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 (polynya-scale) 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 (LF), enabling a more physically complete analysis of wintertime lead variability across the Southern Ocean. We provide results, 3 however, also for the leads-only data set (LF without polynyas, LFWP) 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 (Lead drivers 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. Amongpredictors, ORAS5 surface currents are likely subject to 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 studies, 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. 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:

Dubey, U., Willmes, S., and Heinemann, G.: Southern Ocean sea-ice leads: first insights into regional lead patterns, seasonality, and trends, 2003–2023, The Cryosphere, 19, 3535–3552, https://doi.org/10.5194/tc-19-3535-2025, 2025a.

Dubey, U., Willmes, S., and Heinemann, G.: AntLeads: Monthly wintertime sea-ice lead maps for the Antarctic, April-September, 2003–2023, https://doi.org/10.1594/PANGAEA.977634 , 2025b.

Fraser, A. D., Massom, R. A., Ohshima, K. I., Willmes, S., Kappes, P. J., Cartwright, J., and Porter-Smith, R.: High-resolution mapping of circum-Antarctic landfast sea ice distribution, 2000–2018, Earth System Science Data, 12, 2987–2999, https://doi.org/10.5194/essd-12-2987-2020 , 2020.

Fraser, A. D., Massom, R. A., Handcock, M. S., Reid, P., Ohshima, K. I., Raphael, M. N., Cartwright, J., Klekociuk, A. R., Wang, Z., and Porter-Smith, R.: Eighteen-year record of circum-Antarctic landfast-sea-ice distribution allows detailed baseline characterisation and reveals trends and variability, The Cryosphere, 15, 5061–5077, https://doi.org/10.5194/tc-15-5061-2021, 2021.

Lin, Y., Nakayama, Y., Liang, K., Huang, Y., Chen, D., and Yang, Q.: A dataset of the daily edge of each polynya in the Antarctic, Scientific Data, 11, https://doi.org/10.1038/s41597-024-03848-2 , 2024.

Reiser, F., Willmes, S., and Heinemann, G.: A new algorithm for daily sea ice lead identification in the Arctic and Antarctic winter from thermal-infrared satellite imagery, Remote Sensing, 12, 1957, https://doi.org/10.3390/rs12121957 , 2020.

Verdy, A. and Mazloff, M. R.: A data assimilating model for estimating Southern Ocean biogeochemistry, Journal of Geophysical Research: Oceans, 122, 6968–6988, https://doi.org/10.1002/2016jc012650 , 2017.

Zuo, H., Balmaseda, M. A., Tietsche, S., Mogensen, K., and Mayer, M.: The ECMWF operational ensemble reanalysis–analysis system for ocean and sea ice: a description of the system and assessment, Ocean Science, 15, 779–808, https://doi.org/10.5194/os-15-779-2019 , 2019.

Response to Referee 2 Comments

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

The manuscript presents a novel approach to determine the factors controlling the winter lead frequency in the Southern Ocean from a recent satellite product using random forest regression with permutation importance analysis. Based on this analysis, the authors identify the main predictors for lead frequency for the whole Southern Ocean and separately for the different basins, and discuss the associated mechanisms. This provides very useful information and the study is thus of great interest. However, additional information would be required before publication, in particular in the methodology and the impact of some of the modelling choices, as detailed below.

Your comments are valuable and will help improve the quality of the study. We have carefully considered and addressed each of your points.

General points

1. The manuscript should explain more explicitly the difference between lead frequency and ice concentration. The introduction discusses in general the impact of leads and opening within the ice pack, including studies that use ice concentration (from satellite products and models) as a measure of the surface occupied by leads. Naively, one could have expected that lead frequency and ice concentration would have been related. However, this does not seem to be the case, except in a few regions (e.g. Table 1). That would be useful if the authors explain why this naïve view is not valid.

