Second-round co-review of:
"Effects of snow redistribution parameterization on simulated snow thickness validated by MOSAiC observations"

Revised manuscript submitted to *The Cryosphere* by Gu et al.

We thank the authors for responding to our previous review and addressing many of the points raised. The manuscript has been greatly improved. Our comments on the revised manuscript focus on unresolved concerns about preexisting text and typographical issues with newly added text. A list of references is included at the bottom.

We note that several passages of revised text provided in the Response to Reviewers document do not correspond precisely to the text in the revised manuscript, creating ambiguity as to which version of text the authors intended to be authoritative. Additionally, the Response to Reviewers refer to several revisions that were not actually made in the manuscript. In general, we encourage the authors to carefully proofread their next revision for typographical errors, grammar issues, and consistency with their Response to Reviewers.

Major concerns:

1. Throughout the manuscript, there is an incorrect use of SWE as a measure of snow thickness. SWE and snow thickness are not interchangeable. Please see the detailed comment below (on Lines 170-177) regarding why these two variables are distinct from one another. The study makes no direct comparison with snow thickness data. Consequently, the abstract, main text, and figure labels need to be revised accordingly from "snow thickness" to "SWE". Please also consider revising the use of "snow thickness" in the title for better accuracy.

2. The authors did not adequately address our previous concern regarding the unexpectedly low value for the threshold wind speed V^*. Please see our comment below regarding Lines 222-223, which highlights a fundamental mismatch between the origin of the V^* formulation and the Icepack snwITDrdg scheme. This discrepancy has significant implications for the study results and needs to be discussed.

3. The authors should place their results in context of the study by Hao et al. (2025), which also evaluates the Icepack snow redistribution parameterizations. Please see our comment below regarding Lines 521-533.

4. Please note that *The Cryosphere*'s guidelines on Manuscript Composition require "Conclusions" to be the final section, as is conventional for a paper. The authors have instead designated Section 4 as Conclusions and a new Section 5 as Discussions. Please switch the order of these sections and corresponding text, and consider also

moving current Lines 534-538 to the end of the new Conclusions section. “Discussions” should also be renamed to “Discussion”.

Comments in order of line number:

Abstract, Lines 18-19: Two comments:

- It is awkward to introduce the variable *P* in the Abstract without first defining it, and it is unnecessary. We would suggest changing this to: “… setting a ridged-to-level *ice* snow mass ratio of 5 in the bulk redistribution scheme…” (also adding “ice” for clarity).
- While we suggest omitting *P* at this location, we also note that the manuscript uses uppercase *P* and lowercase *p* inconsistently and interchangeably (e.g., Lines 313 vs. 316, Fig. 2 caption, Fig. 2a,b colorbar labels vs. Fig. 3b x-axis label, etc.). Please fix this by using one version consistently throughout the manuscript and figures.

Abstract, Line 20: It is incorrect syntax to use a parenthetical within a parenthetical (MAE). The sentence could be rephrased to avoid this.

Abstract, Line 21: The word “baseline” is not meaningful in the Abstract, as it is unclear what the “baseline” represents (a default Icepack configuration? a baseline that the authors selected?). The baseline parameter value is based on SHEBA observations, so that could be specified here.

Abstract, Line 23: The value of γ has units of kg m^{-2} in Lecomte et al. (2015), as mentioned on the authors’ Line 217. But the authors omit these units in the Abstract and at several other locations when specifying values of γ (e.g., Line 359, Figure 2 caption, Figure 2d axis label). Please correct this error throughout the text and figures.

Abstract, Line 26: Similar to Line 21, “the original blowing snow experiment” is not meaningful in the Abstract, as it is not explained what this experiment is. Replacing this with “the default parameter value of 1×10^{-5} kg m^{-2}” or simply “a default parameter value” would be more clear.

Introduction, Line 42: Please delete “with the ongoing thinning of sea ice (Kwok, 2018;” since this is no longer applicable.

Introduction, Line 46: Consider changing “snow thickness” to “snow processes” since this is more consistent with the topic of the study.

Introduction, Lines 48: Snowfall is not always the primary control of the observed large-scale distribution of snow thickness. Other factors like sea ice age, snow melt history (in the Arctic), and snow-ice formation (in the Antarctic) also exert leading-order control on the spatial distribution of snow depth on sea ice, including in coupled model simulations. Please correct this statement.

