Responses to Reviewer #1:

Dear Reviewer:

Thank you for your careful reading of our manuscript and constructive suggestions that have contributed to improve it substantially. We appreciate the critical issues you have raised regarding the methodology and interpretation. Following your suggestions, we have carefully revised the manuscript. We have clarified the limitations of the model output, added sensitivity tests to justify the choices of preprocessing and UMAP/DBSCAN parameters, and revised the naming and interpretation of the former "Coast" regime. Detailed responses to each comment are provided below in blue font.

Jinfei Wang
On behalf of all the authors

General Comment:

1. My first and major concern is: the author acknowledges that the model underestimates SIC in some regions, overestimates it elsewhere, and generally underestimates SIT compared with GIOMAS. And that mean the derived thermodynamic and dynamic mass budget components may also be biased, right? Then, it is possible that resulting clusters may simply reflect model structural errors rather than physically meaningful Antarctic sea ice regimes. And more details questions are: do we expect the biased input can have the physical sea ice processes understanding? How sensitive are the identified regimes to known NEMO-SI3 biases in SIC and SIT? Later in discussion, I saw authors also mention that "Coast" regime is not always along the coasts, could it be a model artifact caused by biases in sea ice motion, thickness, or thermodynamic growth in the NEMO-SI3?

Response: Thank you for this comment. We acknowledge that the simulated thermodynamic and dynamic mass budget terms may be biased regardless of how well SIC and SIT are reproduced. Previous studies have shown that even realistic simulations of sea ice area or thickness may result partly from compensating errors between dynamic and thermodynamic processes (Uotila et al., 2014; Nie et al., 2022). We apologize for not acknowledging this limitation explicitly in the original manuscript.

However, a direct observational validation of all sea ice mass budget terms is not currently feasible because reliable, large-scale observations of these components are not available. To provide the strongest observation-based process evaluation currently feasible, we have added a comparison between an observation-based SIC budget and the corresponding SIC budget diagnosed from the NEMO-SI^3 simulation. The observational budget is calculated from daily NSIDC sea ice concentration and sea ice drift data (Tschudi et al., 2019; DiGirolamo et al., 2022), following the budget equation of Holland and Kwok (2012). The total SIC tendency is separated into a dynamic contribution, calculated from sea ice transport, and a residual thermodynamic contribution. For the model, the total, dynamic, and thermodynamic SIC tendencies are obtained directly from the NEMO-SI^3 diagnostics. Figure R1 presents the March-September climatology of both budgets for 1981-2010.

The observation-derived budget shows that thermodynamic processes dominate SIC growth over the ice pack during growth season. The dynamic contribution is predominantly negative near the coast and extends farther north, particularly in the

Amundsen-Ross Sea sector, and becomes more positive toward the ice edge. These broad spatial characteristics are qualitatively similar to those simulated by NEMO-SI^3, which also shows widespread thermodynamic ice growth, negative dynamic contributions in many coastal and offshore regions, and positive dynamic contributions near the ice edge. Although the values do not agree exactly and some regional discrepancies remain, the large-scale spatial patterns are broadly consistent. This indicates that the occurrence of the "Coast" regime away from the coast is not a model artifact. A similar spatial pattern of dynamic and thermodynamic processes has also been reported in previous studies (e.g., Holland and Kwok, 2012; Holland and Kimura, 2016). This consistency provides observation-based support for the physical plausibility of the dominant processes represented by the model-derived regimes.

In addition, previous studies have used the same NEMO-SI^3 mass budget diagnostics to interpret physical mechanisms underlying Antarctic sea ice variability (e.g., Wang et al., 2024; Richaud et al., 2026). Their findings are consistent with established physical understanding, providing additional support for the physical relevance of these diagnostics.

Despite the potential biases in the model, the rank-based quantile transformation used in the classification reduces sensitivity to spatially uniform biases of the input variables. However, we agree that it cannot remove spatially varying model biases or biases in the partitioning between dynamic and thermodynamic processes. We have therefore acknowledged these limitations explicitly in the revised manuscript. To further clarify the model-based scope of the study, we have also revised the title to "A process-based perspective on **modeled** Antarctic sea ice regimes using objective data mining."

