataset is beyond the scope of the present study, we have added a discussion of its potential role in Sect. 4 and identified it as an important avenue for future work:

"A further avenue for improvement is to explicitly account for the circum-Antarctic landfast ice regime in which many coastal leads and polynyas are embedded. Fraser et al. (2020) provide a pan-Antarctic fast-ice dataset for 2000–2018, and Fraser et al. (2021) identified eight regional fast-ice regimes with distinct trends driven by bathymetry and grounded icebergs. Incorporating fast-ice extent or persistence from these products in our RF framework could help to better distinguish coastal dynamical regimes and to explain part of the residual variance in LF along the Antarctic margins."

3. Causal interpretation of the dominance of 2 m air temperature

From a physical perspective, lower 2 m air temperature should promote freezing of open water and therefore act to close leads. However, the analysis indicates the opposite: lower air temperatures are associated with increased lead frequency, and this variable is identified as the top driver. The paper later argues that this reflects a proxy relationship with cold offshore winds. In other words, the predictor is not a direct physical driver but rather an indirect or apparent one. A conclusion in which an apparent or indirect predictor becomes the top driver unfortunately weakens the physical significance of the result. That said, such outcomes can occur in statistical analyses, and I do not dispute the result itself. However, in this case, the interpretation requires stronger support. The manuscript should provide clearer justification for this proxy interpretation and discuss whether it is possible to remove or isolate the apparent effect. As suggested in comment 2, separating coastal and offshore regions may help address this issue.

We thank the reviewer for this observation, which has led to an improvement in the predictor selection. In the revised manuscript, we have removed 2 m air temperature and sea-ice concentration from the predictor set entirely. Instead, we now use the zonal (u) and meridional (v) 10 m wind components as explicit directional wind predictors. This change directly addresses the reviewer's concern by replacing an indirect proxy predictor with physically more direct and interpretable wind-forcing variables.

With this revised predictor set, the zonal u wind emerges as the most influential predictor (17.7%) at the pan-Antarctic scale, followed by ocean current speed (13.9%), wind speed (12.9%), meridional v wind (12.2%), and ice divergence (11.2%). This substitution not only improves the physical interpretability of the results but also improves regional model performance, particularly in the Indian Ocean (r = 0.82) and Pacific Ocean (r = 0.79) sectors.

4. Gap between the spatial scale of leads and the analysis grid scale

As the authors themselves note, leads are small-scale phenomena. The original data used in the study have a spatial resolution of about 1 km^2, yet the analysis is conducted on a much coarser grid of 2° × 5°. The authors acknowledge that such aggregation smooths bathymetrically controlled hotspots and narrow coastal leads. Because many leads are controlled by coastal divergence, tides, and shelf-break dynamics, coarse resolution may bias the apparent importance of predictors away from kinematic drivers and toward broader thermodynamic patterns. Please discuss more explicitly how coarse gridding may suppress mechanical deformation signals and alter the apparent ranking of predictors. If possible, I would appreciate seeing a supplemental analysis at higher resolution for a subset region or time period to demonstrate the scale dependence of the results.

To address this scale issue, we have added a dedicated coastal analysis in Sects. 3.5 and 3.6, which is conducted based on a 10 × 10 km^2 grid for both target and predictors and focuses on exemplary coastal regions. This added analysis highlights how model skill and the relative importance of predictors vary in coastal and shelf-break regions.

5. Unnecessary figures and analysis

Figures 4 and 9 should be removed. The relative importance of predictors is already shown in Figures 5 and 10, and these figures add little additional insight. Moreover, the results depend on the order in which predictors are added, and it is not clear why the present order was chosen. Removing these figures would not cause any essential loss to the paper. As noted elsewhere, there are several areas where additional analysis would be more useful. Therefore, these figures and the associated explanation should be removed in favor of more meaningful analysis.

We thank the reviewer for this suggestion and acknowledge the concern about order-dependence. The incremental analysis (Sect. 3.2, Fig. 4) serves a distinct and complementary purpose to the permutation-based feature importance analysis: while feature importance quantifies the independent contribution of each predictor within the full model, the incremental analysis reveals the marginal gain from adding each predictor group and identifies the minimum predictor set required for skillful LF reconstruction. This information is practically useful for future applications where data availability may limit the choice of predictors.

In response to this comment and the equivalent remark from Reviewer 2 (General Point 6), we have substantially revised Sect. 3.2 to be more concise and focused. The original Figure 9 showing monthly performance for June has been removed and replaced by new analysis Sect. 3.5 (Model performance in exemplary sub-regions) and Sects. 3.6 (Coastal influences), which we believe provide more meaningful insight.

Minor comments

6. Description of the LF dataset

P3:"This study uses the monthly LF dataset by Dubey et al. (2025b) ..."

It would be helpful to include a slightly more concise explanation of this dataset within the paper.

