Co-review of:

“Effects of snow redistribution parameterization on simulated snow thickness validated by MOSAiC observations”

Submitted to *The Cryosphere* by Gu et al.

The manuscript presents a model sensitivity analysis of the treatment of snow redistribution on sea ice and its effects on snow and sea ice thicknesses. The single-column sea ice model (Icepack) is forced and the simulations are assessed using time-series observations from the Multidisciplinary Drifting Observatory for the Study of Arctic Climate (MOSAiC) field campaign. The analysis provides new insights on the modeling of snow redistribution on sea ice within a commonly used modeling framework, as Icepack provides the column physics for the CICE sea ice model and several others.

The scope of the work, albeit simple, is a good fit for *The Cryosphere*. However, there are several parts of the manuscript and analysis that require major revision before the manuscript would be suitable for publication, in addition to numerous smaller changes needed for clarity and readability (see “Minor/typographical comments”). We hope the authors will find our comments below constructive.

Major requested changes include:

1. Correcting and clarifying important details in the description of the observations and methods, and potentially making corresponding revisions to aspects of the methodology;
2. Separating the Discussion and Conclusions into distinct sections;
3. In the Discussion section, placing the study’s results into context of previous relevant findings and fully characterizing the study’s limitations;
4. Adding recommendations for future research in the Conclusions, based on the new findings.

Specific suggestions for item #1 are detailed within “Major comments” below. The other three changes (#2, #3, and #4) are essential because the combined “Conclusions and Discussions” section is currently composed of just three paragraphs, with only superficial contextualization of the study’s results. The existing text provides little insight into how the study’s results have addressed key knowledge gaps or shed new light on the physical processes that influence the snow mass budget on sea ice. Consequently, the study feels incomplete.

Response:

We sincerely thank the reviewer for the careful and constructive assessment of our manuscript. We appreciate the recognition that the topic fits the scope of The Cryosphere and that the study can provide useful insights into snow redistribution on sea ice within the Icepack/CICE modeling framework. In response to the reviewer’s major comments, we have substantially revised the manuscript to clarify the observations, model configuration, forcing data, and snow redistribution parameterizations; expanded and reorganized the Discussion and Conclusions; strengthened the comparison with previous relevant studies; and added a clearer discussion of limitations and future research needs. We have also addressed the detailed comments point by point, with revised manuscript text provided where appropriate.

We would recommend the authors invest significant effort into filling in gaps in their appraisal of existing literature, starting with relevant previous work by Déry & Tremblay (2004), Chung et al. (2011), Lecomte et al. (2015), Merkouriadi et al. (2025), Zhao et al. (2025), Robinson et al. (2025), and Robinson et al. (2026). Full references are provided at the end of this report.

Response:

Thank you for this important suggestion. We agree that the original manuscript did not sufficiently discuss previous work on wind-driven snow redistribution, blowing-snow sublimation, lead-related snow loss, and snow evolution on sea ice. We have expanded the Introduction to better place our study in the context of these earlier studies.

“Blowing snow can also facilitate the sublimation of suspended snow particles (Déry & Yau, 2002; Liston et al., 2020), and studies have shown that under the same atmospheric conditions, the sublimation rate of blowing snow is two orders of magnitude higher than that of a stationary snow surface (Schmidt, 1982). **Previous modeling studies have shown that wind-driven redistribution and blowing-snow processes can substantially modify the snow mass budget on polar sea ice through snow transport, sublimation, and redistribution among different ice surface types (Déry and Tremblay, 2004; Chung et al., 2011; Lecomte et al., 2015). Recent satellite-based studies during MOSAiC further indicate that blowing snow was active over Arctic sea ice and may represent an important sink in the snow-on-sea-ice budget (Robinson et al., 2025, 2026)”**

“Recent work has also emphasized that snow sinks on level sea ice can vary strongly with atmospheric forcing, surface state, and ice conditions (Merkouriadi et al., 2025)”

In particular, it is necessary for the authors to differentiate their study from the recent work of Zhao et al. (2025), which was published at the time of their submission and similarly evaluates snow evolution in Icepack simulations along the MOSAiC drift trajectory using the same three configurations: no redistribution, bulk redistribution, and blowing snow (snwITDrdg) redistribution. How do the authors’ results compare to those from Zhao et al., and how can differences in the results be traced back to the distinct methodologies and analysis frameworks? The existing reference to Zhao et al. in Lines 78-82 is nonspecific; these details should be explored in the Discussion section.

Response:

Thank you for highlighting this point. We agree that the previous reference to Zhao et al. (2025) was too general. We have added a more specific comparison in the Discussion. Zhao et al. (2025) also used Icepack along the MOSAiC drift trajectory and compared no-redistribution, bulk redistribution, and snwITDrdg configurations. However, their study focused primarily on sea ice thickness and the combined effects of snow redistribution and melt-pond schemes, whereas our study focuses on simulated snow thickness/SWE, snow mass loss into leads, and the optimization of snow redistribution parameters using MOSAiC snow observations and lead-loss constraints. Differences between the two studies can therefore be traced to differences in target variables, forcing choices, observational constraints, model version/configuration, treatment of sea ice concentration, and the parameter-sensitivity framework.

"Our study is complementary to Zhao et al. (2025), who evaluated Icepack simulations along the MOSAiC drift trajectory using no-redistribution, bulk redistribution, and snwITDrdg configurations. Zhao et al. (2025) focused mainly on sea ice thickness and the combined influence of snow redistribution and melt-pond schemes. In contrast, the present study focuses on snow thickness/SWE and the snow mass budget, especially the sensitivity of simulated snow loss to leads to the bulk redistribution parameter p, the blowing-snow erosion coefficient γ, and prescribed AMSR2 sea ice concentration. Therefore, while both studies show that snow redistribution strongly affects Icepack simulations during MOSAiC, our analysis provides a more direct evaluation of snow redistribution parameterizations against MOSAiC snow observations and observed lead-related snow loss. Differences between the results of the two studies likely arise from differences in forcing data, model version and configuration, sea ice concentration treatment, observational constraints, and the specific target variable used for evaluation."

<u>Major comments</u>:

Introduction, Lines 46-75: Separate details on key physical processes from Icepack-specific parameterizations

This section is confusing because it mixes discussion of the Icepack-specific parameterizations with descriptions that introduce the key physical processes affecting the snow mass budget. For example, it is unclear whether Lines 53-56 refer to the processes or their model representation ("Beyond bulk redistribution, snwITDrdg redistribution is a crucial physical process that determines the spatial distribution of snow thickness…"), especially given that two of the references cited afterwards are studies unrelated to Icepack.

It would make more sense to reorganize this section to introduce the key physical processes first, including relevant observational evidence, before even mentioning Icepack or its parameterizations. The details about Icepack parameterizations could be moved to Section 2.1 in Data and Methods.

Response:

We thank the reviewer for this constructive suggestion. We agree that the original paragraph mixed the general description of the key physical processes controlling the snow mass budget with Icepack-specific parameterization details.

We have reorganized this part of the Introduction. The revised text now first introduces the key physical processes that govern the snow mass budget, wind-driven snow redistribution, loss of blowing snow into open leads, and blowing-snow sublimation, together with the relevant observational evidence (e.g., Leonard & Maksym, 2011; Clemens-Sewall et al., 2023). All model-specific details, including the bulk snow redistribution parameterization (the assumed ~30% effective snowfall loss) and the snwITDrdg redistribution scheme in Icepack 1.3.2, have been moved to Section 2.3 (Data and Methods). The revised Introduction text is given below:

"**The spatial distribution of snow thickness on sea ice is primarily controlled by snowfall (Wagner et al., 2022), but it is also strongly shaped by wind-driven snow redistribution.** Strong winds can lift snow particles from the surface, and a portion of these suspended particles fall into open leads (Olivier Lecomte et al., 2015; Leonard & Maksym, 2011; Petty et al., 2018; Van den Broeke et al., 2004), while the remaining particles are redistributed on the snow surface, thereby altering the spatial distribution of snow thickness and affecting the mass and energy balance of the snow (Luo & Zhang, 2022). Blowing snow can also facilitate the sublimation of suspended snow particles (Déry & Yau, 2002; Liston et al., 2020), and studies have shown that under the same atmospheric conditions, the sublimation rate of blowing snow is two orders of magnitude higher than that of a stationary snow surface (Schmidt, 1982). **Observational studies highlight the importance of these processes for the snow mass budget.** In the Southern Ocean, about half of the snowfall eventually falls into open leads (Leonard & Maksym, 2011). During the MOSAiC expedition, observations indicate that under typical winter conditions, leads have a minimal impact on the snow mass balance, but when snowfall occurs alongside strong winds and high temperatures, 65%-100% of the recent snowfall can fall into open leads (Clemens Sewall et al., 2023), as strong winds promote snow redistribution and higher temperatures prevent rapid lead refreezing."

Data and Methods, Lines 105-137 (Section 2.2): Expanding and reorganizing descriptions of observations

- Were these different data sets compiled by the authors or was the merged MOSAiC forcing dataset (https://arcticdata.io/catalog/view/urn%3Auuid%3A305b3360-6eea-4b31-9d6e-defe554b81db) used? If the data sets were compiled by the authors into a unified format, will this merged data set be made available, per the Data Policy (see below) of the journal?

Response:

We thank the reviewer for this question. The forcing datasets used in this study were compiled by the authors from the individual MOSAiC data sources rather than taken from the merged MOSAiC forcing dataset at the Arctic Data Center. Specifically, the atmospheric forcing was compiled from the Met City and Atmospheric Surface Flux Station measurements (Cox et al., 2021a, 2021b, 2021c, 2023), the ocean forcing from the CTD buoys and ocean flux buoys (Grosfeld et al., 2016), and the precipitation forcing from the PWD and ERA5. All of these source datasets are publicly available with DOIs, as cited in the Data Availability section. We note that this work began in 2023, prior to the release of the merged MOSAiC forcing dataset in 2024, so that merged product was not yet available when our forcing was constructed. The compilation procedures are now described in detail in Section 2.2, including gap filling, the extrapolation of the ASFS 2 m wind to 10 m, the vertical and cross-buoy averaging of the ocean observations, and the combination of the PWD and ERA5 precipitation, such that the forcing can be fully reproduced from the cited public datasets. The same compiled forcing was used in our previous Icepack study (Gu et al., 2024), where it was shown to reproduce the observed snow and ice evolution well.

- Some of the forcing data and initial conditions need more discussion. Precipitation forcing is one of the biggest sources of potential bias in snow thickness simulations, so it would be helpful to include descriptions of its uncertainties. Additionally, the details about averaging the PWD and ERA5 forcing in Section 3.1, Lines 190-195, should be moved into this section, as that is part of the Methods. The details of the snow and ice thickness data would also be helpful to expand on: what kind of sea ice were the buoys deployed on, what kind of spread occurred in the observed snow thickness and ice thickness, were the buoys consistently operating for the full period of the simulations, etc.? If the buoys were generally deployed on level ice, could this result in a low bias in the authors' observational baseline for snow depth, due to the neglect of thicker snow on and around ridges?

Response:

We thank the reviewer for these suggestions. We have expanded Section 2.2 to address all three points, as detailed below.

1. Thank you for this helpful suggestion. We agree that the forcing data and initial conditions require more detailed description, especially because precipitation forcing is a major source of uncertainty in snow-thickness simulations. We have moved the description of the PWD–ERA5 precipitation forcing from the Results section to the Methods section and clarified that we used the average of PWD and ERA5 precipitation following Wagner et al. (2022), because the actual MOSAiC precipitation likely falls between these two estimates. We also added text discussing the uncertainty associated with precipitation timing, frequency, and magnitude.

"In addition, precipitation data from the present weather detector (PWD) on the deck of the Polarstern and from the ERA5 reanalysis (Wagner et al., 2022) were used. **Precipitation forcing is an important source of uncertainty in the snow-thickness simulations. Following Wagner et al. (2022), we used the average of precipitation from the PWD and ERA5 reanalysis as the model precipitation forcing, because MOSAiC precipitation estimates differ substantially among products and the actual precipitation likely lies between the PWD and ERA5 estimates. The resulting cumulative precipitation from 31 October 2019 to 26 April 2020 is 91 mm, within the observationally constrained range reported by Wagner et al. (2022). Nevertheless, uncertainties in precipitation timing, frequency, and magnitude may contribute to biases in simulated snow accumulation, especially during individual snowfall events.**"

2. Snow and ice thickness observations. We have added a new table (Table 1) summarizing the characteristics of the seven SIMBA buoys used in this study, including the deployment site, sea-ice type, observation period, and initial snow and ice conditions. The buoys were deployed on both first-year ice and second-year ice during the MOSAiC expedition (Perovich et al., 2023; Salganik et al., 2023), at the Central Observatory, at the L1–L3 sites of the Local Scale Distributed Network within a few kilometers of the Central Observatory, and at the M6 site of the Mesoscale Distributed Network tens of kilometers away. The observation periods differ among buoys (Table 1), and the buoy means used for model evaluation were computed only over the periods when each buoy was active.

