Review response on

“From snow accumulation to snow depth distributions by quantifying meteoric ice fractions in the Weddell Sea” by Stefanie Arndt et al.

Anonymous Referee #1

Received and published: 16 January 2024

Arndt et al provide a very valuable and interesting data set and this definitely merit publication in TC. Overall the paper is fairly well written, however, while attempting for brevity (which is a plus), there are however lack of clarity that would need to be improved by providing some more detail throughout the manuscript, and some critical self-evaluation of the fundamental assumption that are made. I think the required revisions are largely technical and minor (although numerous), and thus constitute minor revisions before acceptance.

While I give only a few more generic comments here, the authors are provided detailed comments and suggestions in the annotated manuscript attached to this review. I would expect a point by point response also to those comments with the revised manuscript.

We extend our sincere appreciation to the reviewer for their thorough and insightful review of our manuscript. Their expertise in the field is evident from the comprehensive comments and suggestions provided. We are particularly grateful for their attention to the lack of clarity regarding the usage of the two models and the need for elaboration on uncertainties related to the assumption of immediate flooding and snow ice formation in response to negative freeboard. In response to these valuable insights, we have included additional sentences and explanations in the manuscript, as described in detail in the corresponding comments below. We believe that these enhancements significantly improve the clarity and robustness of our study. Once again, we thank the reviewer for their excellent work and contributions to the refinement of our manuscript.

In addition to both reviews, we conducted a re-assessment of all SNOWPACK model results due to the following reason: Upon re-evaluating the model code for running the SNOWPACK simulations, we identified an error in writing out the ERA5 reanalysis data to a format suitable for the SNOWPACK model. In an earlier version, the surface fluxes were provided in 6-hour intervals, requiring division by 6 to align with the hourly input data. However, in the current version of retrieved ERA5 data, all parameters, including surface fluxes, were available in hourly steps. Despite this, they were still divided by 6 when producing the meteorological input for the model. This error has been rectified, and all simulations were re-run using the corrected meteorological forcing data.

Generic comments:

Use of sea ice and snow ice in the text - I would suggest to follow the following practice: Sea ice as noun = sea ice. Sea ice as adjective (modifier of noun) = sea-ice. Two examples: Climate warming has affected the extent of sea ice. Climate warming has affected the sea-ice extent. Using "snow-ice" throughout, would IMHO improve the readability.

Thank you for bringing this to our attention. Ultimately, the journal determines how they address spelling conventions. However, based on my experience, hyphens are typically omitted during typesetting.

Introduction/Discussion/Summary: I suggest that the authors give a bit more quantitative results from previous work on the importance and contributions of snow-ice and superimposed ice to Antarctic sea ice mass balance (some of this literature is noted in the annotated ms.), this would also place the current work better in context of existing work by Lange, Jeffries, Maksym, Ackley etc. It could be also helpful to note on how WS compares to other sectors of the Southern Ocean to place the results in broader context.

We appreciate your insightful observation regarding the need for better contextualization of our results within the existing research conducted in the Weddell Sea and other Antarctic sea ice regions. In response, we have added relevant paragraphs to the introduction to provide a more comprehensive overview of the research landscape in these areas. Thank you for highlighting this important aspect.

Throughout (abstract, discussion, summary), the assumption on the fact that negative freeboard will always cause flooding when using the simple sea-ice model is fundamental to all the results presented, and the caveats of this assumption should be clearly noted and discussed. There are even studies that looked into this e.g. Ackley et al. 2020, Wever et al., 2021, and I would feel this needs to be better assessed.

We appreciate your feedback on the need for elaboration on uncertainties related to the assumption of immediate flooding and snow ice formation in response to negative freeboard. In response to your valuable input, we have revised the relevant parts of the manuscript to emphasize "potential snow ice formation" to underscore this uncertainty, as further described in detail in the corresponding comments below.

It would also be good with an assessment what the pros and cons are of different type of buoys used for such studies, if flooding could be directly detected with e.g. a thermistor-string type buoy (like Ackley et al., 2020).

We acknowledge the outlined need to assess other measurement techniques for snow and superimposed ice formation. However, delving into such assessments would require extensive discussions of the respective possibilities and limitations, which are beyond the scope of this manuscript. Instead, we appreciate the suggestion and have included a note in the summary and conclusions section highlighting the importance of addressing this aspect in future research endeavors.