We have removed the original statement, "Overall, the model is adequate to provide sea ice budget components during the growth seasons", and revised the discussion of model performance to provide a more cautious interpretation of the simulated budget terms and to acknowledge the limitations of the model-based approach:

"Previous studies have shown that even realistic simulations of SIC and SIT can result partly from compensating biases in dynamic and thermodynamic processes (Uotila et al., 2014; Nie et al., 2022). Since direct evaluation of these components is currently not feasible, we performed an indirect comparison between an observation derived SIC budget and the corresponding SIC budget diagnosed from NEMO-SI^3. The observational budget was calculated from daily NSIDC sea ice concentration and drift data (Tschudi et al., 2019; DiGirolamo et al., 2022), following the budget equation of Holland and Kwok (2012). Both budgets show positive thermodynamic processes dominating during growth season, negative dynamic contributions in many coastal and offshore regions, particularly in the Amundsen-Ross Sea sector, and more positive dynamic contributions near the ice edge (Figure S6). Although the values do not agree exactly and some regional differences remain, the large-scale spatial patterns are broadly consistent. Similar process patterns have also been reported in previous studies (Holland and Kwok, 2012; Holland and Kimura, 2016) and in studies using NEMO-SI^3 mass budget diagnostics (Wang et al., 2024; Richaud et al., 2026). This qualitative agreement supports the physical plausibility of the simulated processes, though the absolute magnitudes of the individual mass budget terms may still be biased. The regimes should therefore be interpreted within the model framework, while their spatial extent, boundaries, and trends may remain sensitive to model biases."

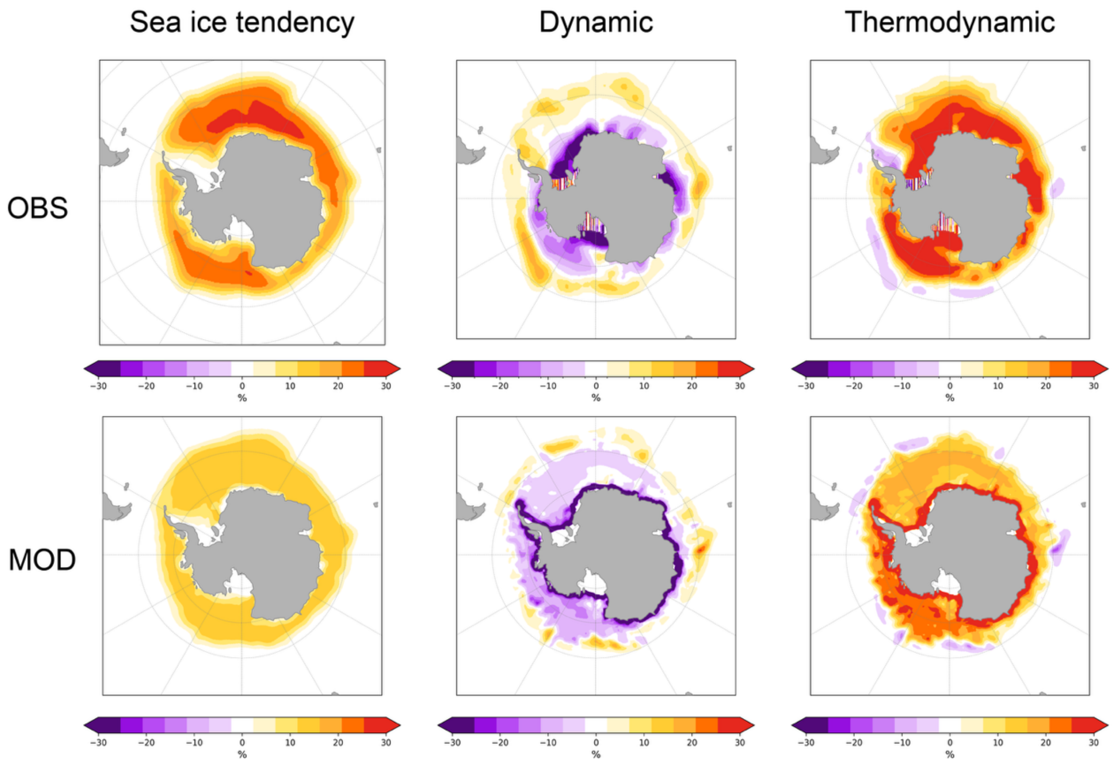

Figure R1: March-September climatology of the sea ice concentration budget over 1981-2010. The first row shows the observation-derived total tendency, dynamic contribution, and residual thermodynamic contribution, calculated from daily NSIDC sea ice concentration and sea ice drift products following Holland and Kwok (2012). The second row shows the corresponding total, dynamic, and thermodynamic contributions diagnosed from the NEMO-SI^3 simulation.

Uotila, P., Holland, P. R., Vihma, T., Marsland, S. J., and Kimura, N.: Is realistic Antarctic sea-ice extent in climate models the result of excessive ice drift?, Ocean Model., 79, 33–42, https://doi.org/10.1016/j.ocemod.2014.04.004, 2014.