   It is correct. Sea-ice concentration during wintertime is actually determined by the presence of leads. The problem is that operational SIC products (mostly passive-microwave) are generally too coarse to resolve leads themselves. In the revised manuscript we have therefore removed SIC as a predictor because it does not represent a physical driver for lead formation but rather an indirect proxy for their presence.

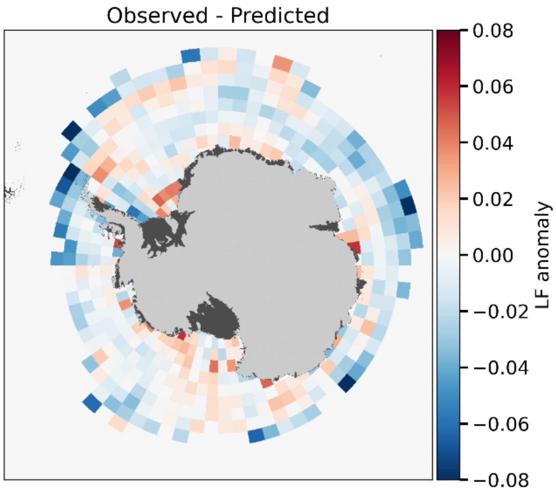
2. It may be hard to evaluate the model performance from the absolute numbers that are provided. Would it be possible to compare the model performance to the one of a much simpler model, assuming for instance a constant lead frequency in each basin or a climatological lead frequency at each point ?

   We appreciate this suggestion. While we have not introduced an explicit climatological baseline model in the revised manuscript, we have now added a coastal analysis section (Sects. 3.5 and 3.6) on a finer 10 × 10 km^2 scale that explicitly addresses the coastal basins, and it also compare results for LF (leads including coastal polynyas) with LFWP (leads without coastal polynas), where the polynya dataset is omitted. This section shows that the RF model captures variability beyond what a simple climatological field could explain, especially in coastal and shelf-break areas.

3. The regions where the differences between observed and predicted lead frequency are the largest are not easy to find from figure 1. From figure 2, it is clear that the predictions

are biased for large values of the lead frequency but we cannot see on Figure 1 to which region this corresponds. Would it be possible to plot the difference between observed and predicted lead frequency in Figure 1 in addition to the absolute values ? This would facilitate the discussion. Additionally, it is mentioned several times in the manuscript that large biases are present close to topographic gradients, in bathymetrically-controlled and coastal zone, at the shelf break, etc. but this is not explicitly shown. Maybe a difference in Fig. 1 will help identifying those zones but an additional diagnostic may be needed to see it.

We have generated the difference map (Observed - Predicted LF), which clearly shows that the largest underpredictions are concentrated along the Antarctic coastal margins, particularly in the Weddell Sea, Prydz Bay, and Ross Sea coastal zones, while the offshore pack ice shows modest overprediction. However, we have not added this as a third panel to Figure 1, because the spatial bias pattern is already addressed quantitatively through the dedicated coastal analysis in Sect. 3.5 (Fig. 8) and the distance-from-coast analysis (Sect. 3.6), which together provide a more systematic characterization of where model skill is highest and lowest.



4. The random forest model is based on monthly mean values at a relatively coarse resolution (2° latitude × 5° longitude grid). This can be justified to reduce the amount of data that has to be processed. However, lead opening and closing could occur at shorter timescales and the link with some atmospheric variables could be obscured by monthly averages. For instance, we can imagine that the link between horizontal wind divergence and lead opening is very strong during the passage of a storm but weak at lower frequency. If possible, repeating some of the diagnostics (like Figure 2 and maybe Figure 5 if possible) at two different resolutions would allow testing this. If it is too time demanding, at least a discussion of the possible impacts of the resolution would be needed.

