Thanks very much to the reviewer for their constructive comments and suggestions on this manuscript, which is crucial for further improving the quality of the paper and providing it with scientific validity and significance.

Here are our preliminary replies to the comments:

Comment 1: while reading this I felt this manuscript was hastily put together, and reads more like data report that does not meet the standards of The Cryosphere.

Re: Our work mainly focuses on the differences in response of three types of sea ice on the same ice floe to the same atmospheric and oceanic forcing. Based on the comments of the reviewer, we will strengthen the Analysis and Discussion on: 1) the differences in the impact of snow cover on sea ice heat budgets of three ice types; 2) the impact of micro porosity (brine content) of the internal sea ice under the refrozen melt pond on the specific heat of sea ice, its temperature changes and mass balance; 3) the consolidation process and energy balance of partially unconsolidated ice ridges; and 4) the comparison with previous results, especially the observation results of the MOSAiC's first drift, including level ice and ridge ice. We have confidence in enhancing the innovation of the paper through these works, to meet the publishing requirements of the Cryosphere.

Comment 2: A fundamental issue is the lack of proper description and cknowledgement of existing work, this is very surprising given the list of authors that should be aware of much this work (and some is their own).

Re: In the revised manuscript, we will fully consider the research foundation of previous studies, conduct analysis and discussion on this basis, and strengthen the comparison with previous results, focusing on summarizing the following research work: 1) observation of sea ice mass balance in the Arctic Transpolar Drift region, 2) the impact of snow cover, especially the impact of high snow accumulation rate on sea ice heat budget, 3) seasonal and spatial variations of ocean heat flux under the ice; 4) the potential errors and influencing factors of reanalysis data in the Arctic Ocean, as well as their correlation with sea ice processes, 5) the consolidation process of ice ridges, 6) the impact of dynamic processes on the mass balance of sea ice, etc.

Comment 3: At the core of the paper is the interpretation of the temperature (and heating?) data from the SIMBA buoys. This has not been explained at any level that this can be reproduced or evaluated. The assumptions behind the interface locations are practically not described at all (was it automated, or manually done while looking at the data, or a combination). This needs to be significantly improved and documented properly, if there is a slightest chance for the reader to try and assess the quality if the work done. No unertanties are given, which in the case of e.g. estimated ocean heat fluxes are important to know. The macroporosity estimates in the ridge has such large range, that is hard to understand what the value of reporting them are?

Re: The identification method based on temperature (heating) data for the air-snow-sea ice-seawater interfaces has been introduced in previous studies, and the revised manuscript will provide a clearer introduction to the identification method. We will further discuss the potential calculation error of ocean heat flux under the ice. Regarding the estimation of the macroscopic porosity of the unconsolidated layer of the ice ridge, we provide a vertical distribution and therefore have randomness. We will provide an average value based on previous research definitions, and analyze the possible impact of the microscopic porosity of the ice block on the calculation results, as well as the impact of parameterization schemes for ocean heat flux on the calculation results.

Comment 4: Related to the above (2), one of the co-authors is an expert in thermodynamic modeling. It would strethen the paper if the modeling can support the derived fluxes and interfaces from the SIMBAs (As the authors have done in earlier papers). I find it strange that e.g. the ridge at nearly 3 m depth suddenly grows very rapidly, when there is no noticeable change in the other thinner ice, can this be only due to thicker snow on level ice? Typically ridge sails also accumulate a lot of snow, even if the crest does not. A thermodynamic model would at least be able to support/strengthen the work in my opinion.

Re: Numerical model experiments are very important for studying the mass balance of sea ice, but they are not the content of this study. We will strengthen the comparative study on the mass balance of sea ice at different measuring sites from the perspective of heat budget. The ice layer under the refrozen pond is mainly affected by the high brine content in the ice, which will affect the cooling process of the ice layer by affecting the specific heat of the sea ice, so the growth of sea ice lags behind. For ridge ice, on one hand, the snow thickness was relatively small, and on the other hand, the bottom of the consolidation layer was not directly connected with the upper ocean, but an unconsolidated rubble layer. The latter will reduce ocean heat flux by weakening ocean turbulence and promote the downward growth of the consolidation layer. When the freezing front develops downwards to the bottom of the consolidation layer, the consolidation layer begins to grow. Due to the presence of ice rubble, the growth rate was significantly higher than that of level ice.

