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.

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.

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.

<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.

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?
- 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?
- 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.
- 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?
- There is a lot of information in a long paragraph form in this section. We wonder if it'd be 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).

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.

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 p. We understood that that is why the authors have tested a range from $p = 0.3$ 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.

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 $(\rho_s - \beta)$, where ρ_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.

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.?

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.

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 γ 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.

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.

Minor/typographical comments:

Abstract / Plain Language Summary:

- 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.

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.
- Line 37: This should be "on Antarctic first-year ice", not "in Antarctic first year ice".
- 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".
- 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.
- 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.
- 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).
- 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).
- 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.
- 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.
- 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.
- 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).
- Lines 76-77: The first part of this sentence is redundant with the previous paragraph.
- Line 83: Typo: "atmospheric".

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?
- 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.
- 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.

- 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…".
- Lines 119-120: Fix the grammar: "[seawater] temperature and salinity…".
- 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.
- Line 142: Condensation is also a process included in Icepack's snow mass budget processes.
- Line 151: "Represents" is too vague. Please mention explicitly that Icepack passes this fraction of snowfall to the ocean as an immediate snow flux.
- Throughout (e.g., Line 152): Please add equation numbers for each equation in the manuscript.
- 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.
- 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 $H_{\mathrm{s,sim}}$, not HS_sim. In-line equations like the one on Line 249 should also be formatted properly as equations.
- Line 160: Consider changing "After" to "When".
- Line 164: Typo: a period is missing before "The threshold".
- 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.

<u>Results</u>:

- 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 (<u>https://www.the-cryosphere.net/submission.html#math</u>).
    - 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 (<u>https://www.the-cryosphere.net/submission.html#english</u>).
    - Line 204: Fix grammar: change "none redistribution" to "no redistribution".

- 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.
- Line 210: Delete "snow" in "snow SWE" due to redundancy.
- Lines 216-235: This gives a nice qualitative description of the results. The section could be strengthened by including more quantitative results.
- Lines 224-225: This is a little unclear. Does this mean that the simulated snow thickness matches more closely with observations over time?
- Lines 227-228: It is important to clarify that this statement is only true until spring/summer melt.
- 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.
- Lines 239, 242, 252, 348, etc.: The variable "P" should be written as p for consistency with previous text and the Icepack documentation.
- Line 243: We suggest writing the period directly (1997–1998), rather than stating "two decades ago".
- 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.
- 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.
- Lines 249-254: Please address several issues:
    - 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.
- 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.
- 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, γ) 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.
- 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 γ represents an increase in the amount of suspended snow…" (or similar wording).

- 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.
- Lines 273-274: This sentence defining γ is redundant with previous text in the Methods (Lines 162-164) and should be deleted.
- Line 276: Figure 2d should be cited here, at the end of the preceding sentence.
- Line 283: Typo: "snow fall" should be "snowfall".
- Lines 282–287: It would make more sense to move these two sentences to the start of the following paragraph.
- 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.
- Line 287: Should this reference be "Figure 3b and 3c"? The 7–11 mm shaded band is shown on both panels of the plot.
- 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.
- Lines 298-308 (Figure 3 and caption):
    - 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 (γ, 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.
- 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.
- 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.
- Line 316: More accurately, the model scheme deposits "snow mass" into leads, not "snow particles".
- 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 γ parameters.
- 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.
- 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.
- Lines 329-335 (Figure 4 and caption):
    - Line 330: Change "snow changes" to "snow depth changes" or "snow thickness changes" for clarity.

    - Panel (a) legend and Lines 332-333: See comment above regarding formatting of the snow mass balance variables.

<u>Conclusions and Discussions</u>:

- 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.
- 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”.
- Line 340: Fix grammar: change “parameterization” to “parameterizations”.
- 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.
- 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?
- 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.
- Line 376: What is meant by “all excess snow”?

<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.

<u>References:</u>

- 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.

References:

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