I also struggled what the purpose was to use two models, if I understand correctly the SNOWPACK model would provide both snow-ice, superimposed ice and snow depth in one go, what is the purpose of using the very simple thermodynamic model? There is neither much (enough) discussion on how the two models compare in e.g. producing flooding (snow-ice) and why there are differences. Maybe this is simply a case to elaborate a bit better on the justification on how the two models complement each other.

We appreciate your feedback on the clarity of the manuscript. The decision to utilize two different models stems from the study's objective, which aims to accurately depict snow ice formation along buoy trajectories while minimizing sources of uncertainty. The primary use of the 1D model is rooted in its straightforward approach, aligning well with this objective. Additionally, incorporating SNOWPACK offers valuable insights into the processes involved, highlighting the benefits of including a snow model.

However, as highlighted in the work of Wever et al., several aspects require further elucidation. Therefore, we chose to prioritize the use of the 1D model for flooding/snow ice formation calculations due to its simplicity and direct applicability to our study objectives. Nevertheless, ongoing efforts are directed towards enhancing SNOWPACK to overcome the limitations associated with the simple 1D model approach.

I would also have liked to see the ice thickness results, now these are not shown at all, although one key factor triggering flooding is that the ice is thin enough. As such it would be good to also show evolution of ice thickness alongside the snow-related parameters. Here the assumption of constant vs. variable ocean heat flux could highlighted in the Summary. I also note that Ackley et al (2017) summarize ocean heat flux observations, and for WS they appear much larger than what is used in this study (at least for parts of the year).

We appreciate the reviewer's insightful comment regarding the absence of sea ice thickness results in our manuscript. We acknowledge that sea ice thickness is a key factor in triggering flooding events, and its inclusion alongside snow-related parameters could provide additional context to our study.

However, we chose not to include sea ice thickness results for several reasons. Firstly, our focus in this study was primarily on evaluating the accuracy of snow thickness measurements and their relationship to flooding events. The comparison with actual measurements of snow thicknesses yielded promising results, underscoring the high confidence in the chosen parameters.

Secondly, while sea ice thickness is indeed important, our study aimed to investigate the specific impact of snow-related parameters on flooding events. Including sea ice thickness results would have extended the scope of the study beyond its intended focus.

Regarding the assumption of constant versus variable ocean heat flux, we agree that this is an important consideration. As for the observation by Ackley et al. (2017) regarding ocean heat flux in the Weddell Sea, we acknowledge the variability in these fluxes throughout the year. Our study utilized specific values for ocean heat flux based on available data and modeling approaches, which may differ from those reported by Ackley et al. We appreciate the reviewer's suggestion and have addressed this discrepancy in the Summary section of the manuscript.

Overall, while we recognize the importance of sea ice thickness in relation to flooding events, we believe that the focus on snow-related parameters aligns with the specific objectives of our study. We thank the reviewer for their valuable feedback and suggestions, which will be duly considered in future research endeavors.

Data availability: Reference to ice core data are given in several places, but I do not see this data is listed with the data sets?

The ice core data are not an explicit outcome of this study; instead, they are utilized for evaluation purposes based on the referenced studies. Therefore, the data are not explicitly cited in this manuscript.

Detailed comments

Since detailed comments and suggestions are provided in the annotated manuscript, we will only reference the initial line numbers here. Language corrections suggested in the manuscript have been implemented (visible in the tracked change document) but are not explicitly mentioned here.

L.3/title — This is a question of “style”, where we would like to stick to the initial “meteoric ice fractions” instead of “meteoric ice contribution”.

L.7 — To make clear, that this is related to the whole Antarctic sea ice cover, we adjusted the sentence accordingly:

A year-round snow cover is a characteristic of the entire Antarctic sea ice cover, […]

L.14+L.15 +L.16 — Thank you for addressing the confusion regarding the reference of the 44%: The 44% was originally referenced to the number of Snow Buoys experiencing snow ice formation. However, it would be more informative, as you mentioned as well, to reference the percentage of the total track length, which calculates to 41%.

Additionally, you are correct in noting that the calculated snow ice formation represents only a "potential layer thickness." Moreover, we have included the notation of the given layer thickness as an “average maximum,” as well as a reference to the “total” snow accumulation.

We have revised the sentence according all the mentioned points above:

The results show that potential snow ice formation, with an average maximum thickness of 35 cm, was detected along 41% of the total track length of the analyzed Snow Buoy tracks, which corresponds to about one quarter of the total snow accumulation.