"Table 1. Characteristics of the seven MOSAiC Snow and Ice Mass Balance Array (SIMBA) buoys used in this study, including deployment site, sea-ice type, observation period, and initial snow and ice conditions. The selected buoys were deployed over both first-year ice (FYI) and second-year ice (SYI) during the MOSAiC expedition (Perovich et al., 2023; Salganik et al., 2023). CO refers to the Central Observatory, L1-L3 are Local Scale Distributed Network sites located within 15 kilometers of the Central Observatory, and M6 is a Mesoscale Distributed Network site located 10 kilometers from the Central Observatory."

Buoy ID	**Site**	**Ice Type**	**Start Date**	**End Date**	**Initial Ice Thickness (m)**	**Initial snow Thickness (m)**
2019T56	CO	SYI	2019-11-02	2020-07-03	0.42	0.18
2019T62	CO	SYI	2019-10-30	2020-07-05	0.80	0.20
2019T63	L2	SYI	2019-11-25	2020-07-27	1.08	0.12
2019T64	M6	SYI	2019-11-27	2020-08-01	1.70	0.16
2019T66	CO	FYI	2019-10-30	2020-08-02	0.42	0.10
2019T67	L1	SYI	2019-11-09	2020-07-07	1.36	0.22
2019T70	L3	FYI	2019-10-10	2020-07-11	0.48	0.06

3. We agree with the reviewer that because the SIMBA buoys were deployed on level ice, thicker snow on and around ridges is not sampled, and the observational mean may therefore underestimate the true area-mean snow depth. We have expanded the Discussion (Section 4) to address the broader implications of subgrid-scale snow heterogeneity. The new discussion notes that snow accumulation and redistribution vary among sea-ice regimes with ice age, ice type, deformation state, and surface roughness (Clemens-Sewall et al., 2022), and that pronounced subgrid-scale spatial variability in snow thickness has been observed in the field (Huang et al., 2026; Mallett et al., 2022). Since Icepack assumes a spatially uniform snow thickness within each ice-thickness category, this heterogeneity is not explicitly resolved. We therefore clarify that our evaluation is based on the mean snow thickness over the observational region, and that the optimized parameter values and conclusions represent the average snow redistribution behaviour across the range of sea-ice conditions sampled during MOSAiC. Because the same model assumption is applied consistently in all simulations, it is unlikely to affect the relative comparison among the snow redistribution parameterizations examined in this study.

- Lines 120-121, 124: We assume that the 10-m and 3-m ocean time series were averaged vertically to be used as ocean forcing, as in the lead author's previous study using Icepack. "Spatially averaged" implies only horizontal averaging. Please clarify the procedure used.

Response:

Thank you for this helpful comment. We agree that the original wording "spatially averaged" was not sufficiently clear. In our procedure, all available ocean temperature and salinity observations, including the 10-m measurements from the eight CTD buoys and the 3-m measurements from the four autonomous ocean flux buoys, were considered representative of the mixed layer, because these observations were confined within the mixed layer depth. Therefore, they were combined to obtain a mixed-layer representative time series and then averaged spatially across the buoys to provide the time-dependent ocean forcing for the Icepack simulations. The current velocity data from the four autonomous ocean flux buoys were used to calculate the friction velocity for estimating ocean–ice heat exchange. We have revised the text accordingly.

"Oceanographic data comprised 10-meter sea temperature and salinity measurements from eight Conductivity, Temperature, and Depth (CTD) buoys, as well as 3-meter sea temperature, salinity, and current velocity data from four autonomous ocean flux buoys. **Because all ocean temperature and salinity observations were located within the mixed layer depth (Rabe et al., 2022), the 10-m and 3-m measurements were combined to obtain mixed-layer representative time series and then spatially averaged across the buoys. These time-dependent temperature and salinity series were prescribed as external ocean forcing in the Icepack simulations. The observed current velocities from the four autonomous ocean flux buoys were used to calculate the friction velocity, which was used to estimate ocean–ice heat exchange.** "

- Lines 121-122: Friction velocity depends on shear stress. What ice–ocean drag coefficient and sea ice drift velocities are assumed/used to calculate the ice–ocean shear stress?

Response:

Thank you for this comment. We agree that the friction velocity should be linked to the ice–ocean shear stress rather than prescribed without clarification. In Icepack, the friction velocity is normally set as a fixed constant. In this study, however, we implemented a quadratic drag law following Jenkins et al. (2010) to relate the friction velocity to the relative free-stream velocity:

$$u_*{}^2 = c_w U^2$$

where c_w= 0.0055 is the dimensionless ice–ocean drag coefficient, and U is the ice velocity relative to the ocean surface current. The observed current velocities used in our calculation are therefore relative ice–ocean velocities. We have revised the manuscript to clarify the drag coefficient, the definition of U, and the calculation of friction velocity.

"The observed relative velocities between sea ice and the ocean surface current from the four autonomous ocean flux buoys were used to calculate the friction velocity, which was then used to estimate ocean–ice heat exchange. Although Icepack prescribes friction velocity as a fixed constant by default, we implemented a quadratic drag law following Jenkins et al. (2010), $u_*{}^2 = c_w U^2$, where c_w is the dimensionless ice–ocean drag coefficient and U is the ice velocity relative to the ocean surface current."

- There is a lot of information in a long paragraph form in this section. We wonder if it'dbe helpful to break up the section into smaller paragraphs and additionally include some of the data information in a table, along with the model settings (see comment below regarding Lines 100-101).

Response:

Thank you for this helpful suggestion. We have revised this section by breaking the long paragraph into several shorter paragraphs to improve readability. In addition, we have added a table1 to describe model setting and a table2 summarizing the buoy-related data information, including buoy type, observed variables, observation depth, and their use in the model forcing/settings.

Table 1. Detailed options of physical parameterizations and model settings for ICEPACK. SST signifies sea surface temperature.

ICEPACK	Value
Time step	3600 s
Number of layers in the ice	7
Number of layers in the snow	1
Ice thickness categories	15 (Clemens-Sewall et al., 2022)
Initial ice thickness	105 cm (mean observe)
Initial snow depth	17 cm (mean observe)
Ice thermodynamic	Mushy layer (Turner et al., 2013)
Shortwave radiation	Delta-Eddington (Briegleb and Light, 2007)
Melt ponds (superimposed ice)	Not used in this study
Ocean heat transfer coefficient	0.006 (Maykut and McPhee, 1995)
SST restoring timescale (days)	0 (use observed SST as oceanic forcing)
Ocean friction velocity minimum (m s-1)	0.0005 (Tsamados et al., 2013)

Table 2. Characteristics of the seven MOSAiC Snow and Ice Mass Balance Array (SIMBA) buoys used in this study, including deployment site, sea-ice type, observation period, and initial snow and ice conditions. The selected buoys were deployed over both first-year ice (FYI) and second-year ice (SYI) during the MOSAiC expedition (Perovich et al., 2023; Salganik et al., 2023). CO refers to the Central Observatory, L1-L3 are Local Scale Distributed Network sites located within a few kilometers of the Central Observatory, and M6 is a Mesoscale Distributed Network site located tens of kilometers from the Central Observatory.

Buoy ID	Site	Ice Type	Start Date	End Date	Initial Ice Thickness (m)	Initial snow Thickness (m)
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2019T66	CO	FYI	2019-10-30	2020-08-02	0.42	0.10
2019T67	L1	SYI	2019-11-09	2020-07-07	1.36	0.22
2019T70	L3	FYI	2019-10-10	2020-07-11	0.48	0.06

Data and Methods, Lines 133-134: Questionable use of density–SWE fitted relationship

We're confused by why this approach of using fitted SnowMicroPen density–SWE relationships was taken, and we're not convinced it's the best approach. Why derive a rough estimate of SWE when one can directly compare observed and modeled snow depths? The simulated density is a known quantity, making it possible to make an apples-to-apples comparison between the simulated and observed snow depths. The derived SWE from the snow depth observations would contribute more uncertainties and biases to the observations. Also, by taking the mean and standard deviation of the actual buoy snow depths, one can better evaluate the model results within the context of spatial heterogeneity in observations.

Response:

Thank you for this important comment. We agree that directly comparing observed and modeled snow depths can be useful for evaluating geometric snow thickness, and that converting snow depth to SWE using an empirical relationship may introduce additional uncertainty. However, in this study, we retained the SWE-based comparison because the Icepack simulations used a fixed snow density of 330 kg m^{-3}, whereas the observed snow density varied substantially, approximately from 200 to 400 kg m^{-3}. As a result, the same snow depth can correspond to very different snow mass. A comparison based only on snow depth would therefore not fully account for variations in snow density and snow mass.

The fitted SnowMicroPen density–SWE relationship from Wagner et al. (2022) was used to convert the buoy snow-depth observations to SWE, allowing the observations and simulations to be compared in terms of snow mass. We acknowledge that this empirical conversion may introduce uncertainty, and we have revised the manuscript to clarify both the rationale for using SWE and this limitation.

Regarding the reviewer's suggestion to use the mean and standard deviation of the actual buoy snow depths, we agree that these quantities reflect spatial heterogeneity in the observed snow cover. In our analysis, however, the SWE estimates were derived from the same buoy snow-depth observations using the fitted density–SWE relationship, so the mean state and spatial variability represented by the buoy snow depths are largely reflected in the SWE estimates. To avoid redundant information, we retained SWE as the primary evaluation metric.

"**Because the Icepack simulations used a fixed snow density of 330 kg m-3, whereas the observed snow density varied from approximately 200 to 400 kg m-3, a direct comparison based only on snow depth would not account for variations in snow mass. Therefore,** the observed snow thickness were converted to snow water equivalent (SWE) using fitted SnowMicroPen (SMP) density–SWE relationships (Wagner et al., 2022), **allowing the observations and simulations to be compared in terms of snow mass. Although this empirical conversion may introduce additional uncertainty, it provides a more physically consistent basis for evaluating the simulated snow mass.**"

Multiple locations: Incorrect characterizations of P in the bulk redistribution scheme

On Lines 49-53, the description of the bulk redistribution scheme isn't quite right (in particular: "30% of snowfall is effectively lost due to ice surface heterogeneity, such as leads and ridges"). It is correct that snow loss can occur from wind blowing snow into open leads as well as during ridging (some snow gets subducted with ice blocks during the ridging process). However, the 30% specifically comes from Sturm et al. (2002)'s observation that ridged ice had 30% more snow in its immediate surroundings than level sea ice, which is not clear in the authors' text. In reality, such variations likely occur due to a combination of wind-driven redistribution from level ice onto the ridged area, as well as wind-driven loss of snow on level ice into leads and to sublimation. However, the Icepack bulk redistribution scheme addresses these variations by assuming loss to leads is the *only* process at play, by instantaneously diverting a fraction of incoming snow into the ocean. Please clarify these critical distinctions in the text.

Additionally, we think it is incorrectly stated in the Abstract (Lines 17-18), the Data and Methods (Line 154), and Results (Line 239) that P represents the ratio of snow mass on ridges to that on level ice. As we have interpreted this Icepack parameterization, the quantity)1 + P (represents the ratio of snow mass on ridges to that on level ice, not d. We understood that that is why the authors have tested a range from P = 0.ε to P = 4 to correspond to the SHEBA observation of a ratio of 1.3 and the MOSAiC observation of a ratio of 5 (Lines 245-246). The characterization of P should be corrected throughout the paper.

As an illustrative example: assume the Icepack column is 25% ridged ice and 75% level ice, and the ridges have 5 m of snow cover and the level ice has 1 m of snow cover. The ratio is 5:1 (as observed in MOSAiC), and p = 4. The fraction of snowfall that is instantaneously passed to the ocean is therefore $0.75 \cdot \left(\frac{4}{1+4}\right)$, which makes geometric sense as it represents the "missing" snow over level ice.

Response:

Thank you for this careful and constructive comment. We agree that our original description of the bulk redistribution scheme was inaccurate and that the definition of P was incorrectly stated in several places.

First, the 30% value in the bulk redistribution scheme should not be interpreted as a direct observational estimate that 30% of snowfall is lost because of surface heterogeneity such as leads and ridges. Rather, it originates from the observation of Sturm et al. (2002) that snow around ridged ice was approximately 30% greater than that on level ice. In reality, this contrast may result from a combination of processes, including wind-driven redistribution of snow from level ice toward ridged areas, snow loss into leads, sublimation, and snow incorporation during ridging. The Icepack bulk redistribution scheme represents these unresolved effects in a simplified way by diverting a fraction of incoming snowfall into the ocean through leads. We have revised the text to clarify this distinction.