Done. A concise description of the LF dataset has been added in Sect. 2.1:

" Monthly LF data are based on thermal infrared satellite imagery, which was used to detect leads as surface temperature anomalies in cold winter sea ice (Reiser et al., 2020). Monthly LF values were computed from daily lead observations as the fraction of clear-sky days during which a grid cell was identified as a lead (Dubey et al., 2025a)"

7. Use of climatological sea-ice concentration during AMSR-E/AMSR2 data gap

P4:"For April to June 2012 ... we use the mean sea-ice concentration ..."

Using climatological values during this period seems inappropriate. The analysis focuses on interannual variability, and therefore replacing missing data with climatology may distort the results.

Instead, it would be preferable either to use SSM/I data or to exclude this period from the analysis.

In the revised manuscript, sea-ice concentration has been removed from the predictor set entirely. The revised predictors no longer rely on any climatological substitution for missing data.

8. Reliability of the ocean current dataset

P4: Ocean surface current speed data were obtained from ORAS5..."

Ocean surface currents in the Southern Ocean are still poorly constrained. I am not convinced that this dataset reliably represents the variability of ocean currents on monthly and interannual timescales.

Datasets such as B-SOSE may be more appropriate. If the authors wish to use ORAS5, they should provide justification for its reliability in this context. Personally, I would suggest excluding this predictor rather than using highly uncertain data. Ocean surface currents are largely determined by winds and sea-ice conditions, which are already included as predictors.

We acknowledge that ORAS5 may not perfectly represent all aspects of current variability. However, ORAS5 is one of the most widely used and validated ocean reanalysis products available for the Southern Ocean, with documented skill in reproducing large-scale current patterns including the Antarctic Circumpolar Current, Weddell and Ross Gyre circulations, and the Antarctic Slope Current. We have added a brief discussion of ORAS5 limitations in Sect. 4:

" Beyond landfast ice, both the target variable and predictor fields carry some limitations that may limit model performance. Among predictors, ORAS5 surface currents are likely subject to some uncertainties in the thin, seasonally ice-covered coastal ocean, where observational constraints are sparse and small-scale bathymetric features, tides and mixing are not fully resolved in the reanalysis (Zuo et al., 2019). Future studies could explore alternative products such as the Biogeochemical Southern Ocean State Estimate (B-SOSE; Verdy and Mazloff, 2017), which provides higher-resolution (~18 km) estimates of Southern Ocean circulation. Additional oceanic predictors, such as ocean current divergence and eddy kinetic energy, represent an important objective for future study, particularly in regions of rough topography and at shelf breaks where the model currently shows comparatively lower skill."

9. Definition of ice divergence and wind divergence

Please clarify how ice divergence was calculated when grid cells include land or coastline. For example, offshore ice drift near a coast can produce strong divergence along the coast. Were coastline or topographic constraints considered when calculating ice divergence? If not, I recommend recalculating divergence while accounting for land boundaries. For wind divergence, it may be acceptable to compute divergence directly from wind fields without considering topography, although clarification would still be helpful.

Ice divergence was calculated from the ice velocity components using centered finite differences. Grid cells containing land or coastline were masked using the land-sea mask of the analysis grid prior to the divergence calculation, ensuring that land contamination did not artificially inflate divergence values near the coast. The following clarification has been added in Sect. 2.2:

"Ice divergence was calculated from the ice velocity components using centered finite differences applied to the gridded NSIDC Polar Pathfinder ice motion product. Grid cells containing land or coastline were masked using the land-sea mask of the analysis grid prior to the divergence calculation, ensuring that land contamination did not artificially inflate divergence values near the coast. For wind divergence, the ERA5 10 m wind divergence field was used directly from the reanalysis product. "

10. Sign of relationships between predictors and LF

Figures 5 and 10 show the relative contribution of each predictor to LF variability. However, it is not clear whether the relationships are positive or negative. For example, I initially assumed that higher 2 m air temperature would increase LF, but Section 4.2 later shows that the relationship is actually inverse. Similarly, the sign of relationships for SLP, ocean current speed, and ice velocity is not immediately obvious. At the beginning of the results section, the manuscript should clearly indicate whether each predictor has a positive or negative relationship with LF.

We thank the reviewer for this point. We note that the RF features importance framework, by design, quantifies only the magnitude of each predictor's contribution to model skill, not the sign or direction of its relationship with the target variable. Providing signed relationships would rely on linear multi-variate relationships. The strength of the RF model, however, is that it also able to reconstruct non-linear structures. To avoid ambiguity, we have mentioned this to Sec. 2.4 to clarify this upfront for the reader.

11. Mismatch in Ross Sea (Fig. 8e)

In Fig. 8, the mismatch between observations and predictions appears particularly large in the Ross Sea (Fig. 8e). What causes this discrepancy? If a clear explanation is not already provided in the manuscript (I may have overlooked it), it should be discussed.

We note that the figure referred to Fig. 8 in the original manuscript, which showed month-specific sample data comparison of observed versus predicted LF for June (2003–2023) across regional sectors, has been removed from the revised manuscript.

References:

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