L.16 We have acknowledged that the phrase "to simulate the snow accumulation" is confusing, as SNOWPACK primarily simulates the temporal evolution of internal snow properties. Therefore, we have revised the sentence accordingly:

In addition, we simulate the evolution of internal snow properties along the drift trajectories with the more complex SNOWPACK model, which results in superimposed ice thicknesses between 2 and 9 cm.

L.31 Indeed! We appreciate you bringing this obvious mistake to my attention!

Snow ice results from the submergence of ice beneath the seawater level, […]

L.26-36 We acknowledge the reviewer's valid point regarding the omission of detailed discussions on snow ice and superimposed ice thickness layers around Antarctica, which are essential for contextualizing the results presented in the paper. As such, we have incorporated the following paragraph into the manuscript to address this oversight:

As such, Arndt et al. (2021) found mean snow ice thicknesses of 0.18 ± 0.18 m in the southeastern Weddell Sea and 0.22 ± 0.22 m in the northwestern Weddell Sea during late summer. In the Ross Sea, Kawamura et al. (1997) observed similar ratios of snow ice to total sea ice thickness, with 22% for first-year ice and 10% for multi-year ice. Conversely, Ackley et al. (2020) reported a snow ice thicknesses of 30 to 35 cm in the Amundsen Sea, attributed to intense bottom melt driven by high ocean heat fluxes and low ice concentration. For the fresh layer of superimposed ice, Haas et al. (2001) documented varying thicknesses, with 0.08 ± 0.06 m in the northwestern Weddell Sea, 0.10 ± 0.05 m in the Amundsen Sea, and 0.12 ± 0.07 cm in the Bellingshausen Sea.

L.49 We totally agree and rephrased the sentence accordingly:

Snow accumulation refers to all the snow that has fallen or has been redistributed by wind at a certain location during a specified period of time.

L.51 Yes, the processes you mentioned, such as redistribution, compaction, or snow loss in leads, do impact the actual snow depth. However, these changes are effectively captured by the measurements of the Snow Buoy, as the system tracks any height changes independent of their cause by measuring the distance to the snow surface.

L.52 We agree and rephrased that part of the sentence accordingly:

Measurements with an automated snow depth probe (MagnaProbe), for example, […]

L.53 We acknowledge that the term "larger spatial scales" may be misleading since the measurement is still a point measurement while drifting through the ocean. We therefore rephrased the sentence accordingly:

To extend these point measurements, in particular, to larger temporal scales, Snow Buoys have been developed.

L.65 If regular Snow Buoy deployments were conducted in other sea ice-covered regions, the same analysis could be performed there. However, unfortunately, this is not the case. Therefore, while the results could potentially be adapted to other regions, there would be a certain level of uncertainty associated with this adaptation.

L.74 We acknowledge the importance of providing information on the number of buoys experiencing snow ice formation. However, we believe that the figure caption may not be the most suitable place for such detailed content. Instead, this information is provided in Section 3.3 "Seasonal transition of snow into snow ice" (initial manuscript, L.227).

L.75-76/ L.88-94 — We completely agree that consistency would enhance clarity in this context. Therefore, we have rephrased the section accordingly to maintain consistency regarding the respective locations (SW, NW, …).

Section 2.2 — Thank you for your additional insights. However, it's worth noting that the proposed table and graph on the initial conditions of each buoy closely resemble those given in the referenced publication by Nicolaus et al. (2021). Additionally, from our perspective, the relevance of the annual snow accumulation and snow ice formation rates per Snow Buoy may not be as significant as the spatial statistics. We therefore prefer to stick to the figures provided, in particular Figure 2, which effectively summarizes these findings on a spatial scale.

L.109-111 — Thank you for highlighting the need for further clarification regarding the limitations of the approach utilized. We acknowledge that this addition will enhance clarity for the reader, which is why we have included a dedicated paragraph addressing these limitations along with additional references:

Previous studies by Wever et al. (2021) have demonstrated that such an assumption tends to overestimate actual flooding and snow ice formation on a floe-scale, primarily due to the significant spatial heterogeneity of snow and ice thickness. To address and assess this limitation, the results of the model runs are evaluated alongside in-situ observations (refer to section 2.5).