Second, we agree that P itself does not represent the ratio of snow mass on ridges to that on level ice. Instead, 1+ P represents this ratio, while P represents the excess snow mass on ridged ice relative to level ice. Therefore, the tested range of P=0.3 to P=4 corresponds to ridged-to-level snow mass ratios of 1.3 to 5. We have corrected the description of P throughout the manuscript, including the Abstract, Data and Methods, and Results sections.

"Icepack includes two snow redistribution processes: bulk redistribution and snwITDrdg redistribution. In the bulk redistribution scheme, **the parameterization is motivated by SHEBA observations showing that snow in the vicinity of ice ridges was approximately 30% greater than that on level ice (Sturm et al., 2002). This observed contrast may result from several physical processes, including wind-driven redistribution of snow from level ice toward ridged areas, snow loss into leads, sublimation, and snow incorporation during ridging. In Icepack, these unresolved processes are represented in a simplified way by assuming that a fraction of incoming snowfall over level ice is instantaneously diverted into the ocean through leads, thereby reducing the effective snowfall contributing to snow accumulation on level ice** (Hunke et al., 2022)."

Abstract Lines 17–18:

"Sensitivity experiments **indicate that setting P=4, corresponding to a ridged-to-level snow mass ratio of 1+P=5,** in the bulk redistribution scheme yields the best agreement with snow observations (MAE = 6.2 mm)."

Data and Methods Line 154:

"**In the bulk redistribution scheme, 1+P represents the ratio of snow mass on ridged ice to that on level ice, while P represents the excess snow mass on ridged ice relative to level ice**."

Results Line 239:

"**Specifically, we focus on optimizing P, which represents the excess snow mass on ridged ice relative to level ice in the bulk redistribution scheme**."

Data and Methods, Lines 155-184: Incomplete descriptions of the two snow redistribution parameterizations

- Line 171: Could the authors include a clear explanation of the exponential component of this equation and its significance? First, what does σ_{ref} represent? Second, from my understanding, the rationale of the exponential term is that more ridged ice (higher σ_{ITD}) will shelter and collect more snow along ridges, thus allowing reduced wind transport into leads; please include a summary along these lines.
- Lines 157-168: How is the wind speed threshold V^* determined, and is snow density fixed or time-evolving in Icepack? We feel that details are lacking on these and other aspects of this redistribution parameterization. It is mentioned here that Icepack assumes a fixed snow density of 330 kg m^{-3}, while later noting that Icepack "accounts for wind-induced compaction … into denser wind slabs." This section is confusing and appears internally inconsistent; clarification is needed.
    - For your awareness, the Icepack documentation (https://cice-consortium-icepack.readthedocs.io/en/main/science_guide/sg_snow.html#snow-redistribution-and-compaction-by-wind) appears to have a unit error in its specification of V^*, which contains the quantity ($P_s - \beta$), where P_s is in kg m^{-3} and β = 44.6 kg s m^{-4}. We hope your description in this section can shed light on the formulation of this parameterization, which is otherwise rather opaque.
- Additionally, well-validated, commonly used estimates of the wind speed required for blowing snow to occur on sea ice are generally around 7–8 m s-1. See, for example, the threshold wind speed formulation of Li & Pomeroy (1997), which was validated using MOSAiC data by Ranjithkumar et al. (2025). Why does Figure 1d show V* to be so much lower than these estimates, around 3.5 m s-1? Additionally, why does the yellow V* line in Figure 1d exhibit slight bumps, such as in May 2020?

- Lines 167-168: Can you include a link to the Icepack technical documentation (https://cice-consortium- icepack.readthedocs.io/en/main/science_guide/sg_snow.html) so readers know where to go for more information?

- Line 169: This section doesn't detail *how* the remaining snow is redistributed across the ice thickness categories. On a basic physical level, does further redistribution enhance snow depth on ridges and erode snow depth on level ice, or vice versa?

- Lines 179-180: Is it accurate that "Icepack assumes a uniform snow thickness within each category"? This seems inconsistent with level and ridged ice within a thickness category having different snow thicknesses.

Response:

Thank you for these detailed and helpful comments. We agree that the previous description of the two snow redistribution parameterizations was incomplete and potentially confusing. We have revised this section to clarify the physical meaning of the exponential term, the definition of σref, the calculation of the wind-speed threshold V^{*}, the treatment of snow density, and the redistribution of suspended snow among ice thickness categories.

For the snwITDrdg scheme, σref is a reference standard deviation of the ice thickness distribution, set to 1 m following the Icepack parameterization. The exponential term $\exp(-\frac{\sigma ITD}{\sigma ref})$ reduces the fraction of suspended snow lost to leads as the ice thickness distribution becomes rougher. Physically, a larger σITD indicates more deformed or ridged ice, which can shelter and trap drifting snow more effectively, thereby reducing wind-driven snow loss into leads.

We also clarified the definition of V^{*}. In this Icepack parameterization, V^{*} is not the general threshold wind speed for the onset of blowing snow over sea ice. Instead, it is the wind speed associated with compacting snow to an effective density ρ_s, following the empirical relationship used by Lecomte et al. (2013):

$$\rho_s = 44.6V^{*} + 174$$

and therefore

$$V^{*} = \frac{\rho_s - 174}{44.6}$$

Using the default Icepack snow density of 330 kg m^{-3}, gives $V^{*} \approx 3.5$ m s^{-1}. This explains why the V^{*} shown in Fig. 1d is lower than the commonly reported 7–8 m s^{-1} threshold for the onset of blowing snow. The slight temporal variations in V^{*}, including the small bumps in May 2020, arise from changes in the effective snow density when wind-induced compaction is active, particularly after snowfall events. We have revised the text to make clear that the fixed value of 330 kg m^{-3} is the default/background snow density, whereas the snwITDrdg scheme can modify the effective upper-layer snow density through wind compaction.

Finally, we clarified that the remaining suspended snow is redistributed locally among ice thickness categories, with preferential redeposition over ridged/deformed ice. Thus, the scheme tends to erode snow from more exposed level ice and enhance snow accumulation near ridges, while neglecting non-local transport between grid cells. We also revised the statement about uniform snow thickness to specify that this assumption applies to the thermodynamic calculation within each ice thickness category, whereas the redistribution and radiation calculations distinguish level and ridged ice effects. A link to the Icepack technical documentation has also been added.

"where ρ_{max} and ρ_s are the maximum snow density and the effective snow density in Icepack, respectively. The value of ρ_{max} is set to 400 kg m-3 (Macfarlane et al., 2023). After erosion begins, snow particles start to suspend when the instantaneous wind speed V exceeds a threshold (V^*). σITD is the standard deviation of the sea ice thickness distribution within a grid cell (Hunke et al., 2022). γ is a tuning coefficient that modulates the snow erosion rate, set to 1×10-5 kg m-2 (Lecomte et al., 2015). **In Icepack, the default/background snow density is 330 kg m-3, but the snwITDrdg scheme can also account for wind-induced compaction of the upper snow layer into denser wind slabs. The threshold V^* is calculated from the effective snow density following Lecomte et al. (2013), using $\rho_s = 174 + 44.6V^*$. Therefore, when $\rho_s = 330$ kg m-3, V^* is approximately 3.5 m s-1. This V^* represents the wind speed associated with compacting snow to the effective density used in the parameterization. Further details are provided in the Icepack technical documentation (Hunke et al., 2022).**

The remaining suspended snow particles are redistributed over the ice surface as:

$$\Phi^R_{local}\Delta t = a_i(1-f)\Phi^T_{local}\Delta t \quad (3)$$

Where f represents the fraction of suspended snow particles lost within the leads (Lecomte et al., 2013):

$$f = (1-a_i)\exp\left(\frac{-\sigma ITD}{\sigma ref}\right) \quad (4)$$

Where σref=1 m, and a_i is the area of the ice within a grid cell. **Here σref is a reference scale for the standard deviation of the ice thickness distribution. The exponential term indicates that snow loss to leads decreases as σITD increases. Physically, a larger σITD reflects more ridged or deformed ice, which can shelter drifting snow and promote snow accumulation near ridges, thereby reducing the fraction of suspended snow transported into leads. The remaining suspended snow is redistributed locally among the ice thickness categories, preferentially increasing snow accumulation over ridged/deformed ice while reducing snow over more exposed level ice**."

Results, Line 286 (also Lines 293, 306, 357): Potential issue with comparison to Clemens-Sewall et al. (2023)

We believe it is incorrect to cite this statistic as "SWE per lead area". It should be just "SWE". Clemens-Sewall et al. (2023) state that the 7–11 mm figure is an area-averaged loss of snow (see their abstract: "a removal… from the entire surface"). Can the authors confirm that the metric presented in Figure 3b and 3c represents accumulated snow loss averaged over the entire Icepack column, not just "per lead area", and is therefore an appropriate comparison

with Clemens-Sewall et al.?

Response:

Thank you for pointing this out. We confirm that the metric shown in Fig. 3b and 3c represents accumulated snow loss expressed as SWE and averaged over the entire Icepack column. Therefore, it is consistent with the area-averaged snow loss reported by Clemens-Sewall et al. (2023). We agree that our previous wording "per lead area" was incorrect and misleading. We have revised the relevant text in the Results section and the figure caption by replacing "per lead area" with "area-averaged SWE" or "accumulated snow loss expressed as SWE", as appropriate.

Discussion/Conclusions, Lines 341-345: Errors and lack of clarity in summary of results

We are confused why there is a disconnect between the authors' summary of their most important results here and what they have presented earlier. These two sentences make several seemingly inaccurate claims about the "delayed seasonal peak" and "earlier snow melt in summer" relative to observations—claims that are neither discussed previously in the Results, nor supported by the authors' simulations (Fig. 1d):

- First, a "delayed seasonal peak" applies equally to the no-redistribution, bulk redistribution, and blowing snow experiments (with a very slight earlier shift in the blowing snow experiment), but the description implies that this bias only applies to the no-redistribution case.
- Second, the timing of snow melt initiation (a component of "seasonal timing") appears identical between the observations and all three simulations. Rather, it is the magnitude and rapidity of snow melt that varies between them.
- Third, the bulk redistribution experiment shows no distinguishable change in the timing of the seasonal peak or melt initiation compared to the no-redistribution experiment.

Additionally, we don't understand how an overestimation of snow in spring and winter causes earlier snow melt in summer (Lines 342-343). Please clarify this in the text.

Response:

Thank you for this careful comment. We agree that the previous summary overstated the differences in seasonal timing and was not fully consistent with the results shown earlier. In particular, the delayed seasonal peak is not unique to the no-redistribution experiment, and the timing of melt initiation is similar among the observations and all three simulations. The main differences among the experiments are the magnitude of snow accumulation and the rapidity of snow melt, rather than a clear shift in melt onset.

We have therefore revised this part of the Conclusions and Discussions section. Specifically, we removed the statement that overestimated winter and spring snow thickness resulted in earlier summer melt, because this causal interpretation was not supported by the simulations. We also clarified that the bulk redistribution scheme mainly reduces snow accumulation, while the snwITDrdg scheme further reduces snow thickness and improves the magnitude of the simulated seasonal evolution, although biases remain during snowfall events and the timing of the seasonal peak.

"Our results show that without snow redistribution, simulated snow thickness is significantly overestimated in winter and spring. The bulk redistribution scheme slightly reduces snow accumulation, **but does not substantially change the timing of the seasonal peak or melt onset.** The snwITDrdg redistribution scheme further reduces snow thickness a**nd improves the magnitude of the simulated seasonal evolution, though biases persist, especially during snowfall events and around the seasonal peak."**

Discussion/Conclusions: Assumption that sublimation of blowing snow is negligible

By optimizing the model parameters p and γ to reduce mismatch with the observed snow depths during MOSAiC, the authors are implicitly assuming that the two model schemes for snow redistribution are fully capturing (or at least parameterizing) the key processes that cause snow mass loss from level ice. However, this is likely a flawed assumption. Sublimation of drifting and blowing snow was clearly active during MOSAiC (see, e.g., Robinson et al., 2025). By neglecting sublimation as a cause of snow mass loss, this study's optimization of p and y may be apportioning an excessive amount of snow loss from level ice to Icepack's representations of lead trapping and wind-driven redistribution. This should be discussed in the text. For example:

- In Lines 377-379, the authors briefly acknowledge the importance of sublimation of blowing snow but then seem to dismiss its importance for unclear reasons, all within the same sentence.
- In Lines 367-370, the authors attribute the overestimation of snow accumulation during the significant snowfall events from May 6-13 solely to observational errors, neglecting the potential role of blowing snow sublimation or other unrepresented processes.