Introduction, Lines 51 and 76: Fix these references—“Lecomte et al., 2015”, not “Olivier Lecomte et al., 2015” (referenced correctly elsewhere).

Introduction, Line 56: The plural “studies” is used but only one study (Schmidt, 1982) is cited. Please fix this inconsistency.

Introduction, Line 59: The word “polar” should be omitted before “sea ice”. These processes presumably affect sea ice in all regions, including those that are non-polar (e.g., Gulf of Finland, Hudson Bay).

Introduction, Line 62: The plural “studies [during MOSAiC]” is inaccurate, as only Robinson et al. (2025) assessed blowing snow during MOSAiC, not Robinson et al. (2026). Move “during MOSAiC” to later in the sentence: “… was active over Arctic sea ice during MOSAiC”.

Introduction, Lines 67-68: The phrase “under the observed conditions” is unclear and potentially misleading, given the earlier broader mention of “in the Southern Ocean”. Consider being specific: “…, based on conditions observed during a Bellingshausen Sea field experiment”.

Introduction, Line 74: This sentence is missing a period at the end.

Introduction, Lines 77-79: The Response to Reviewers acknowledges the concern we raised in our previous review and mentions relevant revisions, but no revisions were actually made in the manuscript (or the response document). Please address the following issue, copied from our previous review:

> *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).*

Introduction, Line 80: Consider changing “parameterization” to “parameterizations” for accuracy.

Introduction, Lines 82-83: Writing that “we used Icepack 1.3.2 to maintain consistency with the model configuration used in this study” makes no sense as an explanation for using a four-year-old version of Icepack instead of a newer version. The logic regarding “consistency” is entirely circular. If there is not a scientific reason to use the older version, the authors could simply state, “We note that newer versions of Icepack have since been released” and mention whether those newer versions contain significant updates to the snow redistribution parameterizations. We highlighted this issue in our previous review and it has not been sufficiently addressed.

Introduction, Lines 89-90: “… highlighting the importance … for simulated sea ice thickness” is quite vague. Can the authors be more specific about what Zhao et al. (2025) found, especially

since that previously-published study is similar to the present manuscript in its objectives and conceptual design? We requested this in our previous review and it has only been partly addressed with the additions from Lines 521-533, which focus on differences between the two studies but not the specific findings of Zhao et al. (2025).

Introduction, Lines 90-92: It is unclear if this sentence is referring to the Zhao et al. (2025) study or the state of knowledge in general. Please clarify.

Introduction, Line 98: This list does not include the newly added Section 5 and needs to be updated. However, please note our comment above in “Major concerns” regarding the order of the Discussion and Conclusions sections.

Data and Methods, Line 105: Fix typo—“colum” should be “column”.

Data and Methods, new Table 1 (Line 123):

- Change “ICEPACK” to “Icepack” for consistency.
- In the Table 1 header, consider changing “ICEPACK” to something more descriptive, like “Model setting”.
- Change “mean observe” to “mean observed” and change “thermodynamic” to “thermodynamics” to fix grammar.
- “Melt ponds” are not the same as “superimposed ice”; please clarify this parenthetical.
- For consistency, specify all units in the second column (e.g., “3600 s”), not the first column (this format is switched in the last two rows).

Data and Methods, Line 141: As we requested previously, please clarify this procedure using specific terminology that can allow reproducibility of the methods. “Combined” is still not clear. Were the 10-m and 3-m time series interpolated to uniform time points and then averaged, or were they interwoven at their original time resolutions, which are presumably different given the different instrument types? If the time series were interwoven, how was the spatial averaging done?

Data and Methods, Line 149: What is the value for c_w? If adding it to Table 1, consider using both the variable name and description for clarity; otherwise it can be specified here.

Data and Methods, Line 161: Please give the range reported by Wagner et al. (2022) here to help readers understand the data.

Data and Methods, Line 164: Typo—change “The Icepack” to “Icepack”.

Major concern — Data and Methods, Lines 170-177: We thank the authors for their response to our concern regarding SWE and snow thickness. However, the explanation for excluding snow thickness as a variable for the analysis lacks sound reasoning and justification.