   We agree that using monthly means on a 2° × 5° grid can obscure part of the event-scale link between atmospheric forcing (e.g. storm-driven divergence) and lead opening. To partially address this, we emphasize that the coastal analysis is carried out on a finer 10 × 10 km^2 grid of the LF data and focuses on key coastal boxes (Sects. 3.5 and 3.6, Figs. 7–11). A full repetition of the RF diagnostics at higher temporal or event-scale resolution does not allow for a high resolution of predictors and is also beyond the scope of this study. We now explicitly highlight RF experiments using higher-resolution, event-scale forcing fields, including climate modes, as a priority for future work.

"Understanding the connection between lead variability, coastal polynya activity, short-lived atmospheric events, and slow, pan Antarctic scale climate shifts including climate modes therefore remains an important avenue for future work, particularly in the context of the variability observed in Antarctic sea-ice since 2016."

5. The contribution of ocean processes to explain the lead frequency is relatively limited but only one oceanic variable is used (ocean current). Why not using more oceanic variables (like for instance the ocean current divergence – equivalent for the ocean to wind divergence) ? Eddy kinetic energy could also be a good candidate as the authors mention the role of the eddies in the discussion and eddy kinetic energy can be large in the region of rough topography and at the shelf break, regions where the model performance is apparently low. Another advantage is that, in contrast to individual eddy that are at a smaller scale than the one used in the model, eddy kinetic energy can be interpolated at the scale of the model. Alternatively, if bathymetry is so important, it could also be a good predictor. This is maybe too much additional work for the current manuscript but the possibility that the ocean role is underestimated due to the limited number of variables used could be mentioned.

   We appreciate this suggestion. We note that some aspects of eddy-driven deformation are already implicitly reflected in our ice-kinematic predictors, in particular ice divergence and ice velocity, which respond to mesoscale ocean features where sea-ice is sufficiently mobile. Nevertheless, we highlight the inclusion of oceanic predictors such as ocean current divergence, and eddy kinetic energy as priorities for future work in Discussion:

   "Additional oceanic predictors, such as ocean current divergence and eddy kinetic energy, represent an important objective for future studies, particularly in regions of rough topography and at shelf breaks where the model currently shows comparatively lower skill."

6. The added value of incremental predictor contribution (3.2) compared to the relative importance (3.3) is not clear. Is the relative contribution in the incremental method depending on the order in which the variables are added (while it is not the case for the relative contribution) ? Are they elements that are clear in 3.2 and not in 3.3 ? If this is the case, this should be more clearly highlighted or alternatively section 3.2 could be suppressed or put in supplement.

   We have substantially revised Sect. 3.2 to be more concise and to clearly articulate what it contributes beyond Sect. 3.3. The two analyses serve distinct and complementary purposes: permutation importance (Sect. 3.3) quantifies the independent contribution of each predictor within the fully specified model, while the incremental analysis (Sect. 3.2) reveals the marginal gain from sequentially adding predictors.

Specific points.

1. Line 32. The formulation can be interpreted as if ice divergence was an atmospheric process only.

   Corrected.

2. Lines 73-83. The introduction is nice and reviews well the different processes. However, the last paragraphs of the introduction are mainly a description of the work that has been done while the specific motivation of the study is not yet clear and this could be developed more at this stage.

   We thank the reviewer for this observation. In the revised version, the introduction is now more concise, and we explicitly highlight the knowledge gap and the specific objectives of our work, which we hope makes the motivation of the study clearer to the reader.

   We added: *"While several studies have described lead distributions and temporal variability in both polar regions (Wang et al., 2016; Reiser et al., 2019; Willmes et al., 2023; Dubey et al., 2025a), a comprehensive quantitative framework for identifying and ranking the physical drivers, and quantifying the relative importance of atmospheric, oceanic, and ice-dynamic controls on Southern Ocean leads and coastal polynyas is still lacking. This knowledge gap limits the representation of lead/coastal polynya related processes in coupled climate models and limits our capacity to assess how future changes in atmospheric or oceanic forcing might influence lead dynamics and their feedback on the polar climate system."*

3. Line 144. If it is standard recommendation maybe adding a reference would be useful ?

   Done.