Nie, Y., Uotila, P., Cheng, B. et al.: Southern Ocean sea ice concentration budgets of five ocean-sea ice reanalyses, Clim Dyn, 59, 3265–3285, https://doi.org/10.1007/s00382-022-06260-x, 2022.

Holland, P. R. and Kimura, N.: Observed Concentration Budgets of Arctic and Antarctic Sea Ice, Journal of Climate, 29, 5241–5249, https://doi.org/10.1175/JCLI-D-16-0121.1, 2016.

Holland, P. R. and Kwok, R.: Wind-driven trends in Antarctic sea-ice drift, Nature Geosci, 5, 872–875, https://doi.org/10.1038/ngeo1627, 2012.

Wang, J., Massonnet, F., Goosse, H., Luo, H., Barthélemy, A., and Yang, Q.: Synergistic atmosphere-ocean-ice influences have driven the 2023 all-time Antarctic sea-ice record low, Commun Earth Environ, 5, 415, https://doi.org/10.1038/s43247-024-01523-3, 2024.

Richaud, B., Massonnet, F., Fichefet, T., Topál, D., Barthélemy, A., and Docquier, D.: Anatomy of Arctic and Antarctic sea ice lows in an ocean–sea ice model, The Cryosphere, 20, 791–810, https://doi.org/10.5194/tc-20-791-2026, 2026.

Tschudi, M., Meier, W., Stewart, J., Fowler, C., and Maslanik, J.: Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors, Version 4, https://doi.org/10.5067/INAWUWO7QH7B, 2019.

DiGirolamo, N., Parkinson, C., Cavalieri, D., Gloersen, P., and Zwally, H.: Sea Ice Concentrations from Nimbus-7 SMMR and DMSP SSM/I-SSMIS Passive Microwave Data, Version 2, https://doi.org/10.5067/MPYG15WAA4WX, 2022.

2. My second major concern is about the use of the quantile transformer and how this scaling affects the physical meaning of the input data. The authors mention that they tested several scaling approaches and found that the quantile transformer gave the most robust and consistent results. Why is the quantile transformer the best choice for sea ice mass-budget terms? Can the authors provide specific quantitative evidence to support this choice? For sea ice mass balance, the absolute magnitude of the budget terms is physically important. By applying a quantile transformer, the original distributions are forced into normal distributions. This may improve the statistical separation of clusters, but it may also distort the physical distances between data points. In other words, weak but spatially variable processes may become overemphasized, while physically dominant processes may become less important in the clustering. The authors state that scaling is only used during the clustering step and that the results are later interpreted in physical units. However, this does not fully solve the problem, because the regime boundaries are already determined in the transformed feature space. Therefore, I think the authors need to provide more evidence for this methodological choice. For example, can they show the clustering results using unscaled variables, standard scaling, robust scaling, and quantile scaling? How different are the regime maps under these different choices? Does the "Coast" regime still appear in the same regions, especially those offshore regions that are not along the Antarctic coast? Is the decreasing trend of the "Coast" regime still significant under other scaling methods?

Response: Thank you for raising this concern. We have removed the age-income analogy which may add confusion and revised the manuscript to explain the purpose and limitations of the quantile transformation more clearly.

We repeated the analysis using unscaled variables, standard scaling, and robust scaling, in addition to the quantile transformation used in the manuscript. The same UMAP and DBSCAN parameters are used in all experiments, so the comparison isolates the effect of preprocessing. The resulting latent spaces and clustering maps are shown in Figure R2.

The unscaled, standard-scaled, and robust-scaled data produce continuous latent space structures, with no clear low-density separation between groups. In all three cases, DBSCAN assigned approximately 99% of the classified samples to a single cluster. With quantile scaling, the largest cluster contains 53% of the samples, and the remaining samples also form several distinct density structures, corresponding to the six regimes. Since the alternative scaling methods cannot produce identifiable counterparts of the six regimes, a direct comparison of the Coast distribution and area trend is therefore not meaningful under those classifications.

In addition, the dynamic and thermodynamic terms have overall similar magnitudes but different distribution shapes. We therefore use the quantile transformation to reduce the influence of the distributional difference and to give the three terms more comparable weight in the clustering. While scaling removes the absolute magnitude and physical units of the budget terms, this step is essential in unsupervised machine learning methods, where the goal is to identify recurring combinations based on the relative contributions of the underlying processes. Without scaling, the distance metric may be dominated by the largest-magnitude terms. This could lead to several regimes being identified in coastal regions that are characterized by essentially the same process combination but differ mainly in intensity, while the contributions of smaller-magnitude terms are obscured. Our aim is not to distinguish regimes according to differences in their absolute magnitudes, but to identify distinct combinations and balances of dynamic and thermodynamic processes. The quantile transformation reduces the dominance of individual variables and allows processes with smaller absolute values to contribute to the classification.