Comment 5: 4) I was surprised of the minimal comparison to the conditions during the first MOSAiC drift (where the same author published another paper). although not the same year, the comparison of these two regions would possibly be an aspect to include. At least I find the differentece os "low snow" and "high snow" regions along the TPD intriguing aspect. Regionality in the Arctic is often overlooked.

Re: High precipitation and snow accumulation do not occur every year in the TPD region, which is related to the amount of warm and humid air mass from low latitudes. 1) We will strengthen the comparison with the first drift of MOSAiC, not limited to flat ice, but also ice ridge. Due to human operation, the ice ridge observation during MOSAiC first drift has been carried out more fully, and the relevant papers are already in the stage of submission or publication. We will cite these literature to explain and compare the mechanisms involving the ridge consolidation process. 2) The mass balance of sea ice is also greatly affected by the temporal nature (or timing) of snow accumulation, such as at our observed site corresponding to the beginning of the growth period. Thus, we will cite the results of some models to illustrate the impact of timing of snow accumulation on ice mass balance; 3) The speed of the southward drift of ice floes is related to the atmospheric circulation pattern, as we discussed previously. We will add the observation results of Wang et al., 2016 and N-ICE as comparative literature to illustrate the impact of the speed of the southward drift of sea ice on the mass balance of sea ice; 4) We will add some calculations of cumulative heat conduction flux on the top boundary of ice layer to illustrate the influence of snow cover on the heat budget of the ice layer.

Comment 6: Generally the motivation for this work and conlusions are a potluck of things and very vague. It simply lacks a clear statement of what is actually the value of this work and primary motivation. What new is presented that would truly change the way sea ice is modeled? Abstract mentions aspects I do not necessarily see in the paper itself, or are extrapolated beyond what I believe can be exploited from the data at hand. I think a complete rewrite of manuscript with clear idea of what are truly the novel aspects of this work would help to pin down the emphasis, and possibly bring the ms to the standards that need to be met to publish in TC.

Re: The main focus or motivation of this study is to compare the thermodynamic response mechanisms of different sea ice under the same atmospheric and oceanic forcing. We believe that with the improvement of the resolution of the sea ice numerical model, the research on these sub-grid scale sea ice thermodynamic process and their spatial heterogeneity is very important, and it is very important to optimize the parameterization scheme of the sea ice model. In addition, according to our understanding, the observations of refreezing process of the melt pond and the gradual cooling and consolidation process of partially consolidated ice ridge starting from late summer are very rare, especially for such full process observations. Relevant observations are conducted on the same ice floe, which is very beneficial for us to compare thermodynamic mechanisms of different types of sea ice. We believe that the relevant research results can support the optimization of numerical models. In the revised manuscript, we will further focus on our research motivation and discuss the direct contribution of research results to the development of numerical models.

Comment 7: There is now ample literature from this area with thicker snow, and the effect of this thicker snow on the sea ice. These works needs to referred to place this work better in context, earlier observations have in fact already observed similar episodes where thick snow, winter storms (warming events) have ample effects on the sea ice. Same goes with some observations of ridge thermodynamics. There is also work on observations of ocean heat fluxes (directly and indirectly) that should be compared to. I am also suprised the work in the exact same area with ice mass balance buoys and ocean heat fluxes from one of co-authors (Perovich) are not referred or comapred to as far as I can see.

To me this is quite a breach of good practice, not to acknowledge earlier work properly, and claiming novelty. This work needs to do much better job to refer to earlier work, and place this work better in context before making claims of novelty.