L.114/ L.137 — We acknowledge the importance of elucidating our selection criteria for ocean heat fluxes within the study. Therefore, we have included a paragraph giving literature values within the Weddell Sea, emphasizing that the studied buoy tracks predominantly reside in inner pack ice zones characterized by lower ocean heat fluxes:

Reported values for ocean heat fluxes in the Weddell Sea vary, ranging from 2-7 Wm^2 (Lytle and Ackley, 1996; Robertson et al., 1995) in the western part to well above 20 Wm^2 (McPhee et al., 1999) in the central part. Considering that the buoys primarily drift in the inner pack ice, the ocean heat flux for the simulations in this study is set toward the lower end of the range, prescribed with a constant flux of 3 Wm^2.

L.115-116 — We agree on the importance of mentioning the parameters that were actually used. As such, we have revised the manuscript to include a sentence addressing this.:

Specifically, snow densities of 340 kg m^{-3} and 264 kg m^{-3}, along with snow thermal conductivities of 0.28 $Wm^{-1}K^{-1}$ and 0.17 $Wm^{-1}K^{-1}$ for perennial and seasonal sea ice, respectively, are applied.

Section 2.4 — Indeed, both models operate independently. The rationale behind this decision to mainly utilize the 1D model for the calculations is primarily rooted in the study's objective, which is to accurately depict snow ice formation along buoy trajectories while minimizing sources of uncertainty. The additional incorporation of SNOWPACK serves to illustrate the additional benefits and insights into process understanding achievable by including a snow model. Nevertheless, as evidenced by the work of Wever et al., there remain numerous aspects requiring further elucidation. Consequently, we opted to prioritize the straightforward 1D model for flooding/snow ice formation calculations. Nonetheless, ongoing efforts are dedicated to enhancing SNOWPACK, aiming to surpass the limitations of the simple 1D model approach.

L.126, L.129, L.132 — We agree that in general more detailed background knowledge on the SNOWPACK model runs helps to evaluate the results. We therefore added more details and background on the model and rephrased the whole paragraph accordingly:

For our simulations, we initialize the model with the initial snow and ice thicknesses as measured during buoy deployment. The initial snow and ice temperature profile is determined from the ERA5 surface temperature and a water temperature of 271.35 K, assuming that the snow and ice column is in thermal equilibrium. For the remaining properties, we follow the approach of

Wever et al. (2021): For the snow layers, we assume an initial density of 275 kg m−3, corresponding to a volumetric ice content of 0.3 m3 m−3 and a volumetric air content of 0.7 m3 m−3, and set the grain radius to 0.15 mm, the bond radius to 0.09 mm and sphericity and dendricity to 0. For the ice layers above sea level, we assume the volumetric ice and air content to be 0.95 m3 m−3 and 0.05 m3 m−3, respectively. Ice layers below sea level are assumed to contain also a fraction of water, calculated from the layers' temperature and an assumed bulk salinity of 1.75 g kg−1.

Following the approach of Wever et al. (2021), we use the Richards equation, combined with the transport equation for salinity to solve liquid water and brine distributions. For two out of the 36 Snow Buoys (2017S49 and 2019S88) the simulations are aborted due to numerical instability and we use a more simple bucket-type approach instead of the Richards equation. In contrast to Wever et al. (2021), we choose an atmospheric stability following Holtslag and De Bruin (1988), as in the SNOWPACK model documentation this was changed to be the new default setting (the effect on the snow depth is minor). Like in the simple thermodynamic sea ice model described above, the ocean heat flux is set to a constant value of 3 Wm-2 and the atmospheric forcing is based on ERA5 reanalysis data, specifically surface heat fluxes, humidity, and surface wind.

Here, we present snow-height-driven simulations, which means that the SNOWPACK simulations are forced to closely follow the snow height evolution as measured by the Snow Buoy. While snow accumulation as indicated in the Snow Buoy datasets will lead to an instant increase in the SNOWPACK simulation (like a precipitation event), sudden reductions in snow height will only be incorporated within the scope of the model physics. Wind-induced transport of snow is neglected.

L.140 There appears to be a misunderstanding: The model results are indeed validated using data from the specific location where the buoy is drifting, rather than relying on the ice regime of the initial deployment location. Therefore, for instance, model results derived from buoys deployed in the southeastern Weddell Sea and drifting towards the northwestern Weddell Sea were evaluated using field data from the northwestern Weddell Sea.

L.144-147 Yes, freeboard and flooding were indeed mapped during the expeditions; however, it was not conducted in a structured manner, which is why it is not explicitly mentioned here.