Therefore, we retained quantile scaling because it is the only tested approach that produces a clearly separated, stable, and physically interpretable six-regime classification. However, we acknowledge that the transformation changes the distances and relative spacing of samples in feature space. We have clarified in the revised manuscript that the regime boundaries are defined within this preprocessing framework.

"In our case, we compared unscaled data, standard scaling, robust scaling, and quantile transformer using the same UMAP and DBSCAN parameters. The unscaled, standard-scaled, and robust-scaled data produced continuous latent space structures, with approximately 99% of the samples assigned to a single cluster. In contrast, quantile transformer produced several distinct and stable density structures. We therefore chose quantile transformer because it provided the clearest and most stable clustering structure for the objectives of this study. We acknowledge that this transformation changes the distances and relative spacing of samples in feature space. The resulting regimes should therefore be interpreted within this preprocessing framework, which aims to identify recurring combinations based on the relative contributions of the underlying processes, rather than distinguish regimes according to differences in their original absolute magnitudes."

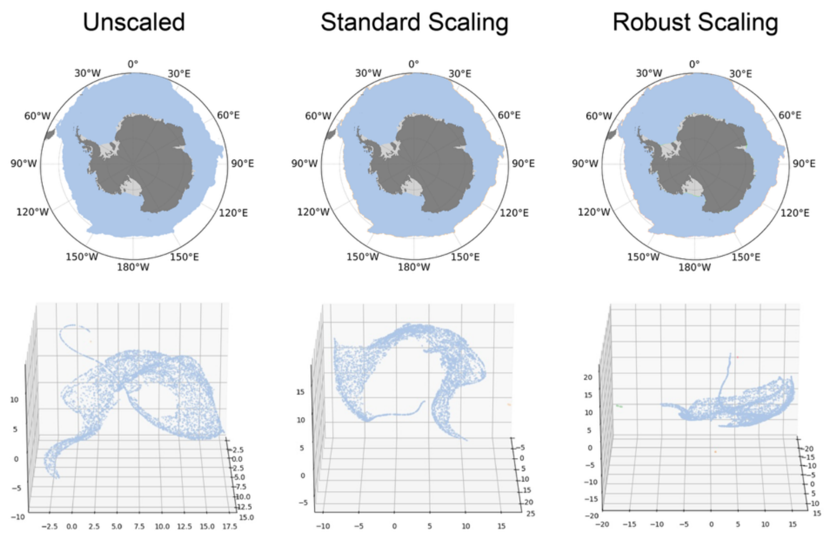

Figure R2: Clustering maps and corresponding latent space representations using unscaled variables, standard scaling, and robust scaling.

3. The "Coast" regime is one of the most important results of the paper. The authors state that the regime is mainly found along the coast, but also in offshore regions in the Indian Ocean, Ross Sea, Bellingshausen and Amundsen Seas (maybe too much). They further acknowledge that it does not necessarily represent coastal processes such as landfast ice formation or coastal leads, but rather divergence-dominated dynamics that

may occur beyond the coastal zone. This raises a major interpretive problem. If the regime is not necessarily coastal, and if it can occur far from the Antarctic coast, then why it is called "Coast"? This issue becomes more serious in the Discussion, where the authors suggest that shrinking of the "Coast" regime may indicate weakening of new ice formation in coastal polynyas and may have implications for brine rejection and Antarctic Bottom Water formation. This conclusion is not sufficiently supported if the regime includes large offshore divergence regions and is not specifically tied to coastal polynya processes. And one thing I noticed, abstract states that the coastal regime has experienced significant area loss, "potentially reflecting weakened new ice formation." The Discussion makes a similar link to coastal polynyas and Antarctic Bottom Water formation. This is too speculative.

Response: We apologize for the ambiguity in our original interpretation of the "Coast" regime.

We agree that the original name "Coast" placed too much emphasis on geographical location and could lead to a misinterpretation of the regime. We have therefore renamed this regime "Divergence" throughout the manuscript. The new name reflects its defining mass-budget characteristics, i.e., positive thermodynamic contributions combined with negative dynamic contributions during sea ice growth season. Although the regime occurs predominantly around the Antarctic coastal margin, similar conditions are also found in several offshore regions, and the regime remains consistently identified across sensitivity tests using different parameter settings.