Response:

Thank you for this important comment. We agree that sublimation of drifting and blowing snow was active during MOSAiC and that neglecting this process may affect the interpretation of our optimized parameters. Our intention was not to imply that sublimation is negligible. Rather, the current Icepack configurations tested in this study do not explicitly separate snow mass loss by sublimation from snow loss into leads or other unresolved redistribution pathways. Therefore, optimizing p and γ may partly compensate for missing or simplified processes, including blowing-snow sublimation.

We have revised the Discussion/Conclusions section to clarify this limitation. In particular, we no longer attribute the May 6–13 snow-thickness discrepancy solely to observational or reanalysis errors. Instead, we note that the overestimation may also result from unresolved processes such as blowing-snow sublimation, non-local snow transport, or limitations in the treatment of snow density and compaction. We also revised the statement on suspended-snow sublimation to avoid dismissing its importance, and we now explicitly state that the optimized p and γ should be interpreted as effective parameters that compensate for multiple unresolved snow-loss processes, rather than as direct constraints on lead trapping or wind-driven redistribution alone.

"For example, the PWD–ERA5 average shows increased snowfall between May 6 and 13 (Figure 1c), while observed snow thickness declined, suggesting potential errors in observations or reanalysis (Wagner et al., 2022). **However, this discrepancy may also reflect unresolved snow-loss processes, including sublimation of drifting and blowing snow, non-local snow transport, and limitations in the model representation of snow compaction and density evolution.**"

"The treatment of suspended snow particles is simplified in Icepack: all excess snow is assumed to enter leads. While sublimation of suspended snow represents an important loss pathway in the Arctic (Liston et al., 2020, Robinson et al. 2025), **it is not explicitly represented in the present model configuration. As a result, the optimized P and γ values should be interpreted as effective parameters that may compensate for multiple unresolved snow-loss processes, including lead trapping, wind-driven redistribution, and blowing-snow sublimation, rather than as direct estimates of lead-related snow loss alone.**"

Data Availability, Lines 386-394: Archiving code and model output

The Cryosphere encourages authors to publicly archive their analysis code and describe its availability in a "Code availability" section (see https://www.the-cryosphere.net/submission.html; "Authors are encouraged to deposit software, algorithms, and model code in FAIR-aligned repositories/archives whenever possible"). Additionally, the journal requires authors to publicly archive any associated data necessary to validate the research findings, which includes model output, to the extent possible (see https://www.the-cryosphere.net/policies/data_policy.html).

Repositories such as Zenodo are an appropriate location for both code and data archives. These steps are important for ensuring the future reproducibility of your results and the openness of science, and we encourage the authors to follow the journal's guidance for code and model output archival.

Response:

Thank you for this helpful suggestion. We agree that archiving the analysis code and model output is important for reproducibility and openness. The analysis scripts and processed Icepack model output used to generate the figures and main results will be archived in Zenodo after manuscript acceptance, once the final versions of the figures, data processing, and model output have been confirmed. The DOI will be provided in the final published version.

<u>Minor/typographical comments</u>:

<u>Abstract / Plain Language Summary</u>:

- General comment: The Abstract should introduce what was done in the analysis, which focused on two parameterizations that treat the redistribution of snow on sea ice. That's not clear in the Abstract in its current form.
- Line 16 (also Lines 49, 54, 56, 145, and others): Why is the blowing snow scheme referred to using a model-specific parameter (snwITDrdg) while the bulk redistribution scheme is not? It would be clearer and consistent to refer to both using their physical descriptions ("bulk redistribution" for the first; "lead trapping of blowing snow" or just "blowing snow" for the second).
- Lines 16-17: "… shows biases during snowfall events" is vague—can this be more specific?
- Line 19: "MAE" should be defined here since this is the first instance of the abbreviation.
- Lines 19, 23: Including the MAE for the two tuned model outcomes is not informative in the Abstract unless the MAE for the baseline simulation (without snow redistribution or blowing snow) is also cited. Alternatively, the statistics would be more relevant if the fractional (%) decrease in MAE for the two tuned model outcomes were provided instead of the two absolute MAE values.
- Lines 25-26: "Arctic snow thickness modeling" could be changed to "modeling of snow thickness on Arctic sea ice" to differentiate from terrestrial Arctic snow, for clarity.

Response:
Thank you for these helpful comments. We have revised the Abstract to more clearly state that the analysis focuses on two snow redistribution parameterizations in Icepack: bulk redistribution and blowing snow redistribution. We also replaced the model-specific term "snwITDrdg" with the more physically descriptive term "blowing snow redistribution" in the Abstract and throughout the manuscript where appropriate, while retaining the model name only when needed for technical clarity.
We also clarified the statement about biases during snowfall events by specifying that the simulations still overestimate snow accumulation during major snowfall events. In addition, we defined mean absolute error (MAE) at its first occurrence. To make the performance statistics more informative, we revised the Abstract to report the reduction in MAE relative to the baseline simulation rather than only listing the tuned MAE

values. Finally, we changed “Arctic snow thickness modeling” to “modeling of snow thickness on Arctic sea ice” for clarity.

“Snow plays a critical role in the mass and energy balance of sea ice through its insulating properties and high albedo. Based on observations from the Multidisciplinary Drifting Observatory for the Study of Arctic Climate (MOSAiC) campaign, we assess the influence of **two Icepack snow redistribution parameterizations, bulk redistribution and blowing snow redistribution,** on snow thickness simulations using the Icepack column model. Our results show that, without snow redistribution, snow thickness is overestimated in winter and spring. The bulk redistribution scheme slightly reduces snow accumulation, while the blowing snow scheme further increases agreement with observations but still **overestimates snow accumulation during major snowfall events. Sensitivity experiments indicate that setting P=4, corresponding to a ridged-to-level snow mass ratio of 5, in the bulk redistribution scheme yields the best agreement with snow observations (mean absolute error (MAE) reduced by approximately 73% relative to the baseline bulk experiment)**. In the blowing snow scheme, the snow erosion coefficient is treated as an effective tuning parameter. When observed sea ice concentration is prescribed, setting the snow erosion coefficient to 2.4×10-5 produces simulated accumulated snow loss to leads consistent with observations and improves the simulated snow thickness **(MAE reduced by approximately 51% relative to the original blowing snow experiment)**. This study provides new insights into snow thickness simulation and the parameterization of snow redistribution, offering valuable guidance for improving **modeling of snow thickness on Arctic sea ice.**”

Introduction:

- Lines 33-35: Consider rephrasing for accuracy. Even if it is thin, the presence of snow helps increase the surface albedo and delay the surface melt of sea ice. Similarly, any melting snow cover, regardless of its thickness, contributes to melt pond formation in the Arctic.

Response:

Thank you for these helpful suggestions. We now state that snow cover generally increases surface albedo and delays surface melt, while melting snow can contribute to meltwater production and melt-pond formation.

“In winter, snow insulates sea ice from the atmosphere, suppressing ice growth. In summer, **the presence of snow generally increases surface albedo and can delay the onset of surface melt; once melting begins, snowmelt can contribute to meltwater production and melt-pond formation, enhancing solar energy absorption**”

- Line 37: This should be “on Antarctic first-year ice”, not “in Antarctic first year ice”.

Response:

Revised as suggest

- Line 38: "Increasingly reported" is not quite correct. Flooding and snow-ice has been observed on occasion in the Arctic, but there has been no observational evidence of snow-ice formation increasing in frequency or prevalence over recent decades. Additionally, note that the comma should be omitted before "and increasingly".

Response:

Thanks for your advice. We change the "increasingly reported" to "occasionally observed"

- Lines 45-46: What simulations, and at what scale? The current wording makes for an excessively broad claim that is inaccurate for existing snow models that explicitly represent small-scale topographic variations and redistribution processes on sea ice, e.g., SnowModel (Liston et al., 2018). The language should also be clarified in this sentence that the statement is referring to simulations of snow on sea ice, not terrestrial snow.

Response:

Thank you for this helpful comment. We have revised the wording to clarify that we are referring specifically to simulations of snow thickness on sea ice. We also softened the statement to avoid making an overly broad claim. The revised text now emphasizes that the spatial distribution of snow thickness on sea ice is primarily controlled by snowfall, but is also strongly shaped by wind-driven snow redistribution.

"**The spatial distribution of snow thickness on sea ice is primarily controlled by snowfall (Wagner et al., 2022), but it is also strongly shaped by wind-driven snow redistribution.**"

- Line 47: Icepack v1.3.2 was released a while ago, in August 2022, and the current version is now 1.5.3. It might be appropriate to acknowledge this; the authors may also consider if any model updates since 2022 are significant enough to affect their results and warrant re-running the simulations using the current Icepack version.

Response:

Thank you for pointing this out. We have clarified that Icepack v1.3.2 was the version used in this study. Although newer Icepack versions have since been released, we retained v1.3.2 to maintain consistency with the model configuration used in our experiments and focused on evaluating the snow redistribution parameterizations implemented in that version. We have added this clarification to the Introduction.

"The Icepack 1.3.2 sea ice column model used in this study incorporates a parameterization for snow redistribution (Lecomte et al., 2013), yet many of its empirical coefficients have not been updated with recent observations or systematically validated. **Although newer versions of Icepack have since been released,**

we used Icepack 1.3.2 to maintain consistency with the model configuration used in this study and focused on evaluating the snow redistribution parameterizations implemented in that version."

- Line 50: The rule-of-thumb parameterization is based on only a single study, so this should be "an observational study" (singular), not "previous observational studies" (plural).

Response:

Revised as suggest

- Line 54: The redistribution scheme is a parameterization, not a "crucial physical process." Again, the model-specific abbreviation "snwITDrdg" should not be used as a stand-in for the actual physical process, which is lead trapping of blowing snow (see above).

Response:

Thanks for your advice. We have revised the text in response to this concern:

"The spatial distribution of snow thickness on sea ice is primarily controlled by snowfall (Wagner et al., 2022), but it is also strongly shaped by wind-driven snow redistribution"

- Line 59: Van den Broeke et al. (2004) is focused on the surface mass balance of snow on the Antarctic continent and is not an appropriate reference for a statement about blowing snow loss to leads.

Response:

Thank you for pointing this out. We have removed this citation from the statement.

- Line 66: Here and elsewhere, caution is needed in interpreting a single field campaign as representative of all Antarctic (or Arctic) conditions. This summary of Leonard & Maksym (2011) overstates the certainty with which that study presents this result (see their wording: " approximately half of the precipitation over sea ice *could* be lost to leads") and also overstates the certainty of the current state of knowledge regarding this poorly constrained process.

Response:

Thank you for this helpful comment. We revised the text to "In the Southern Ocean, Leonard and Maksym (2011) suggested that approximately half of the precipitation over sea ice could be lost to leads under the observed conditions."

- Line 73: It is not accurate to describe NESOSIM as a "data assimilation product"; it is a reanalysis-forced reconstruction model of snow evolution on sea ice.

Response:

Thank you for this correction. We have revised the wording to describe it as a reanalysis-forced reconstruction model of snow evolution on sea ice.

- Lines 74-75: This claim ("have not been extensively evaluated against detailed observations") is both vague wording and an inaccurate characterization of NESOSIM, which has been evaluated and/or calibrated using Operation IceBridge observations (Petty et al., 2018), additional buoy and station in situ observations (Cabaj et al., 2023), and airborne and MOSAiC in situ observations (Cabaj et al., 2025).

Response:

Thank you for pointing this out. We have revised the sentence to acknowledge that existing snow redistribution approaches and reconstruction models have been evaluated with several observational datasets, while emphasizing that uncertainties remain in the specific representation of snow loss into leads and blowing-snow redistribution during MOSAiC.

"Parameterizations of snow redistribution by wind have been applied in climate models (Olivier Lecomte et al., 2015) and **reanalysis-forced reconstruction models of snow evolution on sea ice, such as NESOSIM** (Petty et al., 2018)."

- Lines 76-77: The first part of this sentence is redundant with the previous paragraph.

Response:

Thank you for this comment. We have removed the redundant wording and revised the transition to focus directly on why Icepack is evaluated in this study.

"**In Icepack 1.3.2, the empirical coefficients in the snow redistribution parameterization remain uncertain and have not been systematically constrained using recent MOSAiC observations."**

- Line 83: Typo: "atmospheric".