I propose that the authors read up on the literature that exists (especially in the study region), and this can help place the current observations in historical context (has things changed since the 1980s?, was this winter "normal" in terms of storms (warming events), similar effect of the thick snow,   how did it compare to the first mosaic drift etc.):

Re: Thank you very much for providing so many suggestions on previous research literature. Indeed, we need to increase the citation of these literature and compare our results with previous study to identify the representativeness of the observation results, the degree of anomalies, and new discoveries, especially the effects of snow cover and temperature rise, the oceanic heat flux under the ice, and the consolidation process of ice ridge.

Comments 8: Title, although buoys drifted in the transpolar drift, but only from the North Pole, could be in place to be more specific in the title on the actual region covered..

Re: We will further revise the title, and the preliminary determination as: Sea ice drifting from the central Arctic Ocean to Fram Strait in 2020/21: thermodynamic evolution of different ice types.

Comment 9: Why are these supposedly largest on floe scale?? Not sure what this statement is based on. What about snow, varies on meter-scale and can drive much of the thermodynamics you are (attempting) to discuss in this paper .

Re: There are some deviations in our expression, not particularly, it is “even for”. The heterogeneity of sea ice and snow cover at the floe scale (100-1000 m) is crucial for optimizing the parameterization of subgrid processes in sea ice column models (e.g., CICE).

Comment 10: Line 13 - here you refer to kilometre-scale, is that different from floe scale on line 11?

Re: It is on the scale of 100-1000 m. We will revise it and make it more accurate.

Comment 11: This seems also like quite strong statement, some processes cannot be directly to represented so they are represented by a parameterization, and that will always be the case unless model resolution is increased. Not much value with such generic statement to try motivate this study.

Re： In the revised manuscript, we will emphasize the importance of our results in optimizing parameterization schemes for sea ice column models (such as CICE), which are important components of climate models. And by increasing our knowledge on the sub-grid process of sea ice, we can gain a deeper understanding of the regulatory role of the sea ice-ocean system in the warming of the Arctic lower atmosphere. Then, in the revised version, we will focus on the impact of snow cover, the impact of the brine content in the lower ice layer under the the refroze melt pond, and the consolidation process of ice ridge, which have been relatively scarce topics in previous observations and research, especially for complete ice season and data obtained on the same ice floe.

Comment 12: based on a quick look of the periods, you hardly come into the melt period in the following summer.
Re:
1) For ridge observation site, the melt period has already entered by the end of the observation period. Based on such observation results, we will further compare them with previous results, especially regarding keel melt rate and ocean heat flux under the ice, although the melting period is not complete.
2) In addition, we will emphasize in the revised version that our work mainly focuses on the refreezing process of different sea ice types starting from the end of summer.

Comment 13: for non-MOSAiC peeps the "2nd drift" does not mean anything, suffices probably to say where and when they were deployed.
Re： It is a good comment, we will revise the manuscript following this suggestion.

Comment 14: line 16 - melted through pond? I guess with "open" you it was not refrozen at time of deployment?
Re： Thank you for your clarification. This is an error in the terminology, and we will revise it. When deployed the buoy, the surface of the pond was not frozen.

Comment 15: line 20 - what is meant by enhanced ice dynamics? how does that affect superimposed ice formation?
Re: This refers to sea ice fracture, which would promote seawater infiltration, thereby forming superimposed ice. This has already been introduced in our original text. In the revised version, we will make the expression clearer and easier to understand.

Comment 16: line21-22 - relatively large snow accumulation - relative to what? what is large? Be more specific.

Re: It was relative to the observation results of buoys also deployed in the TPD region. In the revised manuscript, we further compare our results with previous observations.

Comment 17: This is extremely large range of values, and lower end is simply what brines could occupy, so is this really macroporosity? And on what (vertical scale)?
Re: 1) The quantity range of macroporosity is related to the random distribution of disordered ice rubbles, which has been confirmed in previous drilling observations. In order to maintain consistency with previous terminology definitions, we will provide the macroscopic porosity of the entire unconsolidated ice rubble layer in the revised manuscript. 2) Another uncertainty in estimating macroscopic porosity based on the energy balance at the bottom of the consolidated ice layer comes from the parameterization scheme of oceanic heat flux. We will further consider that ice ridges may enhance the ocean turbulence under the ice keel, but the pores between ice rubbles may lead to weakened permeation flow within the unconsolidated ice layer, and the water temperature under the keel and within the unconsolidated ice layer remains around the freezing point. Multiple parameterization schemes will be considered to test the impact of different ocean heat flux parameterizations on porosity estimation.