We have also revised all relevant statements concerning the interpretation and broader implications of the "Divergence" regime. First, we removed the phrase "potentially reflecting weakened new ice formation" from the Abstract because this interpretation is not directly supported by the present analysis. The Abstract now reports only the significant decline in the area of the "Divergence" regime.

Second, we revised the relevant sentences in the Discussion to avoid inconsistency. The Divergence regime is not treated as a direct proxy for coastal polynyas, landfast ice, or coastal leads. Instead, we note that its characteristic combination of divergent ice transport and thermodynamic growth is commonly found in coastal polynyas but is not restricted to them. The revised text reads as follows:

"For instance, the 'Divergence' regime represents conditions in which divergent ice transport is accompanied by thermodynamic ice growth, a mechanism commonly found in coastal polynyas but not restricted to them (Holland and Kimura, 2016). In some coastal polynya regions, such conditions may contribute to large sea ice production and brine rejection, leading to the formation of dense shelf water and Antarctic Bottom Water (Golledge et al., 2025). The declining area of this regime indicates that these divergence–growth conditions have become less widespread within the model domain, though the implications of this change for Antarctic Bottom Water formation require further investigation."

4. I think the authors need to explain more clearly how the UMAP and DBSCAN parameters were selected. For example, what physical criteria were used to tune these parameters? Were the parameters chosen mainly to maximize cluster separation, to maximize stability, or to improve physical interpretability? These are not necessarily the same thing. More specifically, I would like to see sensitivity tests for the main

classification parameters. How do the results change if the UMAP neighbourhood size or minimum distance is varied? How do the results change if the DBSCAN epsilon or minimum-sample parameters are changed? Are the six regimes robust over a broad range of reasonable parameter choices, or do they only appear for a narrow set of parameter values? In particular, does the "Coast" regime still appear in similar locations, including the offshore regions? Does its area trend remain significant? Do the physical budget characteristics of each regime remain similar when these parameters are changed?

Response: Following your helpful suggestions, we have performed additional sensitivity analyses for the UMAP and DBSCAN parameters.

For UMAP, we tested "n_neighbors" values of 5, 25, 50, 100, and 200, and "min_dist" of 0.0, 0.3, and 0.5 (Figure R3). When "min_dist" = 0.0, "n_neighbors" values from 25 to 200 produce similar cluster structures, showing the six regimes identified with the parameters used in the main analysis. With "min_dist" = 0.5, the clusters are generally less well separated. Results obtained with "min_dist" = 0.3 fall between these two cases. Some "n_neighbors" values produce reasonably separated clusters, while others do not reproduce the six-regime structure as clearly. Parameter combinations yielding a single cluster indicate that these settings do not adequately resolve the density structure of the dataset, rather than that the six-regime result depends on one unique parameter combination.

For DBSCAN, we tested "eps" values of 0.5, 1.0, and 1.5, and "min_samples" of 3, 5, and 10 (Figure R4). The major regime structure is relatively insensitive to these parameters, with similar clusters obtained across the tested combinations.

For the parameter combinations that reproduce the six regimes, their spatial distributions are also broadly consistent. In particular, the Coast regime continues to exist both along the coast and in offshore regions, and the characteristic sea ice mass-balance signatures of the regimes were similar. The main results are therefore robust across a range of parameter values and do not depend on a single narrowly defined parameter combination. We keep the original parameters ("n_neighbors" = 100, "min_dist" = 0.0, "eps" = 1.0, and "min_samples" = 3) because this combination gives well-separated and stable clusters with spatial distributions consistent with our physical understanding of Antarctic sea ice processes.

We revised the relevant sentences to clarify the parameter-selection criteria and describe the sensitivity tests as follows:

"The UMAP parameters are selected based on cluster separation and classification stability. Sensitivity tests confirm that the six-regime structure is robust across a range of parameter values that resolve distinct cluster structures (Figure S8)."

"Sensitivity tests across a range of "eps" and "min_samples" values show that the main cluster structure is relatively insensitive to the choice of these parameters (Figure S9)."