Reasoning: The motivation of the study is described in the Introduction. The Introduction explains that snow thickness is an important variable to study because of its role in shielding the

sea ice surface from sunlight, thereby reducing surface melt, and in insulating the sea ice from the cooler atmosphere, thereby reducing sea ice growth rates. These interactions are dependent on snow thickness. Snow water equivalent tells us less information about these important aspects of snow and its interactions with sea ice.

Justification: The authors argue that because snow density varies spatially, SWE is a more physically consistent basis for comparisons than snow thickness. In this regard, it is important to consider how SWE was estimated from the MOSAiC results. There are several steps taken to estimate SWE. First, a statistical model is used to estimate snow density from Snow MicroPen (SMP) data (Eq. 3 in Wagner et al., 2022). The reported root-mean-square-error for modeled snow density is 41 kg/m^3. The uncertainty of SMP-derived densities is larger for wind slab and depth hoar layers (see King et al., 2020), which spatially vary. Next, Wagner et al. (2022) applied the (derived) snow densities and (measured) snow thicknesses during SMP data collection to derive a formula:

$$\mathrm{SWE} = 323.97 \cdot H_s^{1.07}$$

where H_s is snow thickness. Finally, snow thickness measurements collected using a Magnaprobe from other areas of the field site were plugged into the equation to estimate SWE. The snow thicknesses used to compute SWE in this formula do not correspond to the same location or time as the SMP measurements, from which snow density was derived. There is a disconnect in time and space between all datasets that go into the SWE estimate. Accordingly, SWE is not a more physically consistent variable than direct snow thickness measurements if spatial variability and uncertainties are being considered. Converting the SWE uncertainty to equivalent snow thickness, the uncertainty is 4x larger than the uncertainty from direct snow thickness measurements. Since snow thickness is a readily available variable in Icepack, it would make sense to additionally compare with an observed variable that has a high accuracy.

Recommendation: To address this concern, we suggest adding a short paragraph on snow thickness results. This will help place the study's finding in the context of snow on sea ice and be more relatable to the broader snow on sea ice community. Relatedly, the y-axis labels and captions of all figures need revising to differentiate that the variable being shown is SWE and not snow thickness since these variables are not interchangeable.

Data and Methods, Line 173: Fix grammar by changing "observed snow thickness were" to "observed snow thicknesses were" or "observed snow thickness was".

Data and Methods, Table 2 (Lines 187-189): The Response to Reviewers quotes new text stating that "L1-L3 are… located within 15 kilometers of the Central Observatory" and "M6 is… located 10 kilometers from the Central Observatory", while the manuscript version has inconsistent language: "a few kilometers" and "tens of kilometers". Please clarify which version is correct.

Data and Methods, Lines 192 and 199: In Line 192, please change "sublimation" to "surface sublimation" for clarity. Then, in Line 199, change "sublimation" to "blowing snow sublimation". Static surface sublimation is represented in Icepack (though blowing snow sublimation is not),

so the existing statement at Line 199 is inconsistent with the following sentence (“unresolved processes”) and the first sentence of the paragraph.

Data and Methods, Lines 196: Please revise “snow” to “snow thickness”.

Data and Methods, Line 208: As we mentioned in our previous review, the default value of p needs to be stated clearly in this section. The authors introduce results using this default configuration in Figure 1 but the default p value is only specified much later, in Line 316. The Response to Reviewers (regarding original Lines 153-154) acknowledges this and promises a revision, but for some reason, no change was made to the text. Please fix this by adding to the end of the sentence: “... relative to level ice (p = 0.3 by default)”.

Data and Methods, Lines 211, 215, and elsewhere: As we mentioned previously, please use a consistent notation for the variable σ_{ITD} that properly formats “ITD” using subscript (as in, e.g., Line 233). This notation remains inconsistent throughout the text, despite the Response to Reviewers stating that this has been revised as suggested.