Figure 2 Regarding the little numbers in panel A vs. the little dots in panel B/C: As mentioned in the caption the "Numbers indicate the amount of annual buoy cycles contributing to the mean value.", whereas the "Small dots represent individual point calculations for the specific month and region". With these explanations, the differentiation between both is from our perspective sufficient. However, we do agree that it looks like panel B and C have different amounts of small dots, which is not the case; it's just a larger spread in panel C compared to panel B. Therefore, a sentence was added to clarify that:

Considering the transparency of the individual small dots, the higher the transparency, the fewer points are overlayed here, whereas the lower the transparency, the more points are overlayed.

Figure 2A We agree and specified the unit towards *"… in cm per month"*.

L.183-184 We agree that providing only the link to the monthly rates for the annual rates can be confusing. Therefore, we have added a reference to Figure 1B for clarity.

L.188-190 We have clarified in the Introduction that the term ablation includes melting processes, so we are not adding the specific melting here again to reduce confusion.

L.196-197 To prevent confusion, we have decided not to mark the position of the iceberg on the map, as it began moving several years ago and has since drifted out of the Weddell Sea.

L.188-190 We agree that providing a definition of the term "snow ablation" would be beneficial for the reader. Therefore, we have added a sentence in the introduction section accordingly:

In contrast, snow ablation refers to the process of snow (height) loss due to factors such as melting, metamorphism, sublimation, or other forms of erosion.

L.200-203 We don't agree that we need to mention for every result analysis again the number of buoys going into the respective calculation as these numbers are given in Figure 2A. However, we agree that the respective reference to the figure is missing, which we added now.

In contrast, significant snow depth reduction or melt is mainly observed during the summer months in the Marginal Ice Zone (MIZ), with monthly net snow mass loss/melt occurring exclusively in December and January (Figure 2A).

L.203-204 The "low ablation period" is linked to the ablation rates observed in the other regions. To enhance clarity, the sentence has been adjusted accordingly:

The C/SE region, however, exhibits a relatively low ablation period compared to other regions between May and August, with an average loss of up to 3 cm / month.

L.206-208 We are not aware of recent studies specifically addressing the length of the snowmelt season in the Southern Ocean. However, this topic presents an intriguing question worthy of investigation.

L.223-224 We agree and moved the whole paragraph (initial manuscript L.222-226) to Section 2.2. on the Snow Buoy description.

L.227-232 We agree that it is important to reiterate the potential for overestimations in the approach. Consequently, we have added a sentence accordingly at the end of the first paragraph in the section:

For the subsequent analysis, it is important to note that the snow ice thickness may be overestimated, as negative freeboards are directly translated into flooding and subsequent snow ice formation, which may not always be the case (see, e.g., Wever et al. (2021)).

L.227 As already mentioned for the revisions of the abstract, you are totally right: The 44% (46% was a spelling-mistake!) was originally referenced to the number of Snow Buoys experiencing snow ice formation. However, it would be more informative, as you mentioned as well, to reference the percentage of the total track length, which calculates to 41%. The sentence is adjusted accordingly:

The results show that the model detects snow ice formation along 41% of the total track length of the analyzed Snow Buoy tracks, with the snow ice layer observed in 16 of the analyzed buoys. (Figure 1C).

L.228 The northeastern Weddell Sea refers to region C/NE.

L.230 Thank you for your attention to detail. The mean maximum snow ice thickness has been corrected to 35 cm in the text accordingly.

Figure 3B No dashed line is depicted for Snow Buoy 2018S59 because snow accumulation equals snow depth, indicating that no snow ice was formed in the 1-D model.

Figure 3C You are correct; this uncertainty might arise, e.g., from the fact that both snow ice and superimposed ice are distinguished solely by their densities. To address this uncertainty, we have added another sentence on this matter at the end of the first paragraph in Section 3.4, where the formation of superimposed ice is discussed:

However, it is imperative to account for uncertainties, notably the differentiation between snow ice and superimposed ice, which relies solely on density values (as discussed in Section 2.4).

Figure 3, caption Thanks again for your attention to detail as we indeed missed to state the superimposed ice layer thickness in the caption, which we just added.

*[…], and additional superimposed ice layer thickness in **C**.*

Figure 3 We agree that it aids in clarity to indicate in the panels which parameters are retrieved from the Snow Buoy and which are from the respective model. Accordingly, we have added the references in the legend as suggested.