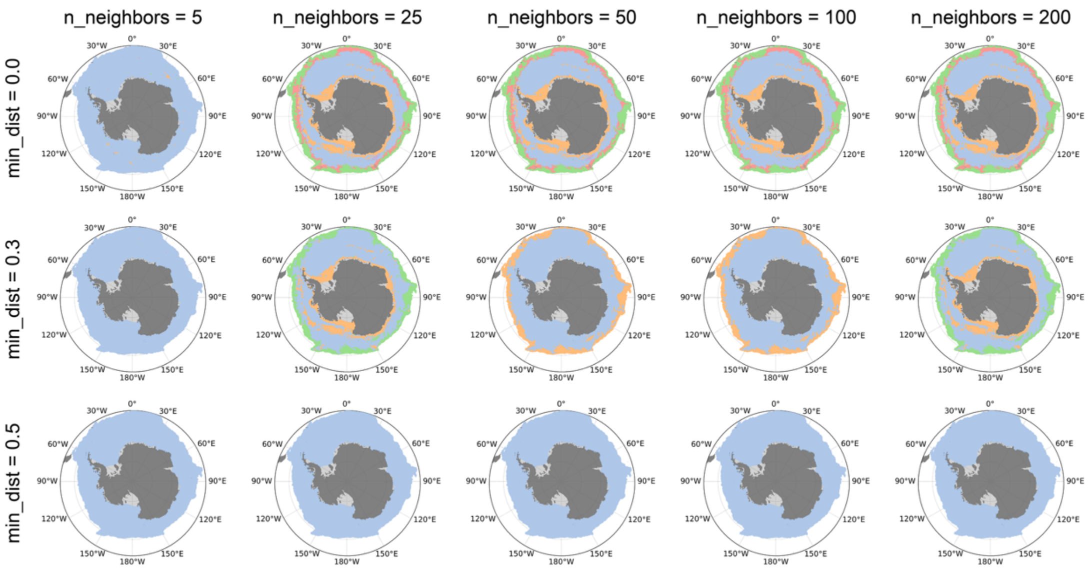

Figure R3: Clustering maps from the sensitivity analysis of the UMAP parameters. The number of nearest neighbors (n_neighbors) was varied among 5, 25, 50, 100, and 200, while the minimum distance (min_dist) was set to 0.0, 0.3, or 0.5. Some parameter combinations produce a continuous latent space structure and thus only one cluster, indicating that these settings do not resolve meaningful density structures in the present dataset. Among the parameter combinations that produce separable clustering results, the main spatial patterns and physical characteristics of the regimes remain broadly consistent.

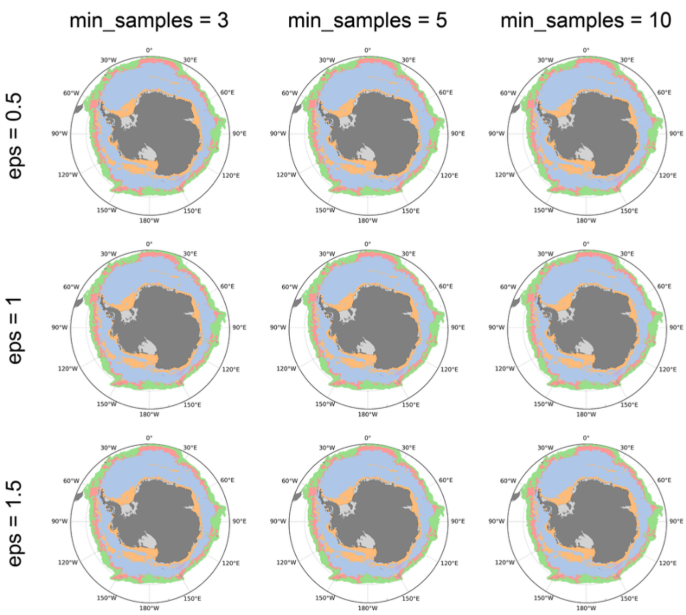

Figure R4: Clustering maps from the sensitivity analysis of the DBSCAN parameters. The minimum number of samples (min_samples) was varied among 3, 5, and 10, while the neighborhood radius (eps) was set to 0.5, 1.0, or 1.5.

Specific comments

Line 30: make sure you have the sea ice or sea-ice in consistency for the whole manuscripts.

Response: Thank you for pointing this out. We have now standardized the terminology to “sea ice” for consistency.

Line 33 "we conduct a process-based analysis of sea ice variability..": This is a good motivation. However, the current analysis uses model-derived budget components whose realism is not sufficiently validated.
Response: Thank you for raising this point. We agree that the identified regimes should be interpreted as process-based regimes within the NEMO-SI^3 modeling framework, rather than observationally validated classifications of physical processes. We have revised the sentence to clarify the interpretation: *"To address this knowledge gap, we conduct a process-based analysis of sea ice variability across different spatial and temporal scales* ***using model-derived mass balance components from NEMO-SI^3****, and develop a regime-identification framework to diagnose the physical drivers."*

Line 82-87: The uncertainty discussed here appears to be mostly clustering uncertainty, not uncertainty in model physics, input variables, or preprocessing. Please clarify what uncertainties are actually quantified.
Response: Thank you for pointing this out. We have revised the wording to clarify the uncertainty discussed here: *"In addition, the NEMI workflow evaluates cluster-assignment stability across repeated clustering iterations, providing a measure of clustering uncertainty for the identified regimes."*