Response:

Revised as suggest

Data and methods:

- Lines 94-95: "… within each grid cell" doesn't make sense here because Icepack only simulates one "grid cell" (i.e., vertical column). Additionally, if the grid cell doesn't interact with adjacent grid cells, can the authors describe how the ice dynamics are handled, with particular regard to the ridged ice fraction?

Response:

Thank you for this clarification. We agree that the phrase "within each grid cell" was misleading for the single-column Icepack configuration used here. We have revised the text to state that Icepack represents a single vertical column and partitions the ice area into thickness categories within that column. We also clarified that lateral advection between grid cells is not represented; ridging and the ridged-ice fraction are treated through the model's mechanical redistribution scheme within the column.

"Subgrid ice classes are described by dividing the ice area fraction into discrete thickness categories **within the single Icepack colum**. The model solves a set of equations that represent ice dynamics, thermodynamics, and mechanical redistribution (ridging). **In this single-column configuration, lateral interactions with adjacent grid cells are not represented; changes in the ridged-ice fraction are handled through the prescribed ice thickness distribution and the mechanical redistribution scheme within the column**."

- Lines 97-98: This sentence is too vague and can be deleted, because the MOSAiC forcing data are described immediately after, in Section 2.2.

Response:

Revised as suggest

- Lines 99-100: Please add a sentence at this location specifying the time range of the model simulation, both start and end dates. Then, move the existing sentence on these lines to the next section (Section 2.2) and describe in that section exactly what observational data from MOSAiC were used for model initialization.

Response: Thank you for this helpful suggestion. We have added the simulation period in Section 2.1 and moved the initialization information to Section 2.2,

"The focus is on the effects of snow redistribution parameterization on snow thickness simulation. **The Icepack simulation was conducted December 2019 to July 2020**."

"**The Icepack was initialized using MOSAiC observations of initial snow thickness and ice thickness** from the Snow and Ice Mass Balance (SIMBA) array (Lei et al., 2022)."

- Lines 100-101: Several relevant items:

- Please describe the model settings here or provide them in a table so that readers don't have to dig into a series of papers in the literature (which we assume to be the case because the plural

  "studies" is used by the authors, but only one study is cited).

- Include a description here of how the ocean is represented in Icepack—is it a slab ocean layer?

  Note that snow deposition into leads is a key process discussed in this study; is the ocean salinity affected by the freshwater input from snow, resulting in an increase in stratification and decrease in ocean heat fluxes to the sea ice, or are these potential feedbacks neglected?

- Lastly, fix the grammar: "… model settings and physical parameterizations …".

Response:

Thank you for these helpful comments. We have revised this part of the Methods section to provide a clearer description of the model configuration and corrected the grammar to "model settings and physical parameterizations". We also added a table summarizing the key Icepack settings used in this study, including the number of ice thickness categories, snow redistribution options, prescribed atmospheric and oceanic forcing, and sea ice concentration settings, so that readers do not need to rely on previous studies for these details.

We also clarified how the ocean is represented in the present Icepack single-column configuration. In this study, ocean temperature, salinity, and current forcing are prescribed from observations and used as external forcing for the ice column. Snow deposited into leads is treated as an immediate snow mass flux from the ice-snow column to the ocean. However, the resulting freshwater input is not allowed to feed back onto ocean salinity, ocean stratification, or ocean-to-ice heat fluxes. We have added this limitation to the revised text.

"The Icepack simulation was conducted December 2019 to July 2020. The key model settings and physical parameterizations are summarized in Table 1 (Gu et al., 2022). **In this single-column Icepack configuration, ocean temperature, salinity, and current forcing are prescribed from observations and used as external forcing for the ice column. Snow deposited into leads is treated as an immediate snow mass flux from the ice-snow column to the ocean; however, the resulting freshwater input is not allowed to feed back onto ocean salinity, ocean stratification, or ocean-to-ice heat fluxes.**"

Table 1. Detailed options of physical parameterizations and model settings for ICEPACK. SST signifies sea surface temperature.

ICEPACK	Value
Time step	3600 s
Number of layers in the ice	7
Number of layers in the snow	1
Ice thickness categories	15 (Clemens-Sewall et al., 2022)

Initial ice thickness	105 cm (mean observe)
Initial snow depth	17 cm (mean observe)
Ice thermodynamic	Mushy layer (Turner et al., 2013)
Shortwave radiation	Delta-Eddington (Briegleb and Light, 2007)
Melt ponds (superimposed ice)	Not used in this study
Ocean heat transfer coefficient	0.006 (Maykut and McPhee, 1995)
SST restoring timescale (days)	0 (use observed SST as oceanic forcing)
Ocean friction velocity minimum (m s-1)	0.0005 (Tsamados et al., 2013)

- Lines 119-120: Fix the grammar: "[seawater] temperature and salinity …".

Response:

Revised as suggest

- Lines 139-141: This sentence could be deleted since this relationship is already spoken about twice, in the Abstract (Lines 9-10) and Introduction (Lines 29-30), in similar terms.

Response:

Revised as suggest

- Line 142: Condensation is also a process included in Icepack's snow mass budget processes.

Response:

Revised as suggest

- Line 151: "Represents" is too vague. Please mention explicitly that Icepack passes this fraction of snowfall to the ocean as an immediate snow flux.

Response:

Thank you for this suggestion. We have revised the text in response to this concern.

"This observed contrast may result from several physical processes, including wind-driven redistribution of snow from level ice toward ridged areas, snow loss into leads, sublimation, and snow incorporation during ridging. In Icepack, these unresolved processes are represented in a simplified way by assuming that a fraction of incoming snowfall over level ice is instantaneously diverted into the ocean through leads, thereby reducing the effective snowfall contributing to snow accumulation on level ice"

- Throughout (e.g., Line 152): Please add equation numbers for each equation in the manuscript.

Response:

Revised as suggest

- Lines 153-154: Fix typos: "layer" should be "level". Additionally, the typical value of p = 0.3 should be introduced here, not much later in Lines 242-244.

Response:

Thank you for pointing this out. We have corrected "layer ice" to "level ice" and introduced the typical value p=0.3 at the first definition of the parameter.

"where s_f is the snowfall rate, a_{lvl} is the area of the level ice within a grid cell, a**nd 1+ p represents the ratio of snow mass on ridged ice to that on level ice, while p represents the excess snow mass on ridged ice relative to level ice.**

"

- Throughout (e.g., Lines 157, 161, 171, 249, 279, etc.): Please write variables and equations using properly formatted equation text, not plain text, and maintain consistency with the Icepack documentation. For example, use σ_{ITD} and σ_{ref}, not σITD and σref, and use $H_{s,sim}$, not HS_sim. In-line equations like the one on Line 249 should also be formatted properly as equations.

Response:

Revised as suggest

- Line 160: Consider changing "After" to "When".

Response:

Revised as suggest

- Line 164: Typo: a period is missing before "The threshold".

Response:

Revised as suggest

- Line 171: Typo: a negative sign is missing inside the exponential in the equation, i.e., $\exp\left(-\frac{\sigma_{ITD}}{\sigma_{ref}}\right)$ is the correct term, according to the Icepack documentation.

Response:

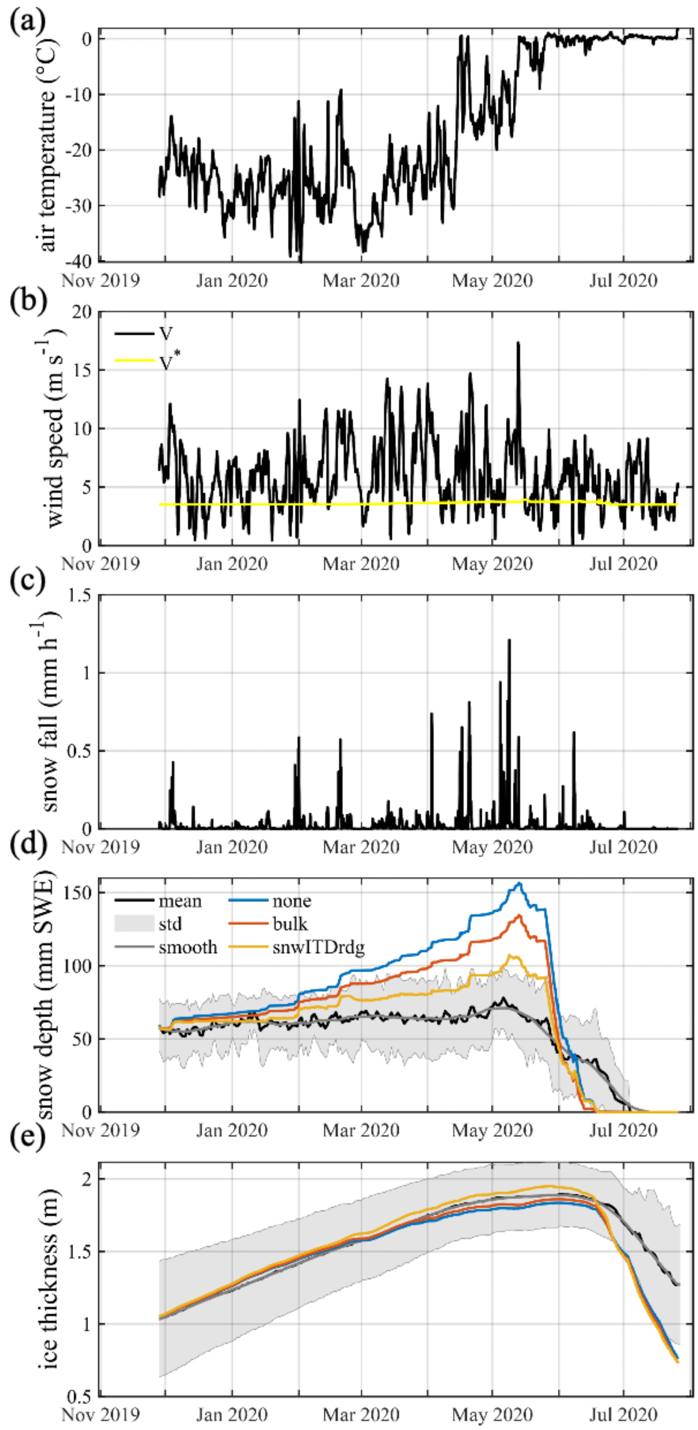
Revised as suggest

Results:

- Lines 196-205 (Figure 1 and caption):
    - In panel (b), change the legend labels "wind" and "windstar" to V and V^* for consistency with the text. In panel (d), change the y-axis label to read H_s, not Hs, and change the legend label from "ITDrdg" to "snwITDrdg" for consistency.
    - It is difficult to read the text in the figures—can you please increase the font size and figure resolution?
    - In this figure and others, we suggest reducing the x-axis limits so that there is less empty space in the left-hand side of the figure.
    - Line 197: Change "Time series simulation" to "Simulation time series".
    - Line 197 and throughout: Change "2m" to "2 m" and "10m" to "10 m", for consistency with *The Cryosphere*'s specifications for units (https://www.the-cryosphere.net/submission.html#math).
    - Line 199: Change "windstar" to V^*.
    - Line 202: Change "thin black line" to "gray line", because the line shown is not black and is not thinner than the mean observation line.
    - Line 202: Change "20-day" to "20 d" for consistency with *The Cryosphere*'s specifications for units (see above) and hyphens (https://www.the-cryosphere.net/submission.html#english).
    - Line 204: Fix grammar: change "none redistribution" to "no redistribution".

Response:

Thank you for these detailed suggestions. We have revised Figure 1 and its caption accordingly



"Figure 1. **Simulation time series** of (a) 2 m air temperature, (b) 10 m wind speed, (c) snowfall, (d) snow thickness, and (e) ice thickness. The black line represents the observation, and the yellow line in (b) represents the wind speed threshold (***V****) for snow erosion and suspension in the snwITDrdg scheme. In (d) and (e), the thick black line and grey shading represent the multi-site mean observations and their standard deviation, respectively, while th**e gray line** indicates the 20d moving average of the mean observations. The blue, red, and yellow lines represent the snow thickness simulations with **no redistribution**, bulk redistribution, and snwITDrdg redistribution, respectively."

- Line 207 (also Lines 216, 230, 313, 326, 330): For reasons of grammar and clarity, it would be better to refer to the default experiment as simply the "no-redistribution experiment" instead of the "none scheme" or "none experiment". Another reason this wording makes more sense is that an absence of snow

  redistribution is not a distinct model "scheme", but rather a distinct configuration or experiment.

Response:

Thank you for this suggestion. We have replaced "none scheme" and "none experiment" with "no-redistribution experiment" throughout the manuscript.

- Line 210: Delete "snow" in "snow SWE" due to redundancy.

Response:

Revised as suggest

- Lines 216-235: This gives a nice qualitative description of the results. The section could be strengthened by including more quantitative results.