L.248 We acknowledge that we omitted to provide the proper context for the ice cores and to cite the respective literature reference. Therefore, we have added a sentence accordingly:

[…] which is reasonably consistent with the observed snow ice thickness of 21 ± 18 cm, ranging from 0 to 58 cm, obtained from ice cores taken on 19 different ice floes in the same region in 2021 (Figure 1A, Arndt (2022)). However, while only 14% of the Snow Buoy data points indicate potential snow ice formation in February/March (Figure 2B), 84% of the analyzed ice cores in the southeastern Weddell Sea (16 out of 19) confirm the presence of snow ice (Arndt, 2022).

L.253-254 You are correct in noting that the statement is unsupported, given the constant ocean heat flux we utilize. Additionally, we have recognized that the paragraph referring to the northwestern Weddell Sea does not align well with the content pertaining to the eastern Weddell Sea in this section. As the content is addressed in the following section on the western Weddell Sea, we have deleted the entire paragraph.

L.256-257 We agree that the reference to snow depth is not entirely clear in the text. Accordingly, we have adjusted the sentence accordingly:

Here, the highest monthly mean snow depth of 62 cm is observed in December and the lowest monthly mean snow depth of 45 cm in July (Figure 2C).

L.265-269 You are right! This was a clear repetition mistake in the text, and we have deleted the paragraph. Thank you for bringing this to our attention.

L.273-274 Considering that explicit buoy names are not referenced in the maps and are not consistently used throughout the text, we agree that providing them here may cause confusion.

L.280 The buoy mentioned here is among the explicitly named example buoys, which is why its name is provided. Therefore, it is distinct from the one mentioned previously.

L.283-284 We agree that proper referencing to the expedition data is lacking here. Therefore, we have adjusted the sentence accordingly:

[…] the sea ice model gives a mean snow ice thickness of only 12 cm (Figure 3B), i.e., one third of the measured layer thickness during the Endurance22 expedition, as mentioned above.

L.285 Thank you very much for your valuable input and suggestion. We have incorporated the recommended calculation, and the outcomes unequivocally illustrate that the Snow Buoys accumulated substantially less snow in comparison to the sampled ice floes. Consequently, we have included the following paragraph in the manuscript:

This variation in snow accumulation rates can be reassessed for the Endurance22 expedition by using the measured snow and snow ice thicknesses, under the assumption of a one-to-three ratio between snow and snow ice densities. Accordingly, an estimated snow accumulation of approximately 120 cm is derived for the sampled ice floes during the Endurance22 expedition, while the Snow Buoy 2021S114 had accumulated only around 70 cm of snow until February 2022. These variations in snow accumulation may be due to the Snow Buoys usually being deployed on level ice and therefore do not account for snow drifting and accumulating in ice ridges.

L.291 It's indeed a valuable point to further consider these variations. Based on the additional calculations above, it has become evident that the considerable variability in snow thickness significantly contributes to the variability in the snow ice formation. To acknowledge these distinct contributions, we have included the following sentence at the end of the paragraph:

However, for the inner ice pack, the highly heterogeneous patterns of snow accumulation appear to be the major contributor to the variability in snow ice formation, as shown above.

L.312 To clarify, this sentence refers to the number of buoy data points. Therefore, we have adjusted the sentence accordingly:

In this context, 50% of the analyzed buoy data points have superimposed ice thickness calculated, […]

L.328-329 Indeed, as you mentioned, due to its more northern location, the melt onset begins earlier in the season, enabling earlier and more extensive melt compared to regions further south.

Summary/ conclusion We consider a summary of the main results here to be a repetition of the previous subsection rather than a benefit to the manuscript, so we have decided not to include such an additional short paragraph. Additionally, comparisons to previous observations in the Weddell Sea have been elucidated within the respective regional sections. Furthermore, comparisons to other study regions have been incorporated into the introduction, as suggested in the earlier review.

L.348-349 We acknowledge the importance of clearly outlining the uncertainties of the study to ensure a proper assessment of the results. Accordingly, we have included the following paragraph to address this aspect.:

We acknowledge that our assumption on ocean heat flux does not take into account its regional and temporal variability, potentially causing ocean heat flux and thus snow ice formation to be underestimated. Additionally, the immediate assumption of snow ice formation with negative freeboard may result in an overestimation of actual flooding and snow ice formation. While these processes may balance each other out, future research could enhance process understanding at the snow/ice and ice/ocean interface, for instance, through the deployment of autonomous thermistor string buoys, aiming for improved evaluation of the presented results.