Line 109: How much is underestimated in SIT compared with GIOMAS?
Response: We have added a quantitative comparison with GIOMAS: *"As for SIT, the model generally underestimates the GIOMAS values by 0.14 m."*

Lines 146-148: It is quite strange to see the age-income analogy in a geophysical budget analysis. In sea ice mass balance, absolute magnitudes are physically meaningful.
Response: Thank you for this comment. We have removed the age-income analogy from the revised manuscript and replaced it with a direct comparison of different preprocessing methods. We have also revised the text to clarify the role of the quantile transformation, as well as its strengths and limitations in the clustering analysis.

Lines 153: The statement that the quantile transformer "outperformed" other methods requires quantitative evidence. What metrics were used? Cluster separability? Stability?
Response: We agree that the term "outperformed" was too general and required clearer supporting evidence. We have therefore removed this wording and replaced it with a direct comparison of the clustering results obtained using different preprocessing methods (Figure R2). Cluster separability was assessed from the structure of the UMAP embedding and quantified using the fraction of samples assigned to the largest DBSCAN cluster. For the unscaled, standard-scaled, and robust-scaled data, approximately 99% of the classified samples are assigned to a single cluster, consistent with the largely continuous structures observed in the corresponding UMAP embeddings. With quantile transformation, the largest cluster contained 53% of the samples.

We have revised the manuscript to report these criteria explicitly: *"In our case, we compared unscaled data, standard scaling, robust scaling, and quantile transformer using the same UMAP and DBSCAN parameters. The unscaled, standard-scaled, and robust-scaled data produced continuous latent space structures, with approximately 99% of the samples assigned to a single cluster. In contrast, quantile transformer produced several distinct and stable density structures. We therefore chose quantile transformer* *because it provided the clearest and most stable clustering structure for the objectives of this study. We acknowledge that this transformation changes the distances and relative spacing of samples in feature space. The resulting regimes should therefore be interpreted within this preprocessing framework, which aims to identify recurring combinations based on the relative contributions of the underlying processes, rather than distinguish regimes according to differences in their original absolute magnitudes."*

Lines 158-162 (line 169 as well): Why transfer the data into normal distributions given the above skewed dynamic and thermodynamic distribution. This is risky. The transformation can modify distances, correlations, and relative importance of variables
Response: We acknowledge that the quantile transformation changes the distances and relative spacing of samples in feature space. The purpose of the transformation is not to imply that the sea ice budget terms are physically normally distributed, but to reduce the influence of their different skewness on the neighborhood structure used by UMAP.

We have also tested unscaled data, standard scaling, and robust scaling using the same UMAP and DBSCAN parameters. These alternatives produced continuous latent-space structures, while quantile transformation produced several distinct density structures. We therefore retained this transformation while explicitly acknowledging that the regime boundaries are defined within the selected preprocessing framework.

Lines 196-199: The statement that the model's climatological sea ice mass balance aligns well with established understanding is too broad. Please provide more direct evidence.
Response: Thank you for this comment. We agree that the original statement was too broad. However, a direct observational validation of all individual sea ice mass-balance components is not currently possible because reliable, large-scale observations of these terms are unavailable. We have added an indirect process-level evaluation comparing an observation-derived SIC budget with the corresponding SIC budget diagnosed from NEMO-SI[3] (see Figure R1 and our responses above). Both budgets show similar physical processes during the growth season. We have therefore revised the text to describe the specific model features that are qualitatively consistent with previous studies, rather than making a general statement about model performance.

The revised text reads as follows:
"During the growth season, the model simulates widespread thermodynamic ice growth, negative dynamic contributions near the Antarctic coast, and positive dynamic contributions farther offshore. These spatial patterns are qualitatively consistent with previous studies showing that coastal ice divergence and mechanical redistribution reduce local sea ice concentration, while freezing within divergent regions and equatorward ice transport contribute to sea ice growth across the ice pack (Holland and Kwok, 2012; Haumann et al., 2016; Holland and Kimura, 2016; Himmich et al., 2023)."