Response:

Thank you for this helpful suggestion. We have revised this section to include more quantitative information. "For the no-redistribution experiment, simulated SWE is overestimated relative to observations starting in mid-January, particularly during periods of snowfall. The simulation peaks at 155 mm on May 13, **compared with the observed corresponding to an overestimation of about 80 mm SWE.** It then rapidly melts, with complete melt occurring by June 17. The bulk scheme reduces snow thickness slightly, but the seasonal trend remains similar, **with a peak SWE of approximately 130 mm and a mean absolute error (MAE) of approximately 23 mm SWE before June**. In contrast, the snwITDrdg scheme significantly reduces SWE compared with the no-redistribution experiment and bulk scheme, **lowering the peak SWE to approximately 105 mm and reducing the MAE to approximately 13 mm SWE before June.**"

- Lines 224-225: This is a little unclear. Does this mean that the simulated snow thickness matches more closely with observations over time?

Response:

Thank you for pointing this out. We have clarified this sentence to specify that blowing-snow redistribution improves the simulated temporal evolution of SWE by reducing excessive snow accumulation during high-wind periods.

"Under these conditions, suspended snow particles are redistributed over the ice surface or deposited into open leads (Lecomte et al., 2013, 2015; Hunke et al., 2022), thereby reducing snow accumulation and **bringing the simulated temporal evolution of SWE closer to the observations.**"

- Lines 227-228: It is important to clarify that this statement is only true until spring/summer melt.

Response:

Revised as suggest

- Lines 232-235: The model outputs surface ice melt and basal ice melt, right? Those results would be helpful to report in the results to support this speculation about increased surface exposure after snow melt.

Response: Thank you for this helpful suggestion. Yes, Icepack outputs diagnostics for surface ice melt, basal ice melt, and snow melt. We examined these diagnostics for the no-redistribution experiment, and the bulk redistribution and snwITDrdg experiments show similar temporal behavior, although with different magnitudes. The diagnostics indicate that snow melt begins first in late spring, followed by enhanced surface ice melt after the snow cover is largely depleted, while basal melt increases more gradually. This supports our interpretation that rapid snow melt increases exposure of the ice surface to atmospheric forcing and contributes to enhanced surface melt and the subsequent underestimation of ice thickness after mid-June.

Because these melt diagnostics are used here only to support the interpretation of the ice-thickness bias, and because the main focus of the manuscript is snow redistribution rather than detailed partitioning of ice melt terms, we do not add this diagnostic figure to the main text

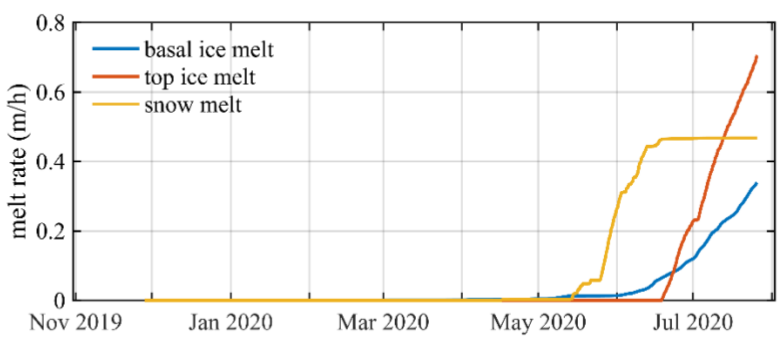


Figure R1. Time series of melt-rate diagnostics from the no-redistribution Icepack experiment, including basal ice melt (blue line), surface ice melt (red line), and snow melt (yellow line).

- Lines 239, 242, 252, 348, etc.: The variable "P" should be written as p for consistency with previous text and the Icepack documentation.

Response:

Revised as suggest

- Line 243: We suggest writing the period directly (1997–1998), rather than stating "two decades ago".

Response:

Revised as suggest

- Lines 245-246: It would be worthwhile to emphasize that this observation does not include small ridges, which make up the majority of ridged ice.

Response:

Thank you for this important clarification. We have revised the text to note that the MOSAiC estimate of large-ridge snow accumulation does not include small ridges, which make up much of the ridged-ice area. We therefore present this value as an upper-end observational constraint rather than a fully representative ridge-scale mean.

"However, observations during the MOSAiC campaign indicate that large ridges accumulated nearly five times the snow depth of level ice (Itkin et al., 2023). **Because this estimate does not include small ridges, which constitute a large fraction of ridged ice, it should be interpreted as an upper-end observational constraint.**"

- Line 248: Based on the equation on Line 152, this range is effectively reducing snowfall from between –23% and –80% (each multiplied by the level ice fractional area), right? That would seem insightful to state in the text if those numbers are correct.

Response:

Thank you for this helpful suggestion. We have added this quantitative interpretation to the text.

"we increased p from 0.3 to 4.0 to examine its impact on simulated snow and ice thickness. Based on Eq. (1), this range corresponds to $p/(1+p)$ values from approximately 0.23 to 0.80, so the fraction of snowfall passed to leads is $a_{lvl}\,p/(1+p)$."

- Lines 249-254: Please address several issues:
  - o Fix the grammar in Lines 249-252 to change these two sentences into grammatically complete sentences, not sentence fragments.

- Additionally, the equation applies to H_s, so this should read "snow thickness", not "snow and ice thickness", on Line 251. You could add a separate sentence mentioning that the MAE metric applies similarly to ice thickness.
- However, we are confused whether MAE is specified in units of mm (snow depth) or mm SWE. If it is specified in mm (snow depth), please clarify that here and in Figure 3, where mixed units of mm and mm SWE are used. Alternatively, if it is specified in SWE, please clarify that here, change H_s to a more appropriate variable name (because SWE is not snow thickness), change "mm" to "mm SWE" throughout the paper, and consider providing relevant statistics like the "~6.2 mm [SWE]" in Line 254 alongside the equivalent conversion to snow depth, as valuable context.

Response:

Thank you for these detailed comments. We have revised the MAE description into grammatically complete sentences and clarified that the snow comparison is based on SWE rather than snow depth. Therefore, the snow MAE is reported in mm SWE. We also revised the notation from Hs to SWE for this calculation. The same MAE formulation is applied separately to ice thickness using ice-thickness units.

"**The mean absolute error (MAE) was used to evaluate the agreement between simulated and observed SWE before June:** $MAE = 1/n \sum |SWE_{sim} - SWE_{obs}|$**, where n is the number of data points over the period prior to June.** SWE_{sim} **and** SWE_{obs} **the simulated and observed snow water equivalent, respectively. Therefore, the snow MAE is reported in mm SWE. The same MAE formulation was applied separately to ice thickness, with ice-thickness errors reported in m**."

- Line 255: Likely typo: "Figure 3b" should be corrected to "Figure 3c". However, if the figure panels were reordered for consistency with the text (see suggestion below), then Figure 3b would be correct.

Response:

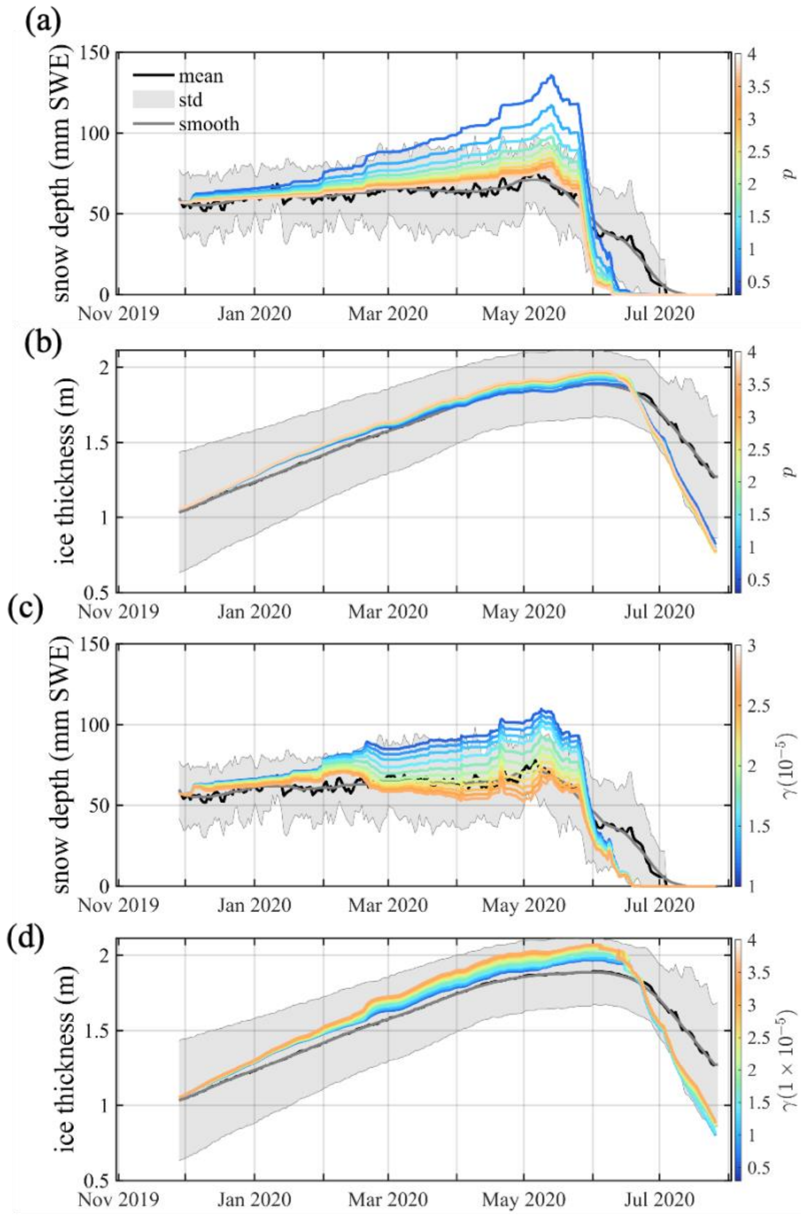
Revised as suggest

- Lines 256-263 (Figure 2 and caption):
  - Amend the y-axis labels, colorbar labels, and caption text to use proper and consistent notation for the variables (H_s, lowercase p, Y) as well as full scientific notation instead of "e" shorthand.
  - Are the simulations in panels (c) and (d) with simulated SIC or prescribed AMSR2 SIC? Please specify this in the caption. If it is only showing one, why not include both here, as in Figure 3?

- Panels (b) and (d) are not discussed in the text (except a potentially erroneous reference to Figure 2b on Line 297; see below). If these panels showing ice thickness evolution are not important, omit them; otherwise, please describe their significance in the Results section.

Response:

Thank you for this suggestion. We have revised the y-axis labels, colorbar labels, and figure caption to use consistent mathematical notation. We have clarified in the Figure 2 caption that panels (c) and (d) show the snwITDrdg sensitivity experiments with prescribed AMSR2 sea ice concentration. We chose to show this configuration because the prescribed-SIC experiments are directly constrained by observations and are used for the parameter optimization discussed in this section. The sensitivity experiments with and without prescribed sea ice concentration show consistent trends in simulated snow thickness as γ varies, although their absolute values differ. Therefore, to avoid redundancy, Figure 2 only presents the prescribed-SIC experiments, while the quantitative differences between simulations with and without prescribed AMSR2 sea ice concentration are shown in Figure 3. We have added a short discussion of panels (b) and (d) in the Results section. These panels are retained because they show that the sensitivity of ice thickness to the redistribution parameters is weaker than that of snow thickness, but still relevant for interpreting the thermodynamic response of sea ice.



“Figure 2. Time series of (a) snow thickness and (b) ice thickness from sensitivity experiments under the bulk redistribution scheme with varying P, where p is the ratio of snow depth on ridges to that on level ice, and (c) snow thickness and (d) ice thickness **show the snwITDrdg sensitivity experiments with prescribed AMSR2 sea ice concentration**. γ is the tuning coefficient controlling the snow erosion rate. In panels (a) and (b), the 10 colored lines represent sensitivity experiments with p ranging from 0.3 to 4.0. In panels (c) and (d), the 11 colored lines correspond to sensitivity experiments with the γ tuning coefficient ranging from 1×10^{-5} to 3×10^{-5} .”

“Figure 2b and 2d show that the sensitivity of ice thickness to p and γ is smaller than the sensitivity of snow thickness, indicating that the redistribution parameters primarily affect the snowpack while exerting a weaker indirect influence on sea ice thermodynamics.”

- Lines 264-265: The simulation parameterizes blowing snow and does not explicitly simulate snow particles, so it would be more accurate to say, “… an increase in Y represents an increase in the amount of suspended snow…” (or similar wording).