L.353 We agree that regional disparities in sea ice thickness contribute to the variable layer thickness of snow ice within different regions. Accordingly, we have incorporated a corresponding paragraph detailing recent observations of sea ice thickness from observational data to reinforce the findings presented in Section 3.3.:

This assumption is supported by observational data on sea ice thickness in the Weddell Sea. During the Endurance22 expedition in February 2022, conducted in the northwestern Weddell Sea, a predominant sea ice thickness of 1.05 meters was recorded (Rabenstein, 2022). Contrarily, data from the previous summer in the southeastern Weddell Sea, in February/March 2021, indicated a modal sea ice thickness of 1.5 meters (Arndt, 2022), highlighting the associated significant bottom melt, and related flooding and snow ice formation processes in the region.

L.356-357 We concur with the need for clarification regarding the contributions to the thick snow ice layer. Accordingly, we have rephrased the following sentence to provide additional clarity:

As a result, our analysis suggests that the thickest snow ice layers within the perennial sea ice zones of the northwestern Weddell Sea are primarily sourced from the southeastern Weddell Sea, owing to the sequential snow ice formation process occurring first in the southeastern region and then extending to the northwestern Weddell Sea.

L.370-371 We agree that the phrase "hidden processes" is rather cryptic and have therefore clarified the sentence to refer to snow ice and superimposed ice formation:

These hidden processes, i.e., the formation of snow ice and superimposed ice, will remain hidden, […]

Review response on

"From snow accumulation to snow depth distributions by quantifying meteoric ice fractions in the Weddell Sea" by Stefanie Arndt et al.

Anonymous Referee #2

Received and published: 25 January 2024

This study aimed to try to understand some of the complex snow processes that occur on Antarctic sea ice such as snow-ice formation and super imposed ice in order to improve snow models and also satellite measurements of sea ice volume. They used multiple Snow Buoys and insitu core data from multiple ship campaigns over the years in the Weddell Sea. The snow buoys tell the amount of snow accumulations and melting that occurs throughout the year, and the cores give the amount of snow ice and super imposed ice. A 1-d sea ice model and a more sophisticated snow model (SNOWPAK) are used to model the amount of snow, and super imposed ice and ice formation that is taking place at the buoy locations as they drift throughout th4e Weddell Sea. They found that the snow models were not the best at producing the amount of snow and ice compared to observations, which is likely caused by missing processes that are taking place within the ice. They also determined that more snow/ice formation is occurring in the western weddell sea and at lower latitudes.

While this paper was well written and easy to follow, I wish there were more details on the individual models and processes there in. What ways could the models improve to improve the snow-ice and super imposed ice estimates? How do the models produce the snow-ice?

More details like this could improve the paper. What would happen if you change the densities and salinities of the snow and ice in the models, will the snow depth be more similar to the buoy snow depths? More details like this would help.

Otherwise, I really enjoyed this study, and I think It makes a great addition to our understanding of the Antarctic snowpack.

We sincerely appreciate the constructive input provided by the reviewer for enhancing our manuscript. Also, we are delighted to hear that they enjoyed reading it. To address the highlighted lack of detail in describing the two model approaches, we have substantially revised both subsections, as detailed below.

In addition to both reviews, we conducted a re-assessment of all SNOWPACK model results due to the following reason: Upon re-evaluating the model code for running the SNOWPACK simulations, we identified an error in writing out the ERA5 reanalysis data to a format suitable for the SNOWPACK model. In an earlier version, the surface fluxes were provided in 6-hour intervals, requiring division by 6 to align with the hourly input data. However, in the current version of retrieved ERA5 data, all parameters, including surface fluxes, were available in hourly steps. Despite this, they were still divided by 6 when producing the meteorological input for the model. This error has been rectified, and all simulations were re-run using the corrected meteorological forcing data.

2.3 One-dimensional thermodynamic sea ice model

A simple one-dimensional thermodynamic ice growth model based on the number of freezing degree days (Thorndike, 1992), as used in Arndt et al. (2021), is applied to estimate the evolution of the thermodynamic sea ice growth at the bottom of the ice and the resulting ice freeboard. For the latter, a simplified assumption is made that a calculated negative freeboard causes potential flooding of the snow/ice interface and subsequent snow-to-ice conversion, i.e., snow ice formation, both taking place in the same time step. Previous studies by Wever et al. (2021) have demonstrated that such an assumption tends to overestimate actual flooding and snow ice formation on a floe-scale, primarily due to the significant spatial heterogeneity of snow and ice thickness. To address and assess this limitation, the results of the model runs are evaluated alongside in-situ observations (refer to section 2.5).