Lines 216: "UMAP parameters were chosen based on physical intuition" is too vague. What physical intuition? Which parameters? What ranges were tested? What criteria determined the final choice?
Response: Thank you for pointing this out. We have replaced "physical intuition" with explicit selection criteria and added the sensitivity tests (see Figure R3 and our response above). Specifically, we varied the UMAP "n_neighbors" parameter among 5, 25, 50,

100, and 200 and "min_dist" among 0.0, 0.3, and 0.5. We retained "n_neighbors" = 100 and "min_dist" = 0.0, which produced well-separated and stable clusters with spatial distributions consistent with our physical understanding of Antarctic sea ice processes. We have revised the relevant text as follows:

"The UMAP parameters are selected based on cluster separation and classification stability. Sensitivity tests confirm that the six-regime structure is robust across a range of parameter values that resolve distinct cluster structures (Figure S8)."

Lines 221-230: The comparison with KMeans and Agglomerative clustering is useful but insufficient. DBSCAN may capture visually appealing structures in UMAP space, but that does not prove physical correctness. Please include sensitivity to DBSCAN parameters and preprocessing choices.

Response: Thank you for this comment. We performed sensitivity tests for the DBSCAN parameters "eps" and "min_samples" (see Figure R4 and our response above). Specifically, we tested "eps" values of 0.5, 1.0, and 1.5 and "min_samples" values of 3, 5, and 10. The main cluster structure was relatively insensitive to these parameters, with similar clusters recovered across the tested combinations. We added the following sentence to the revised manuscript:

"Sensitivity tests across a range of "eps" and "min_samples" values show that the main cluster structure is relatively insensitive to the choice of these parameters (Figure S9)."

Lines 245-246: The sentence stating that the regime does not necessarily reflect landfast ice or coastal leads undermines later discussion linking this regime to coastal polynyas and new ice formation?

Response: We have retained this sentence because it clarifies the physical meaning of the newly named "Divergence" regime. The regime is defined by its mass-budget signature, which represents positive thermodynamic contributions combined with negative dynamic contributions, and is not intended to serve as a direct proxy for landfast ice, coastal leads, or coastal polynyas.

To avoid inconsistency, we have revised the corresponding text in the Discussion. We no longer interpret the decline of this regime as direct evidence of reduced coastal polynya activity or weakened new ice formation.

"For instance, the 'Divergence' regime represents conditions in which divergent ice transport is accompanied by thermodynamic ice growth, a mechanism commonly found in coastal polynyas but not restricted to them (Holland and Kimura, 2016). In some coastal polynya regions, such conditions may contribute to large sea ice production and brine rejection, leading to the formation of dense shelf water and Antarctic Bottom Water (Golledge et al., 2025). The declining area of this regime indicates that these divergence–growth conditions have become less widespread within the model domain, though the implications of this change for Antarctic Bottom Water formation require further investigation."

Lines 299-305: The statement that increased divergence is no longer compensated by thermodynamic gain is interesting, but it needs more direct support. Are these trends computed within a moving regime mask? If so, the area and intensity effects may be

mixed.
Response: Thank you for pointing this out. The trends were calculated over the grid cells assigned to the "Divergence" regime in each year, and the regime mask therefore varies with time. We acknowledge that these trends reflect a combination of changes in intensity and changes in location. They cannot sufficiently support that the weakening thermodynamic contribution causes the decline in regime area. We have clarified how the trends were calculated and revised the interpretation to avoid implying a direct causal relationship.

"Since these trends are calculated over the time-varying regime mask, they reflect both changes in process intensity and location. However, the decline in the area of the 'Divergence' regime coincides with stronger divergent transport and weaker thermodynamic compensation among the grid cells classified within this regime."

Lines 329-341: I am not sure how much I can speculate from post-2016 change is primarily related to significant area loss in the coastal and pack regimes, given that some concerns above.
Response: We have revised the original interpretation to avoid implying a direct causal relationship: *"We focused particularly on shifts between the pre- and post-2016 periods, when Antarctic sea ice shifted from net-growth to net-loss. This transition was characterized mainly by significant area losses in the 'Divergence' and 'Pack' regimes, reflecting concurrent changes in the sea ice dynamic and thermodynamic processes."*

Lines 355-364: The claimed advantage over SIC or bathymetry-based classifications needs stronger demonstration in the main text. If the difference is mainly shown in supplementary figures, don't you put that in the main manuscript?
Response: Thank you for this comment. We agree that the advantage of the NEMI framework should be supported more directly in the main text. Rather than adding an additional figure near the end of the Discussion, we have strengthened the comparison by including a quantitative measure of grid-cell assignment disagreement in the main text, while retaining the detailed spatial comparison in Fig. S11.

The following sentence has been added: *"The grid-cell assignment disagreement between NEMI and the SIC/bathymetry-based classification, defined as the proportion of grid cells assigned to different regimes by the two methods, ranges from 40.6% to 68.1% during the sea ice growth season and from 50.0% to 97.7% during the melt season."*