Comment 18: Line 25-26 - why would it change the anisotropy and why does it matter at all? Please clarify why this should be of importance.
Re: The spatial heterogeneity of snow and sea ice characteristics will increase the anisotropy of air-ice heat exchange by changing the heat released from the ice layer to the atmosphere (such as the conductive heat flux on the ice surface). In the revised version, we will add some discussions on the differences in the conductive heat flux on the ice surface at different measurement sites and their integration over the time to illustrate the impacts, especially by the differences in snow cover.
The anisotropy of air-ice heat exchange will further increase the uncertainty of atmospheric reanalysis data regarding near surface temperatures, as well as the assessment of uncertainty in Arctic climate warming amplify. Thanks for the suggested literature: Batrak & Müller, 2019, which is beneficial for optimizing our discussion and improving scientificity.

Comment 18: Line 27-28 - first half of sentence is very vague, and has no value. Provide some more concrete results from this study.
Re: we will remove this sentence and focus on the concrete results derived from our study.

Comment 19: Line 26 - what about the role of ocean heat flux in this area?
Re: The heat flux of the ice bottom ocean was relatively weak in the deep water region of the Arctic Ocean, and had little impact on the energy balance of the ice bottom. However, when the ice floe drifted to the shallow water area of the continental shelf in northeastern Greenland, the oceanic heat flux under the ice rapidly increased, which was the main factor promoting the melting of the ice bottom. We

will add a summary of the spatiotemporal variation characteristics of oceanic heat flux in the revised abstract.

Comment 20: General comment - to me Introduction is overly long, for the limited scope if the topic the paper attempts to examine. I would consider to make the Introduction more concise, and limit the number of self-citations (seminal papers could be cited, instead of own work of more recent nature)..
Re: We will further compress the introduction, focus on the scientific issues of our concern, and add some original literature citations.

Comment 21: LIne 30 - I would not say it completely restricts, since gases (mass) can be exchanged.. and energy is transferred through, please re-phrase.
Re: Yes, it is not completely restricts, we can rewrite this sentence.

Comment 22: Line 41 - why so? Why are they more salty when ice is thinner?
Re: We will revise this expression to make it clearer. Thin sea ice is more likely to melt through or promote the lower ice layer to enter a high permeability state, thus having higher salinity. (Kim et al., 2018).

Comment 22: What about Wadhams and Toberg (2012) that state the opposite? Wadhams, P., & Toberg, N. (2012). Changing characteristics of arctic pressure ridges. Polar Science, 6(1), 71 – 77. https://doi.org/10.1016/j.polar.2012.03.002
Re: Thin ice has a more pronounced response to wind and current forcing, making it more prone to deformation. The observation data of up-looking sonar in the Fram Strait also indicates that although the proportion of deformed ice is decreasing, the thin deformed ice is increasing, indicating a trend of increasing first-year ice ridges. In the revised manuscript, we will cite the researches on the changes in the probability distribution of ice thickness in the Fram Strait and discuss this issue in conjunction with the research by Wadhams and Toberg (2012).

Comment 23: how is this estimated, and what fraction of ice volume is in ridges?
Re: This was estimated using the the probability distribution data of ice thickness obtained from the observation of the mooring system in the Fram Strait using up-looking sonars, and we will further cite the updated research results by Sumata et al., 2023. We also will revise this expression, in fact, it (66%) is the proportion of ice thickness samples is 66%, not the ice thickness or volume.

Comment 23: Lines 60-64. Does read awkwardly. Lu et al works on melt ponds and not ridges, so the citation to Lu et al does not make sense here. Please consider to split to few sentences to improve clarity.
Re: we will split this sentence into few sentences to improve clarity.

Comment 24: Is the physics different elsewhere than in the Arctic? How?