Major concern — Data and Methods, Lines 222-223: The authors have not satisfactorily addressed the concern we raised in our previous review regarding the very low model value of V^* (3.5 m s^{-1}) relative to the commonly-understood threshold for wind-blown snow (7–8 m s^{-1}; Li & Pomeroy, 1997), which is well-supported in observations from MOSAiC by Ranjithkumar et al. (2025). The authors argue in their Response to Reviewers that V^* is “not the general threshold wind speed for the onset of blowing snow” but rather a separate compaction threshold for the effective density ϱ_{s}. This distinction does not make sense. Compaction and densification of the surface snow layer occur because of the fragmentation, abrasion, and sublimation of recently deposited snow grains *during blowing snow events*, after which the snow grains can be packed more tightly. This can admittedly happen at slower wind speeds that support rolling or saltation of snow grains, but not full suspension. However, at wind speeds as slow as 3.5 m s^{-1}, experimental evidence shows that negligible snow densification occurs (e.g., Walter et al., 2024; see their Figs. 8 and 10). Thus, 3.5 m s^{-1} remains implausibly low as a compaction threshold, especially for attaining densities of 330 kg m^{-3} (note that fresh snow can be deposited initially at densities as low as ~100 kg m^{-3}).

The most likely reason for this is a fundamental mismatch between the Lecomte et al. (2013) empirical formulation and how Icepack and this study have implemented V^*. Lecomte et al. assume this is a *“mean seasonal surface wind speed”*, which is an entirely different metric than the instantaneous or hourly wind speed in the model. This probably explains why 3.5 m s^{-1} is unrealistically small: seasonal averages are lower than instantaneous wind speeds. This discrepancy with observations and its origin in the formulation of V^* should be explained and discussed as a potentially significant limitation of Icepack’s snwITDrdg parameterization, along with the consequences for the study’s results. For example, based on Fig. 1b, how much less frequently would snow erosion occur if a more realistic instantaneous V^* value (e.g., 7–8 m s^{-1}) were used instead of 3.5 m s^{-1}? How would the magnitude of snow loss and the study’s optimization results for γ be affected, at least qualitatively?

Results, Lines 263-264. “All snow thickness values are converted to SWE for consistency with observations”. As noted above, the observations in their native unit is snow thickness. Deriving SWE units from the observed snow thickness leads to greater uncertainty and thus is less consistent.

Figure 1 (Lines 270-279): For panel (d), the y-axis label and corresponding figure caption should simply be “SWE (mm)” since snow depth is not shown.

Results, Lines 282-283: Fix the confusing wording and incorrect grammar by amending to: ”... peaks at 155 mm on May 13, corresponding to an overestimation of about 80 mm SWE compared to observations.”

Results, Lines 286, 289, and 327: Does “before June” mean “for the period ending on May 31”? If so, it would be more clear to write “through May” rather than “before June”. Another reason this suggested wording is more clear is that “before June” implies an instantaneous absolute error on May 31, rather than a metric evaluated over several months.

Results, Line 291: A semicolon is missing before “Figure 1b”.

Results, Line 323: Change “increased” to “varied” for clarity. Stating that “we increased p from 0.3 to 4.0” implies that just two values of p, 0.3 and 4.0, were tested, rather than varying p within that range.

Results, Line 326: Saying that “the fraction of snowfall passed to leads is $a_{lvl}\ p\ /\ (1+p)$” restates Eq. 1 and is not informative. It should be stated what value is used for a_{lvl} in these simulations, and that calculated quantity could then be provided for p from 0.3 to 4.0.

Results, Line 329: Fix typo — “... *are* the simulated and observed SWE…”

Figure 2 (Lines 340-348):

- Please fix the incorrect definition of p, which is not “the ratio of snow depth on ridges to that on level ice”. In response to our previous review, the authors fixed this error at other locations, but the new text added here repeats the prior mistake regarding this definition.
- For panels (a) and (c), the y-axis labels and corresponding figure captions should simply be “SWE (mm)” since snow depth/thickness is not shown.
- On Line 344, change “show” to “for” to correct the grammar in this sentence.

Results, Lines 355-358: As we mentioned in our previous review, it would be worthwhile to discuss deficiencies of these physics. For example, during melt, snow is not redistributed by the wind—it is effectively fixed in place. So, while sea ice concentration can decrease in melting conditions, allowing more snow loss to occur in the model, this does not happen in reality. In the Response to Reviewers, the authors said they added a discussion in the revised manuscript about this point, but no content was added to the revised manuscript.

Results, Lines 360 and 378: Change “snow thickness” to “SWE” at these locations.