4. Line 257-258. The first sentence is very general and could be suppressed.

   Done.

5. Is the Section 3.4 adding new information or could it be moved to the supplement for instance ?

   We appreciate the reviewer for raising this point. In the revised manuscript, the original Sect. 3.4 (which presented the monthly model performance for June) has been removed entirely and replaced by new analysis Sect. 3.5 titled "Lead drivers in exemplary sub-regions" and further with Sect. 3.6 " Coastal influences".

6. Line 241. The discussion about whole number percentage is confusing while on Figure 5 and in most of the text the number are given with one decimal. Why not giving all the numbers with one decimal ? Same issue line 311.

   Done. All percentage values throughout the manuscript have been standardized to one decimal place.

7. It not clear what is shown on Figure 8 and what is meant by sample-by-sample comparison? Is it the actual lead frequency and the predicted lead frequency on the same x-point ? If this is the case, what is the added information compared to Figure 7?

   The original Fig. 8 showing the sample-by-sample comparison has been removed in the revised manuscript now. In its place, the new Fig. 8 presents scatterplots of LF (leads with polynyas) and LFWP (leads without Polynyas) observed versus predicted monthly mean lead frequency, providing a clear and directly interpretable assessment of model performance in each coastal region.

8. Lines 396-399: Is there a good way to test the hypothesis of 2m temperature acting as proxy for offshore winds? Maybe by testing the correlation between temperature and wind speed/direction or something along those lines?

   In the revised manuscript, this point is no longer relevant because we have removed 2 m air temperature from the predictors. Instead, we now use the zonal u and meridional v wind components. These wind components act as direct physical drivers of lead formation, so the earlier concern about T2m being only an indirect proxy no longer applies.

9. Line 374. The previous paragraph states that 2m air temperature is the main driver. How this finding is it ‘consistent with earlier work’ which shows that Antarctic leads tend to occur in area influenced by bathymetric steering ? Maybe ‘this’ refers to something else ?

   This apparent inconsistency arose from the previous version of the manuscript where T2m was the top predictor. With T2m removed from the predictor set in the revised manuscript, the zonal u wind now emerges as the primary pan-Antarctic driver of lead formation. The relevant passage has been revised accordingly and the original inconsistency no longer exists.

10. Line 381. That would be useful to give the coordinates of Maud Rise and Gunnerus Ridge (or to show them on a figure) and maybe show the biases of the model for those points ?

    The approximate coordinates of Maud Rise (~66°S, 3°E) and Gunnerus Ridge (~67°S, 34°E) have been added in the revised text where these features are first mentioned in Sect. 1.

11. Line 471. The link with ‘bathymetric contrasts’ is not clear here.

    We thank the reviewer for noting the unlink reference. In the revised manuscript, we have updated the Discussion to reflect the new analyses, and this sentence has been rephrased so that the ambiguous link to “bathymetric contrasts” is removed.

12. Lines 475-478: If r=0.73 shows comparatively higher model skill, what is it in comparison to? And, if the Ross sea has many events that are on sub-monthly scales that the model can’t account for, then shouldn’t it result in lower model skill?

We acknowledge that the original text was misleading in describing the Ross Sea as showing "comparatively higher model skill". In the revised manuscript, the Ross Sea correlation of r = 0.73 is now described in the context of the full range of regional correlations (0.63–0.82), noting that the Ross Sea shows moderate skill comparable to the Weddell Sea, rather than being described as comparatively higher.

13. Line 506. Is this indirect dynamical effect due to the resolution of the reanalysis (and the spatial resolution of the model) that is not able to reproduce the winds and their divergence at the scale that matters for leads opening ? (see also the general point 4 above).

    This point is no longer directly relevant in the revised manuscript, as 2 m air temperature has been replaced by the directional wind components, removing the need to interpret an indirect dynamical effect through temperature.