Response:

Thank you for this clarification. We agree that Icepack parameterizes blowing snow rather than explicitly simulating individual snow particles. We have revised the wording to state that an increase in γ represents an increase in the parameterized amount of suspended snow.

“In the snwITDrdg redistribution simulation, an increase in γ **represents an increase in the parameterized amount of suspended snow,** while a decrease in sea ice concentration enlarges the leads, which provide more open-water area to receive drifting snow.”

- Lines 271-273: It would be worthwhile to discuss deficiencies of these model physics. For example, during melt, snow cannot be redistributed by the wind—it is effectively fixed in place. The model physics do not account for this.

Response:

Thank you for this helpful suggestion. We have added a discussion of this limitation

“In addition, Icepack estimates a constant snow density of 330 kg m^{-3}, whereas actual snow density evolves with wind compaction, temperature fluctuations, moisture content, and sublimation (Liston et al., 2020; Macfarlane et al., 2023), contributing to mass and thickness biases.”

- Lines 273-274: This sentence defining Y is redundant with previous text in the Methods (Lines 162-164) and should be deleted.

Response:

Revised as suggest

- Line 276: Figure 2d should be cited here, at the end of the preceding sentence.

Response:

Revised as suggest

- Line 283: Typo: “snow fall” should be “snowfall”.

Response:

Revised as suggest

- Lines 282–287: It would make more sense to move these two sentences to the start of the following paragraph.

Response:

Revised as suggest

- Lines 285-286: Change “snow water equivalent” to “SWE” here and in any subsequent instances, because the abbreviation was previously defined and should be used consistently thereafter.

Response:

Revised as suggest

- Line 287: Should this reference be “Figure 3b and 3c”? The 7–11 mm shaded band is shown on both panels of the plot.

Response:

Revised as suggest

- Line 297: Should this refer to Figure 2a, not 2b? The sentence only mentions “simulated snow thickness” earlier, not ice thickness, though both show rapid melting between May and July (with snowmelt occurring earlier and ice melt occurring later). Either way, please clarify which metric(s) are being discussed.

Response:

Thank you for pointing this out. The relevant panel is Figure 2c

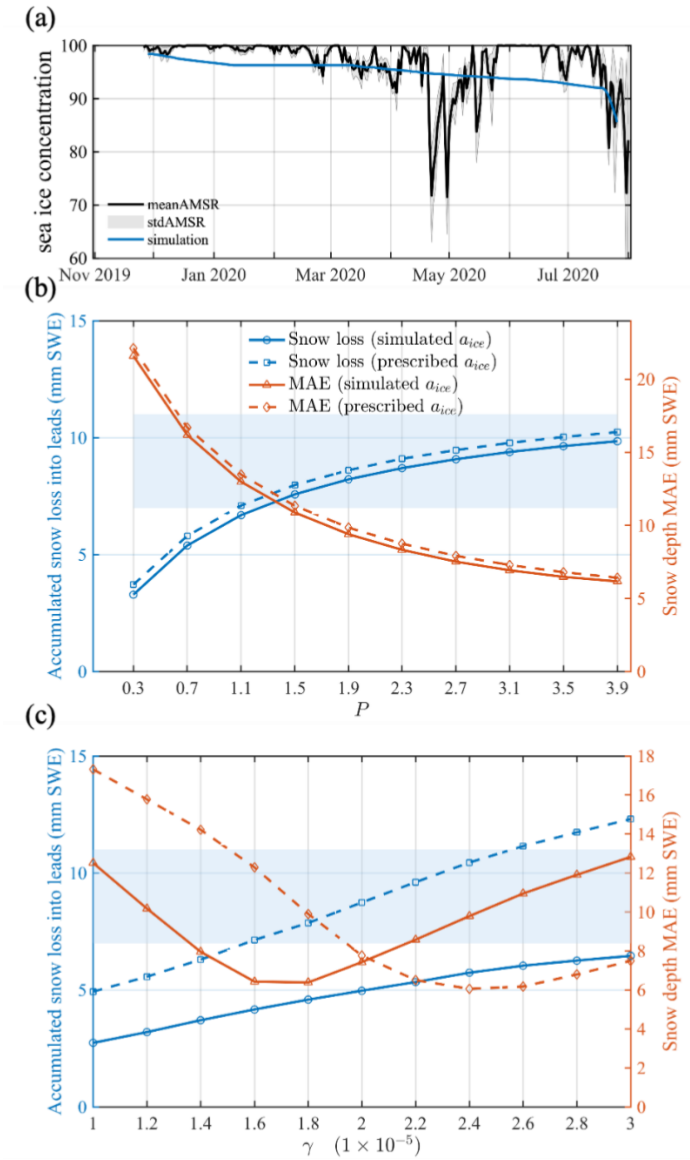
- Lines 298-308 (Figure 3 and caption):
    - o Please switch panels (b) and (c), for consistency with Figure 2 and the order of the discussed

parameterizations in the text, then amend the figure references in the text. It would also help to clarify in the caption that there are two different parameterizations being tested here.

- Amend the x-axis labels (Y, p), legend labels (e.g., σ_{AMSR}, a_{ice}), and caption text (p) to use proper and consistent notation for the variables. Change "sim" to "simulated" for clarity in panels (a) and (b), since it's the same length as writing out "prescribed". Change "lead" to "leads".
- We could be missing something, but we don't see sea ice concentration defined as the variable a anywhere in the text. If using it in the panel (b) legend, please define it in the caption.
- As mentioned above, should Lines 304, 306, and the y-axis labels be changed from "mm" to "mm SWE" to clarify that the MAE and accumulated snow loss metrics are in SWE, not units of snow depth?
- In current panel (c), the small differences between the simulations with prognostic vs. prescribed sea ice concentration are not currently discussed in the text; consider mentioning them.

Response:

Thank you for these detailed suggestions. We have revised Figure 3 and its caption accordingly. First, we switched panels (b) and (c) so that the order of the panels is consistent with Figure 2 and with the order of the parameterizations discussed in the text. We have also updated the corresponding figure references in the text. We revised the axis labels, legend labels, and caption text to use consistent notation. Finally, We have added a sentence in the Results section to discuss the small differences between simulations with model-simulated and prescribed AMSR2 sea ice concentration in the bulk redistribution sensitivity experiments. Both configurations show similar sensitivity trends as p increases, but the prescribed-SIC configuration produces slightly different accumulated snow loss because the observed sea ice concentration modifies the level-ice and lead area fractions used in the bulk. redistribution calculation.



"Figure 3. (a) Time series of sea ice concentration from AMSR2 observations and Icepack simulations**. (b) and (c) show the statistical results for sensitivity experiments for two snow redistribution parameterizations: bulk redistribution with varying p, and blowing-snow redistribution with varying γ, respectively**. The blue line and the left blue y-axis represent the accumulated snow loss into open leads during the period from 9 April to 28 April, while the orange line and the right orange y-axis indicate the mean absolute error (MAE, **in mm SWE**) between simulated and observed snow thickness. The blue shaded area denotes the observed range of accumulated snow loss into open leads (~7–11 mm **area-averaged SWE**) based on Clemens-Sewall et al. (2023). Solid lines denote simulations with model-simulated sea ice concentration, whereas dashed lines denote simulations with prescribed observed sea ice concentration. $\boldsymbol{a_{ice}}$ **denotes AMSR2 sea ice concentration**"

"The simulations with model-simulated and prescribed AMSR2 sea ice concentration show similar sensitivity trends as p increases, with only small differences in accumulated snow loss and SWE MAE. These differences arise because prescribing AMSR2 sea ice concentration slightly modifies the level-ice and lead area fractions used in the bulk redistribution calculation.

"

- Lines 309-311 (and throughout): Fix the formatting of the snow mass balance terms, which should use subscripts and correct variable notation, e.g., S_{su}. It should also be specified that snow can be lost during sea ice ridging in the real world, when a fraction of snow becomes subducted and submerged, though this process is not captured in Icepack.

Response: Thank you for this suggestion. We have revised the notation of the snow mass balance terms using proper subscripts. We also added a clarification that snow loss can occur during sea ice ridging in the real world when part of the snow is subducted and submerged, but this process is not explicitly represented in the Icepack configuration used here.

"In the snow mass balance, snow thickness is influenced by snowfall (S_f), snowmelt (S_m), snow sublimation (S_{su}), snow-ice formation (S_i), and snow loss into open leads (S_{lo}). In reality, snow can also be lost during sea ice ridging when part of the snow cover becomes subducted and submerged; however, this process is not explicitly represented in the Icepack configuration used in this study."

- Lines 315-319: This is a fascinating result. Is there a way to show the snow depth distribution across the ice thickness categories for the model simulations? That would be insightful.

Response:

Thank you for this helpful suggestion. We agree that the snow distribution across ice thickness categories is insightful for understanding the snwITDrdg parameterization. We examined the model diagnostics for category-dependent snow erosion and redistribution under the snwITDrdg scheme, which indicate that the redistribution is strongly dependent on ice thickness category. However, the SIMBA observations used in this study provide averaged snow and ice thickness over the observational region and do not include corresponding snow-depth distributions for individual Icepack thickness categories. Therefore, we do not include this diagnostic figure in the main text, because it cannot be directly evaluated against observations and may give a misleading impression of observational support at the category scale. Instead, we revised the text to better explain the modeled mechanism and its limitation.

"In addition to depositing a portion of snow mass into open leads, the snwITDrdg scheme diagnostically redistributes snow among ice thickness categories. This redistribution, modulated by the ridged-ice fraction and surface type within each category, results in increased snow thickness over thinner ice categories, making the ice more susceptible to surface flooding and subsequent snow-ice formation. **However, enhanced snow-**

ice formation is not evident in the averaged SIMBA observations used in this study. Furthermore, previous studies have suggested that Icepack may overestimate snow-ice formation (Gu et al., 2022). Therefore, this category-dependent redistribution should be interpreted as a model-diagnostic mechanism rather than as a directly validated observational feature."

- Line 316: More accurately, the model scheme deposits "snow mass" into leads, not "snow particles".

Response:

Revised as suggest

- Lines 321, 323: It could be helpful to remind the reader that the "bulkm scheme" and "snwITDrdgm scheme" are not distinct parameterizations, but rather simulations using the optimized values for the p and Y parameters.

Response:

Thank you for this suggestion. We have changed the "scheme" to "configuration".

"Compared to the bulk scheme, the bulkm **configuration** removes a larger fraction of precipitation, resulting in reduced overall snow accumulation. Meanwhile, the snwITDrdgm **configuration** increases Slo relative to snwITDrdg, thereby reducing snowmelt at the end of the simulation. The Ssu values are relatively consistent across all snow redistribution schemes."

- Lines 323-328: Please avoid using the variables (e.g., S_{lo}, S_{su}) when the words are easy to use. This will improve the clarity of the text.

Response:

Thank you for this suggestion. We have revised the text to use descriptive terms.

"Meanwhile, the snwITDrdgm configuration increases **snow loss into lead**s relative to snwITDrdg, thereby reducing snowmelt at the end of the simulation. The **sublimation** losses are relatively consistent across all snow redistribution schemes. Prescribing sea ice concentration has minimal effect on the no-redistribution experiment and bulk schemes. However, within the snwITDrdg and snwITDrdgm scheme, prescribing sea ice concentration leads to a significant reduction in **snow loss into leads** and a corresponding increase in **snowmelt**."

- Lines 325-328: It would be helpful to expand on why there are differences between the snow redistribution schemes with regard to prescribed sea ice concentrations.