Re: In in the Arctic peripheral seas, the oceanic heat flux would be larger, the melt period would longer, which are major differences from the ice in the central Arctic Ocean. We will make the expression clearer.

Comment 25: Melt season consolidation might be more important for unconsolidated than in winter, since it can be more rapid.

Re. Generally, melt season consolidation occurs suddenly and is related to the discharge of melt water of snow and ice, with randomness in both time and space; Although the consolidation process in winter is relatively slow, it is universal and can cause desalination as level ice growth, affecting the stratification of the upper ocean. Almost all non fully consolidated ice ridges undergo winter consolidation. Therefore, we believe that both mechanisms are crucial for the mass balance of sea ice, and our work mainly focuses on the consolidation and growth process of ice ridges from late summer to spring, as well as their comparison with level ice and refreezing melt pond. In the revised manuscript, we will add a review and summarize the contributions and differences of different mechanisms on ice ridge consolidation, and compare our results with the observations of MOSAiC CO. For example:

Salganik et al. Preferential SummerMelt of Deeper Ridge Keels in the Central Arctic Ocean from Multibeam Sonar Data. submitted to Geophysical Research Letters
Salganik et al. Different mechanisms of Arctic first-year sea-ice ridge consolidation observed during the MOSAiCexpedition. Submitted to Elementa: Science of the Anthropocene.

Comment 26: Line 68-69 - Also familiarize yourself with the work of Marchenko and coworkers. There can also be freezing in summer when meltwater refreezes in keel rubble. e.g. Marchenko A. 2022. Thermo-Hydrodynamics of Sea Ice Rubble. In: IUTAM Bookseries. Springer International Publishing. p. 203 – 223. doi: 10.1007/978-3-030-80439-8_10
Re: As mentioned above, in the revised manuscript, we will add a review and summarize the contributions and differences of different mechanisms on ice ridge consolidation. Thanks for the literature recommendation.

Comment 27: Line 85 - what is the estimate of deformed ice based on, is it by area or volume?
Re: it is by area, we will clarify it.

Comment 28: line 87 "Snow/scatting" you mean the surface scattering layer .. ?
Re: Sorry, it is a typing error. We will correct it.

Comment 29: Figure 1 - its really hard to distinguish the initial color of the deployed SIMBAs from the bathymetry that has very similar shade. Is the bathymetry necessary to show? Or use a different color scheme for the Date.

And should also use the same color for the end point "triangle" as use for the starting point, impossible to track the gray lines fo figure out which one is which.
Re: We will modify this illustration following the suggestion.

Comment 30: Not sure how relevant this is for the non-MOSAiC reader ... and this is "one leg of the MOSAiC expedition", and not an "expedition" in it self ..
Re: we will modify this express following the suggestion.

Comment 31: anchored.
Re: It is a typing error. We will correct it.

Comment 32: highly subjective assessment, please be clearer what you mean by massive, or give dimensions later and drop the word "massive"
Re: we will modify this express following the suggestion. Actually, it means 30% of areal coverage.

Comment 33: Is this total thickness, sail + keel? Please specify.
Re: Yes, and we will specify it.

Comment 34: Why mention the Bruncin SIMBA at all.. consider rewriting and only mention the four that are used here..
Re: we will remove the Bruncin SIMBA here.

Comment 35: was this the duration of the heating or the duration after the heating the temperature was measured, please clarify.
Re: It is the duration after the heating. We will clarify it.

Comment 36: assume this was "bare ice"? or with the SSL?
Re: The sites have some thin SSL (~5cm), we will clarify it.

Comment 37: Line 132 - how did you assess consolidation? how accurate was this "measurement"?
This is based on the first sudden drop during drilling to determine the bottom of the consolidation layer, with an error of approximately 5-10cm. When the freezing front of the consolidation layer develops downwards, the interface where the consolidation layer begins to grow was basically consistent with the position determined by the drilling hole, with a deviation of less than 5cm. In the revised version, we will provide this detail.