Figure 3 (Lines 385-397):

- In panel (a), gray shading is used to represent the standard deviation of AMSR2 sea ice concentration, but the figure caption and manuscript text do not explain how this was calculated. What geographic area does this represent?
- For panels (b) and (c), the secondary y-axis labels and corresponding figure captions should simply be “SWE MAE (mm)” since snow depth is not shown.
- The added text on Lines 396-397 (“a_{ice} denotes AMSR2 sea ice concentration”) is incorrect, as the variable a_{ice} represents both simulated and prescribed sea ice concentration in the study (including in the Fig. 3b legend).
- The caption is missing a period at the end of the final sentence.

Results, Line 414: Similar to on Line 56, the plural “studies” is used but only one study (Gu et al., 2022) is cited. Please fix this inconsistency.

Results, Lines 418 and 420: For clarity, change “the bulk scheme” to “the default bulk configuration” and change “snwITDrdg” to “the default snwITDrdg configuration”. The existing wording is confusing because, in each case, the default and optimized *configurations* (referring to different parameter settings) use the same snow-redistribution *scheme* (referring to the underlying parameterization).

Results, Line 424: Similar to above, please change “within the snwITDrdg and snwITDrdgm scheme, prescribing…” to a less confusing wording: “in the default snwITDrdg and optimized snwITDrdgm configurations, prescribing…”.

Figure 4 (Lines 433-439):

- The y-axis labels and figure captions should simply be “SWE change (mm)” since snow depth is not shown.
- In the caption (Line 435), change “bulk” to “default bulk” and “snwITDrdg” to “default snwITDrdg” for clarity.

Conclusions, Lines 442-446: “Using the Icepack… agreement with observations.” This sentence is still unclear. Please consider rewording to:

> *Using the Icepack column model, we assessed the effects of two snow redistribution parameterizations and a no-redistribution configuration on simulated snow thickness and SWE. We compared the model output to MOSAiC buoy observations.*

Conclusions, Line 470: Please consider adding a concluding sentence that is forward-looking. For example, what are the directions that future research on this topic should take, based on the new information gained from this study?

Discussion, Lines 475. The revision is a major improvement regarding the May 6-13 event. There are other plausible scenarios that could explain the discrepancy between decreasing

snow depth and snowfall events. Around 12 May 2020, a major storm traveled through the MOSAiC domain, which greatly deformed the sea ice cover in the Central Observatory and broader area. In severely windy conditions, snow could have eroded from the buoy locations during the storm, as the authors mention. Another possibility is that the ice in which the buoys were deployed became deformed and erroneously affected the buoy sensors. Therefore, we suggest adding this possibility in the text to improve interpretation:

> *This discrepancy may partly stem from uncertainties in snowfall forcing and buoy observations.*

Discussion, Lines 490-491: This sentence is confusing because the grammar is incorrect and the wording is inaccurate, suggesting that all snow enters the ocean after being lofted by the wind. Please amend the sentence to:

> *The treatment of suspended snow particles is simplified in Icepack: a fraction of suspended snow is passed instantaneously to the ocean through leads.*

Discussion, Line 498: Similar to on Lines 56 and 414, the plural “studies” is used but only one study (Clemens-Sewall et al., 2022) is cited. Please fix this inconsistency.

Major concern — Discussion, Lines 521-533: We appreciate the authors’ addition of text placing their findings in context of Zhao et al. (2025), though more discussion is needed in the Introduction (see comment above regarding Lines 89-90). Separately, we now realize that the Hao et al. (2025) study also has significant conceptual overlap with this manuscript, despite their evaluation of the Icepack snow redistribution schemes in an Antarctic landfast sea ice context. It is puzzling that the authors have not discussed those previously published findings, given the very limited amount of related work examining snow redistribution in Icepack (which we believe is comprised of only this manuscript, the Zhao et al. study, and the Hao et al. study). We encourage the authors to add discussion of Hao et al. (2025) after this paragraph in the Discussion section. For example, Hao et al. likewise found that the snwITDrdg scheme appears to perform better than the bulk redistribution scheme. In what other ways can these results be compared and contrasted to those of Hao et al.?

Discussion, Line 538: We suggest changing “Arctic snow conditions” to “snow conditions on Arctic sea ice”. As mentioned previously, this wording is useful to differentiate between terrestrial snow and snow on sea ice.

Acknowledgments, Line 566: Delete the extra space within the URL.

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