Response:

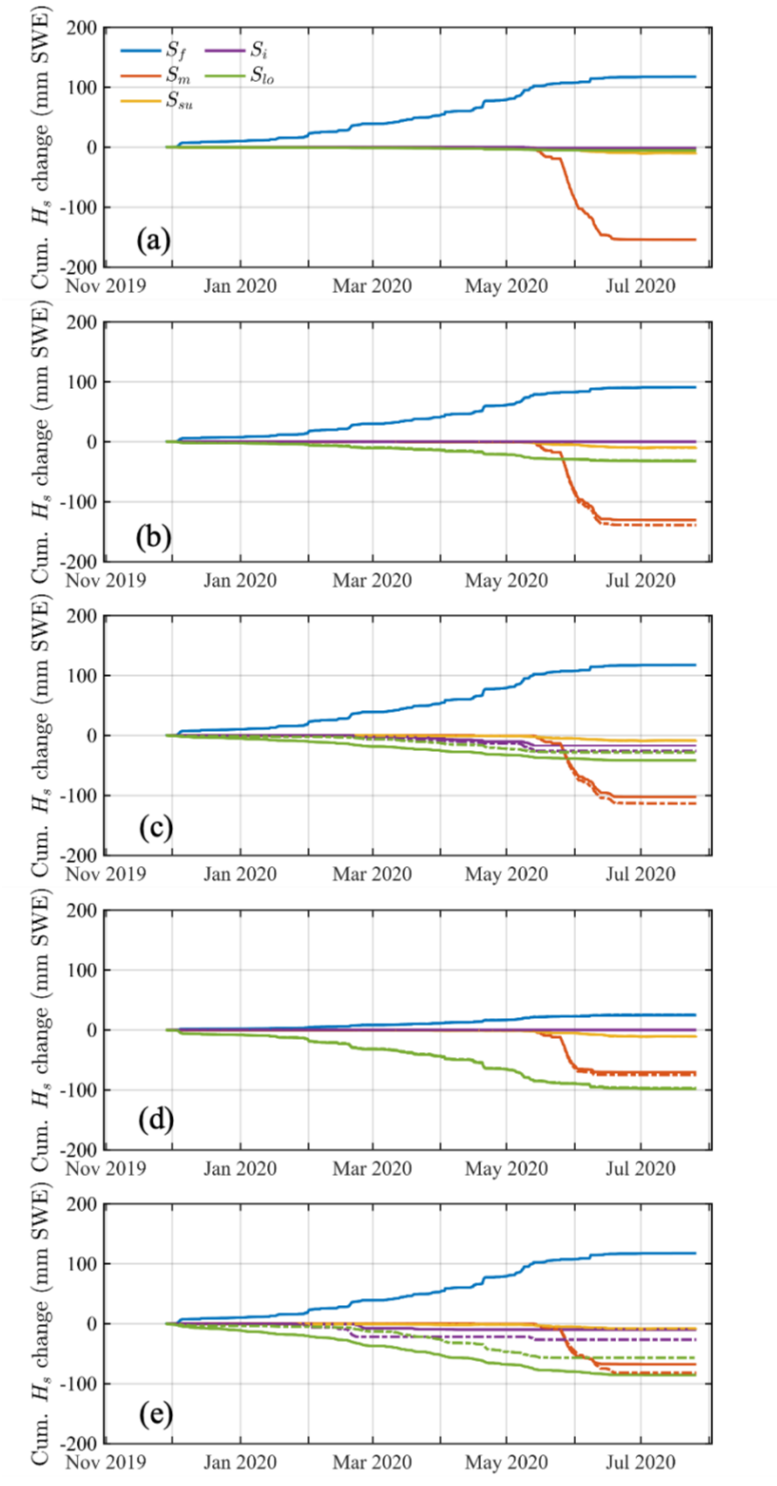
Thank you for this helpful comment. We have expanded the discussion to explain why prescribing sea ice concentration affects the schemes differently. In the bulk redistribution scheme, snow loss to leads is controlled mainly by the prescribed fraction of incoming snowfall diverted through the level-ice area fraction, so the impact of sea ice concentration is relatively small. In the snwITDrdg scheme, however, snow loss into leads depends more directly on the open-water fraction and the redistribution of wind-eroded snow. Therefore, prescribing observed sea ice concentration, which includes stronger lead-opening events, changes the amount of snow available for lead loss and alters the balance between snow loss into leads and later snowmelt.

"This difference arises because the bulk redistribution scheme primarily reduces incoming snowfall through a prescribed level-ice-dependent fraction, making it less sensitive to changes in sea ice concentration. In contrast, the snwITDrdg scheme redistributes wind-eroded snow into open leads, so its snow-loss term depends more directly on the open-water fraction. Prescribing AMSR2 sea ice concentration therefore modifies the amount of snow lost to leads and changes the remaining snow available for subsequent melt."

- Lines 329-335 (Figure 4 and caption):
    - o Line 330: Change "snow changes" to "snow depth changes" or "snow thickness changes" for clarity.
    - o Panel (a) legend and Lines 332-333: See comment above regarding formatting of the snow mass balance variables.

Response:

Thank you for these suggestions. We have revised the Figure 4 caption to clarify that the figure shows cumulative snow thickness changes. We also corrected the formatting of the snow mass balance terms in both the figure legend and the caption



"Figure 4. Time series of cumulative snow **depth** changes in (a) no-redistribution, (b) bulk, (c) snwITDrdg, (d) bulkm and (e) snwITDrdgm redistribution configurations. **Different colored lines represent snowfall (S_f), snowmelt (S_m), snow sublimation (S_{su}), snow-ice formation (S_i), and snow loss into open leads (S_{lo}),** respectively. Solid lines denote simulations without prescribed AMSR2 sea ice concentration, while dashed lines indicate simulations with prescribed AMSR2 sea ice concentration."

Conclusions and Discussions:

- Line 336: Fix grammar: "Discussions" should be changed to "Discussion". If keeping these two together, "Discussion" should precede "Conclusions"—but please note the recommendation at the top about separating the sections.

Response:

Thank you for this suggestion. We have separated the Conclusions and the Discussions.

- Line 339: Only two snow redistribution schemes were used, so "various" should be changed to "two". As mentioned above, an absence of snow redistribution is not a "scheme". Experiments with varying parameter values are also not distinct schemes; these are more accurately model "configurations".

Response:

Thank you for this clarification. We agree that "various" was imprecise. We have revised the wording to state that we tested two snow redistribution parameterizations and multiple model configurations, including a no-redistribution baseline and sensitivity experiments with different parameter values.

"Using the Icepack column model, we simulated snow thickness during the MOSAiC campaign **under two snow redistribution parameterizations and multiple model configurations**"

- Line 340: Fix grammar: change "parameterization" to "parameterizations".

Response:

Revised as suggest.

- Lines 349-350: Is it possible that the snowfall forcing was biased high, so that the snow thickness results would be in stronger agreement with the observations if a more extreme loss to leads was implemented? This aspect would be insightful to expand on in a Discussion section.

Response:

Thank you for this insightful comment. We agree that uncertainty in snowfall forcing could influence the optimized redistribution parameters. In the revised Discussion, we now explicitly note that discrepancies between simulated and observed snow thickness may partly stem from uncertainties in the snowfall datasets. We further clarify that if the PWD–ERA5 average overestimates snowfall during certain events, stronger parameterized snow loss to leads could improve agreement with observed snow thickness while partly compensating for forcing biases rather than representing redistribution processes alone. We therefore interpret the optimized P and γ values as effective parameters that may compensate for multiple unresolved snow-loss processes and forcing uncertainties.

“While sublimation of suspended snow represents an important loss pathway in the Arctic (Liston et al., 2020, Robinson et al. 2025), **it is not explicitly represented in the present model configuration. As a result, the optimized P and γ values should be interpreted as effective parameters that may compensate for multiple unresolved snow-loss processes, including lead trapping, wind-driven redistribution, and blowing-snow sublimation, as well as possible biases in snowfall forcing.**”

- Line 365-366: It is unclear which experiment(s) these “improvements” are referring to. Please clarify. Additionally, is there an interpretation that can be provided on the results of snow melting too quickly in summer?

Response:

Thank you for pointing this out. We have clarified that the “improvements” refer to the optimized bulk redistribution and blowing-snow redistribution configurations. We also expanded the discussion of the rapid summer melt. In the revised text, we state that rapid summer melt may result from missing melt-pond physics, inaccurate snow thermal conductivity, and simplified snow density evolution, which can lead to excessive surface heat fluxes once the snow cover becomes thin.

“**Despite improvements in the optimized bulk redistribution and blowing-snow redistribution configurations, several limitations remain.** The simulated snow thickness still peaks approximately seven days later than observed and melts too quickly in summer. This discrepancy likely stems from uncertainties in snowfall datasets. For example, the PWD–ERA5 average shows increased snowfall between May 6 and 13 (Figure 1c), while observed snow thickness declined, suggesting potential errors in observations or reanalysis (Wagner et al., 2022). However, this discrepancy may also reflect unresolved snow-loss processes, including sublimation of drifting and blowing snow, non-local snow transport, and limitations in the model representation of snow compaction and density evolution. **Rapid summer melt may also result from missing melt-pond physics or inaccurate snow thermal conductivity, leading to excessive surface heat fluxes** (Yin et al., 2021).”

- Lines 367-370: This seems like an important consideration worth expanding on. What are the potential sources of biases in the PWD–ERA5 average, and would it tend to be biased high or low regarding snowfall frequency and magnitude? Additionally, can the description in Lines 369-370 be made more specific and clear in order to distinguish which findings were described by Wagner et al. (2022) and which are the authors’ hypothesis? This content would be best placed in a Discussion section.

Response:

Thank you for this helpful suggestion. We have expanded the discussion of uncertainty in the PWD–ERA5 snowfall forcing. In the revised text, we distinguish between the findings of Wagner et al. (2022), who reported uncertainty among MOSAiC snowfall estimates, and our interpretation of the May 6–13 event.

Specifically, the PWD–ERA5 average shows increased snowfall during this period, whereas the observed snow thickness declined. We now interpret this mismatch as potentially reflecting event-scale biases in the snowfall forcing, while also acknowledging that unresolved processes such as blowing-snow sublimation, non-local transport, and simplified snow compaction and density evolution may contribute.

"**This discrepancy may partly stem from uncertainties in snowfall forcing. Wagner et al. (2022) showed that MOSAiC snowfall estimates differ among products, and we used the PWD–ERA5 average to reduce product-dependent uncertainty. However, event-scale biases in snowfall timing, frequency, or magnitude may remain. For example, between May 6 and 13, the PWD–ERA5 average shows increased snowfall (Figure 1c), whereas the observed snow thickness declined. We interpret this mismatch as a possible overestimate of snowfall during this event. However, this discrepancy may also reflect unresolved snow-loss processes, including sublimation of drifting and blowing snow, non-local snow transport, and limitations in the model representation of snow compaction and density evolution.**"

- Line 376: What is meant by "all excess snow"?

Response:

Thank you for noting this unclear wording. We have replaced "all excess snow" with a more specific description. In the revised text, we clarify that Icepack does not explicitly represent sublimation of suspended snow in the present configuration; instead, the fraction of suspended snow that is not redeposited on the ice surface is passed directly to the ocean through leads.

"The treatment of suspended snow particles is simplified in Icepack: **the fraction of suspended snow that is passed directly to the ocean through leads**"

<u>Data Availability</u>:

- Line 391: It should be specified that this is the Alfred Wegener Institute Sea Ice Portal ("Sea Ice Portal" is otherwise unclear).
- Lines 391-392: The AMSR2 ASI data is missing its source; a URL should be included and/or a DOI should be cited. Citing the Spreen et al. (2008) study is not sufficient.

Response:

Thank you for these helpful comments. We have revised the Data Availability section to clarify that the autonomous sea ice measurements were obtained from the Alfred Wegener Institute Sea Ice Portal. We also added a specific source for the AMSR2 ASI sea ice concentration data from the University of Bremen Sea Ice Portal, including the data URL and the PANGAEA DOI for the AMSR2 ASI Arctic dataset, in addition to the algorithm citation of Spreen et al. (2008).

“The MOSAiC meteorological data are available from the Arctic Data Center (Cox et al., 2021a, 2021b, 2021c, 2023). SIMBA ice and snow thickness data are available from PANGAEA (Lei et al., 2021). Autonomous sea ice measurements (Ocean CTD Buoy and Flux Buoy) from 1 October 2019 to 1 September 2020 were obtained from t**he Alfred Wegener Institute Sea Ice Portal** (Grosfeld et al., 2016). AMSR2 ASI sea ice concentration data were obtained from the University of Bremen Sea Ice Portal **(Melsheimer and Spreen, 2019)** and are calculated following Spreen et al. (2008). SAR-based sea ice divergence data drift during MOSAiC 2019/2020 are available from PANGAEA (von Albedyll, 2024).”

Melsheimer, C. and Spreen, G.: AMSR2 ASI sea ice concentration data, Arctic, version 5.4 (NetCDF), PANGAEA, https://doi.org/10.1594/PANGAEA.898399, 2019.

References:

- The References list is out of order. Please order it alphabetically, following *The Cryosphere's* guidelines (https://www.the-cryosphere.net/submission.html#references).
- Several studies that have DOIs are missing their DOIs, and the DOIs are formatted inconsistently (with and without the URL prefix).
- Lines 482-486: This reference to Grosfeld et al. (2016) is missing the first half of the author list, including the first author.

Response:

Thank you for pointing out these issues. We have carefully revised the reference list according to The Cryosphere guidelines. The references have been reordered alphabetically, missing DOIs have been added where available, and DOI formatting has been made consistent throughout the list. We also corrected the Grosfeld et al. (2016) reference by adding the missing authors, including the first author.

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