Comment 38: line 143-44, nothing of this albedo work is mentioned in the abstract, was this solely an add-on that is not necessarily in the core of the paper?
Re: Comparing the freezing process of different types of sea ice is the core of this study. The melt pond refreezing will significantly affect the albedo, which in turn will

affect the freezing process of sea ice under the pond, which we believe is an important process. Therefore, we will keep this part of the content, but will clearly indicate the main purpose of analyzing this data.

Comment 39: Table 1 - please also add the info on freeboard
Re: We will do it.

Comment 40: Line 159 - CTD buoy, why same color as SIMBA, at all of the SIMBA sites?
Re: we will use other mark color for the CTD buoy 2020O10.

Comment 41: Is this done with temperature alone, or also the heated profiles? There is insufficient detail about the methods to detect the interfaces provided. This needs to be improved so reader can assess the robustness of the interface detection, and the assumptions that go into it.
Re: We used both the temperature and heating data to identify the interfaces. We will give the detail of the methods in the revised manuscript.

Comment 42: Figure 3 - Use the same x-axis limits on all the panels, makes it much easier to compare the temporal evolution and timing! Please adjust the panels that the all show the same period (even if there is missing data). Figure 3 - ALso show the air temperarture above the panels and adjust the x-axis so they are the same for all panels, this way you can also decide to show a smaller temperature range, that it is easier to have a closer look at the evolution of the ice temperatures. Also, please show the heated temperatures as well, in case they were used to deduce the interfaces.. In periodes when the deployment hole was freezing, is the blue line simply an approximation of the ice thickness and deployment? In that case, show that as blue dashed line!

Re: We will modify this illustration following the suggestion. And we will show the heat data as supplementary materials.

Comment 43: And an interesting comparison is how different the first MOSAiC drift was compared to the drift from North Pole to Fram Strait.. if it really matters a lot, where the ice floe is along the TPD drift, in regard to e.g. snow accumulation..
Re: In the revised manuscript, we will add some comparisons with MOSAiC first drifting observations and previous observations in the TPD region to illustrate the impact of snow accumulation and drift speed towards the Fram Strait on sea ice heat budget and mass balance.

Comment 43: Line 192 - Please provide the uncertainty of the estimates. Given the assumptions, how large is the uncertainty in the estimate ocean heat flux? Same order as the estimate itself?

Re: In the revised version, we will provide potential errors in estimating ocean heat flux and relative errors compared to the estimated values.

Comment 43: Line 194 - Did it reach the keel bottom? can the bottom of the keel be indicated in Figure 3?
Re: Yes. By April 2021, it is estimated that the bottom of the consolidation layer has extended to the bottom of the ice keel, and thereafter the bottom of the ice keel can be identified since then. Before this time, the bottom of the keel cannot be recognized. We will make the express more clearly.

Comment 44: Line 206-7: I do not really understand how you can make this argument. There is ample evidence that deeper ridge keels melt much faster, which might also mean there is a larger heat flux at depth .. or is that supposedly simply due to macroporosity? See e.g.
Amundrud, T. L., Melling, H., Ingram, R. G., & Allen, S. E. (2006). The effect of structural porosity on the ablation of sea ice ridges. Journal of Geophysical Research: Oceans, 111(6), 1 – 14. https://doi.org/10.1029/2005JC002895
and
Shestov, A., & Ervik, Å. (2016). Studies of Drifting Ice Ridges in the Arctic Ocean during May-June 2015. Part II. Thermodynamic properties and melting rate. In 23rd IAHR International Symposium on Ice. Ann Arbor, Michigan.
Lines 213-214 - what is the evidence for this assumption? Wouldn't the ocean heat flux be larger closer to the bottom of the keel? And if the rubble is isothermal (at seawater temperature) any ocean heat flux would melt the keel also in midwinter?
Lines 219-222. This explanation needs to be improved for clarity,. Again the expected uncertainty for the estimate should be given. Looking at the derived macroporosity values seem sometime unrealistic.