Model runs are initialized with the measured initial sea ice thickness during buoy deployment. The atmospheric forcing of the model, i.e., surface temperature and heat fluxes, is based on ERA5 reanalysis data (Copernicus Climate Change Service, 2017), which were extracted for the nearest-neighbor grid points of the daily buoy positions.

Reported values for ocean heat fluxes in the Weddell Sea vary, ranging from 2-7 Wm^2 (Lytle and Ackley, 1996; Robertson et al., 1995) in the western part to well above 20 Wm^2 (McPhee et al., 1999) in the central part. Considering that the buoys primarily drift in the inner pack ice, the ocean heat flux for the simulations in this study is set toward the lower end of the range, prescribed with a constant flux of 3 Wm^2.

For snow density and thermal conductivity regionally adjusted parameters, following Arndt (2022), are utilized. Specifically, snow densities of 340 kg m^{-3} and 264 kg m^{-3}, along with snow thermal conductivities of 0.28 $Wm^{-1}K^{-1}$ and 0.17 $Wm^{-1}K^{-1}$ for perennial and seasonal sea ice, respectively, are applied. Further details regarding the model setup can be accessed in the appendix of Arndt et al. (2021).

2.4 Multi-layer snow model SNOWPACK

To estimate the amount of both snow ice and superimposed ice formed during the buoys' lifetime, we use the multi-layer snow cover model SNOWPACK. In the one-dimensional SNOWPACK model, snow microstructure is represented in detail and liquid water flow and refreezing processes are taken into account (Bartelt and Lehning, 2002; Lehning et al., 2002a; Lehning et al., 2002b; Wever et al., 2015; Wever et al., 2016). SNOWPACK was originally developed to represent physical processes in the snow cover in alpine regions, but has been adapted and applied to sea ice environments recently (Wever et al., 2021; Wever et al., 2020).

For our simulations, we initialize the model with the initial snow and ice thicknesses as measured during buoy deployment. The initial snow and ice temperature profile is determined from the ERA5 surface temperature and a water temperature of 271.35 K, assuming that the snow and ice column is in thermal equilibrium. For the remaining properties, we follow the approach of Wever et al. (2021): For the snow layers, we assume an initial density of 275 kg m^{-3}, corresponding to a volumetric ice content of 0.3 m3 m^{-3} and a volumetric air content of 0.7 m3 m^{-3}, and set the grain radius to 0.15 mm, the bond radius to 0.09 mm and sphericity and dendricity to 0. For the ice layers above sea level, we assume the volumetric ice and air content to be 0.95 m^3 m^{-3} and 0.05 m^3 m^{-3}, respectively. Ice layers below sea level are assumed to contain also a fraction of water, calculated from the layers' temperature and an assumed bulk salinity of 1.75 g kg^{-1}.

Following the approach of Wever et al. (2021), we use the Richards equation, combined with the transport equation for salinity to solve liquid water and brine distributions. For two out of the 36 Snow Buoys (2017S49 and 2019S88) the simulations are aborted due to numerical instability and we use a more simple bucket-type approach instead of the Richards equation. In contrast to Wever et al. (2021), we choose an atmospheric stability following Holtslag and De Bruin (1988), as in the SNOWPACK model documentation this was changed to be the new default setting (the effect on the snow depth is minor). Like in the simple thermodynamic sea ice model described above, the ocean heat flux is set to a constant value of 3 Wm^{-2} and the atmospheric forcing is based on ERA5 reanalysis data, specifically surface heat fluxes, humidity, and surface wind.

Here, we present snow-height-driven simulations, which means that the SNOWPACK simulations are forced to closely follow the snow height evolution as measured by the Snow Buoy. While snow accumulation as indicated in the Snow Buoy datasets will lead to an instant increase in the SNOWPACK simulation (like a precipitation event), sudden reductions in snow height will only be incorporated within the scope of the model physics. Wind-induced transport of snow is neglected. The simulated snow densities are used to distinguish between snow (density ≤ 600 kg m^{-3}), superimposed ice (600 kg m^{-3} < density < 918 kg m^{-3}) and snow ice (density ≥ 918 kg m^{-3}).