Re: We agree that the oceanic heat flux under the ridge would larger that under the level ice. The ocean turbulence would rapidly decay in the unconsolidated pores within the keel, so when the unconsolidated layer is relatively thick, the ocean heat flux at the bottom of the consolidated layer will be very low. Therefore, based on the following background: 1) The winter ocean heat flux is very low because the water temperature in the upper ocean is close to the freezing point temperature, 2) The ocean heat flux under the keel bottom may be larger than that under the level ice, 3) The ocean heat flux will decay within the macroscopic pores, 4) The definition should state that the influence of micro porosity of ice block is unavoidable,
we will add some sensitivity calculations to illustrate the impact of the estimated/parameterized ocean heat flux on the estimation of macroscopic porosity, and qualitatively evaluate how much micro porosity accounts for the estimated total porosity.

Comment 45: I would like to see some of the critical data here, also shown in Figure 3, so that one can compare the key forcing relative to the SIMBA temperature. Please

consider (even if some data is shown twice), to ease the readers job to compare the forcing data, with the SIMBA observations ..
Re: This illustration mainly reflects the comparison of atmospheric forcing and sea ice state with climate state, so it is difficult to combine the ice temperature data. We will add air temperature data to Figure 3, which is conducive to evaluating the impact of changes in air temperature on ice temperature.

Comment 46: From what data is the multi-year average, the SIMBA data or ERA5 data?
Re: It is from ERA or remote sensing product, we will clarify it.

Comment 47:Line 567: But it was partly consolidated by time of deployment?
Re: Yes, it was partly consolidated by time of deployment. We will correct this description.

Comment 48: But, most of the same season was actually covered by the first MOSAiC drift, although not the very early stages of re-freezing. Please correct. To me what is interesting is how different the snow conditions were, compared to the earlier paper by the first-author from MOSAiC SIMBAs. Comparison to that study should be done more thoroughly, that could enlighten regional comparisons..
Re: 1) The early stages of ice refreezing was missed by the first MOSAiC drifting because it started from min-October, and the surface freezing for both ice and melt pond occurred in later August to September, which is the purpose we conducted this study. This is crucial for identifying the difference of thermodynamic seasonal evolution processes of level ice (bare or with thin snow), refrozen ponded ice, and partially consolidated ice ridges. Thus we will declare our research focus in the introduction of revised manuscript. 2) We will strengthen the analysis of the impact of snow cover and compare our results with that obtained from the first MOSAiC drifting and other measurements obtained from the TPD region.

Comment 49: line 574: the drift was same for all four buoys, so how does this shape the freezing of the different ice types?
Re: this conclusion is driven from the comparison among the sea ice mass balance obtained from TPD region. We will make the expression clearer.

Comment 50: line 574 "small ice thickness" - to me the ice thicknesses were not small, in fact when the MOSAiC drift started in late 2020, the initial ice thickness was much less (at least according to the work by Krumpen and others). So I find this statement is not grounded.
Re: The thinner ice is compared to satellite remote sensing observations on buoy trajectories in the past 10 years. This expression is indeed not rigorous enough, we will modify it.

Comment 51: line 575 - for 4 individual points, is hardly possible to tell that melt ponds had more snow than other parts of the ice floe?? Is there snow buoy data to corroborate this statement?
Re: We will further compare it with observations of snow buoy or transect measurement of snow thickness to confirm this conclusion. E.g., cite the paper
Itkin P, Hendricks S, Webster M, von Albedyll L, Arndt S, Clemens-Sewall D, Jaggi M, Oggier M, Ricker R, Rohde J, Schneebeli M, Liston G. 2022. Sea ice and snow mass balance from transects in the MOSAiC Central Observatory. Elementa: Science of the Anthropocene.

Comment 52: line 575 - relatively warm compared to?
Re: Relative to the climatology. We will clarify it.

Comment 52: line 576 - more frequent than during the MOSAiC drift? and are these rather "warm events" and contribute to the supposedly "relatively warm" temperatures..
Re: Warming events and the Arctic climate warming amplification can lead to higher temperatures during the observation period relative to the climatology. We will further compare our results with the situation of the first drift of MOSAiC, including precipitation, temperature, warming events, and accumulated snow on ice surface.

Comment 53: Line 581-584 - what about the snow, you stated there was lot of snow at the melt-pond cite? Earlier work in the area has already shown the effect of snow and synoptic events, the observations presented here, should be placed better in context of those earlier observations and acknowledge the earlier work on the subject which as not been cited here.
Re: The accumulation of snow on the ice area and its occurrence time are very important to the sea ice mass balance. Thus, we will 1) strengthen the comparison with the previous observation results, especially the N-ICE observation results, 2) further strengthen the analysis of the impact of snow on the sea ice conductive heat flux, 3) add some key literatures.

Comment 54: Line 587-588: Is this macroporosoity variability in time or in space? Such a large range of values seem to have little value as such. Please clarify why this result is valuable, since it represents a single ridge.
What about the drilling when deploying, was the macroporosity estimated then?
Re: 1) Even if it is a single point of observation, such observations (covering the entire freezing season) have not been many in the past. In the central Arctic Ocean, there are even no such observations. Therefore, our observation of this long-term series is still very meaningful, which is conducive to analyzing the coupling between the consolidation process of the ice ridge and the porosity of the lower rubber layer. We will emphasize the significance of such observation data in the revised manuscript.
2) this is macroporosoity variability in space (vertical variation). Due to the random distribution of ice blocks in the rubber layer, the porosity has a certain degree of

dispersion, which has been confirmed in previous borehole observations. We will further clarify our definition in the revised version and explain that it may be affected by the micro porosity of the ice block itself. Therefore, lower values can be removed, but the vertical average value is trustworthy.

3) When deployed the equipment, only the position of the bottom of the consolidation layer was recorded, without recording the complete vertical distribution of pores. On the other hand, even if the vertical distribution of pores is recorded, it is only a one-time observation and cannot determine the effective porosity at the corresponding bottom boundary when the consolidation layer grows downwards. Therefore, it is still necessary to evaluate the coupling relationship between porosity and consolidation process based on energy balance.

Comment 54: Line 588 - hardly if it is 75%? How was the consolidation related to the ocean heat flux?

Re: 1) The vertical variation of macroscopic porosity presented here, rather than the vertical average value given through borehole observations, is therefore a relatively wide range. 75% corresponds to the thermal equivalent porosity at the cavity. We will make our definition clearer in the revised version and provide the vertical average porosity for comparison with previous observations. At the same time, we will analyze the impact of the micro porosity of the ice itself on our estimation results.

2) Because it is winter, the water temperature is basically consistent with the freezing point temperature, and ocean turbulence decays within the bubble layer, the impact of ocean heat flux on ice ridge consolidation is very low. In the revised version, we will add some sensitivity calculations to quantify the impact of ocean heat flux.

3) Comment 55: 590-591 above you state that ridges slow down the growth, but there you claim the reduce the ice growth. Please clarify.

Re: The freezing process of winter ice ridges is mainly reflected in the consolidation of the rubber layer, rather than the freezing at the bottom of the keel. Our expression here is not rigorous enough, we will further revise it

Comment 56: lines 591-593: Since melt ponds and ridges are included in most models, you need to be more specific about what needs to be improved exactly.

Re: In the revised manuscript, we will emphasize that 1) After the complete freezing of the melt pond, the refroze pond will still affect the growth of the lower ice layer, mainly because the high porosity of the lower ice layer (mainly the brine channel) will increase the specific heat of sea ice and reduce its cooling rate, and 2) the winter freezing mechanism of ice ridges. These processes are not fully parameterized in the sea ice model.

Comment 56: Lines - 604-607 - very vague statements that carry very little value. Delete or be more concise and to the point about what the value of this study is.

Re: We will remove this sentence and focus on our results and their scientific implications.

Comment 57: Data availability - are also the derived interfaces going to be published, and not the raw data? Who is acknowleded for the per-MOSAiC data that is somewhat used in this paper?
Re: The data will be available soon on the PANGAEA, which is in the review stage.

Comment 58: Line 751-761 - These two look very similar,. what is the difference??
Re: Sorry, it an editing error, we will correct it.