Author Response to Editor and Referee Comments

Editor comments

Comment 1 (Appendix)

Editor comment: Appendix is short to the point of it being redundant. Would suggest getting rid of it and assimilating the material into the main text (in line with referee 2), or move some of the content in the body to here (in line with both referees). I have no strong opinions either way; I would have gone for the first option personally, but this isn't my paper (I think the readability is fine as is, disagreeing with referee 1 on that front).
Author response: Agreed, we actually realised the material in the previous appendix was already in the main text, so we have removed it from the appendix.
Changes to manuscript: We have moved content from the first half of section 2.1 and material from section 2.2 to the Appendix, in line with comments from both reviewers. We have also added more information on the selection of model parameters to the Appendix, in line with reviewer 1.

Comment 2 (General Comments)

Editor comment: I agree with referee 2 about "Arctic + Antarctic" grouping to "differing stratification" or similar. I think the authors can keep this general rather than locking themselves into the grouping by geographical location, especially when that grouping reads quite unbalanced at the moment in terms of length (the "Antarctic" subsection is quite short, and the "Antarctic Summer" subsubsection sticks out unnecessarily like the Appendix).
Author response: We did intend for the 'Arctic/Antarctic' grouping to reference a differing background stratification sensitivity test. We have reworked the wording to make this clearer, and the simulations using the Arctic-like or Antarctic-like stratifications are now referred to as such, with the section 3.3 now being titled 'Sensitivity to background stratification', as we agree we don't want to limit ourselves.
Changes to manuscript: We have changed the naming of the "Antarctic" experiments to a differing background stratifcation sensitivity experiments (section 1, 2.1, 3.3, and 4). We also added a sentence to emphasize this in section 4 (lines 105-106).

Comment 3 (Results)

Editor comment: Fig. 2 to me is taking up a lot of space and not adding very much. Consider shrinking this a bit.
Author response: Yes, agreed.
Changes to manuscript: Figure 2 has been shrunk a little.

Comment 4 (General Comments)

Editor comment: Would recommend adding background grid lines in the line plots, partly to help the reader, but also to break up the blocks of white space.
Author response: We will add grid lines to the line plots.

Changes to manuscript: All figures containing line or scatter plots (Figures 1, 3, 5, 6, 8, 9, 12, 13 and 14 have had background grid lines added to them).

Comment 5 (General Comments)

Editor comment: There are some in-line referencing format that doesn't work, e.g. line 146, 468, 479, where you probably want
cite rather than
citep (no brackets, since the object is the article itself).
Author response: We have amended these in-line citations using the proper version of "cite".
Changes to manuscript: See the response above; corresponding text has been revised where needed (now lines 487, 441, 495).

Comment 6 (General Comments)

Editor comment: The spelling is internally inconsistent at the moment (e.g. 457 vs 464, "parameteriSation" vs "parameteriZation"). Copernicus type-setting might change it at some point, but would push for internal consistency for now, with all the "S"s or "Z"s etc. (unless you want to go with the Oxford spelling distinguishing Latin or Greek roots...)
Author response: We have amended the 'parameteriSation' cases, so that is it consistently 'parameteriZation' throughout.
Changes to manuscript: See the response above.

Referee 1 comments

Comment 1 (General Comments)

Referee comment: This manuscript presents an interesting and timely investigation of the role of submesoscale mixed-layer eddies (SMLEs) in modulating the seasonal ocean heat budget and sea ice evolution near the marginal ice zone (MIZ). By comparing eddy-resolving (3D) and non-eddy (2D) MITgcm simulations, the authors isolate the thermodynamical impacts of SMLEs, which are often neglected in coarse-resolution climate models. The study addresses a significant knowledge gap by quantifying how SMLEs influence sea ice melting and freezing processes through heat transport and feedback mechanisms. The paper is generally well-motivated, clearly written, and scientifically sound. The approach is methodologically appropriate, and the results provide new insights into the coupling between submesoscale dynamics and polar climate processes. The identification of distinct summer and winter responses strengthens the contribution and highlights the necessity of submesoscale parameterization in climate models. Overall, this is a contribution of interest to the polar oceanography and climate modeling communities. Some clarifications and improvements would further strengthen the manuscript before publication.
Author response: We thank the reviewer for their overall positive assessment of the manuscript. We agree that there is room for improving the presentation of the work. We detail the response to the comments below.
Changes to manuscript:
See the changes detailed below in response to the specific comments.

Comment 2 (Introduction)

Referee comment: The introduction provides a comprehensive overview of submesoscale dynamics and their relevance in polar regions. However, it is somewhat verbose and would

benefit from streamlining to emphasize the central research question. The authors may consider shortening the general background on SMLEs, improving transitions between topics, and highlighting the novelty of their thermodynamic focus relative to previous studies. While observational and modeling studies are well cited, the flow from general submesoscale theory to polar-specific impacts could be smoother. Adding brief linking sentences to connect classical SMLE mechanisms with polar sea ice interactions would enhance readability. Moreover, the main research gap, which involves quantifying the thermodynamic impacts of SMLEs across seasons and background stratifications, should be emphasized earlier, ideally before detailing the study objectives.
Author response: We agree the introduction could do with better streamlining. We will link sentences, reference the research gap earlier, and emphasize how the previous research directly links to the new research here, making the transition between paragraphs easier.
Changes to manuscript: The introduction has been updated, including adding linking sentences, referencing the research gap higher up, and some of the background on SMLEs has been shortened.

Comment 3 (Materials and Methods)

Referee comment: The description of the model setup contains many technical details in long, complex sentences. For example, information on vertical and horizontal grid spacing, mixing schemes, and viscosity settings could be split into shorter sentences or a table. This would improve readability and make it easier for readers to understand the experimental design.
Author response: We have some of the material referred to above in section 2.1 to the Appendix (A1). Following comments also from referee 2, we also moved material from section 2.2 to A2, and have cited relevant studies (Horvat 2016/Abernathey 2011/Marshall and Radko 2003) where needed in these sections. Also, where sentences have become long, we have broken them up for ease of reading.
Changes to manuscript: We have moved much of the detail of the model set-up to the Appendix (A1). We have also moved detail on the residual-mean framework to A2.

Comment 4 (Materials and Methods)

Referee comment: Some parameter choices, such as the Smagorinsky coefficient, horizontal eddy viscosity, and small horizontal diffusivity in winter, are described, but the rationale is brief. It would strengthen the manuscript to explain why these values were selected, particularly how they affect numerical stability and the development of SMLEs, and whether sensitivity tests were conducted.
Author response: These values were selected (a higher horizontal viscosity on the divergent part of the flow dv/dy and a lower viscosity on the rotational component of the flow du/dy) to decrease grid point noise, correlated to sea ice formation, on the meridional velocity field whilst not dampening the zonal jet. Sensitivity tests were conducted and these values were found to be the best for this purpose. To reduce the grid-point noise in the thermodynamic fields, the small amount of horizontal of temperature and salinity was introduced in the model. In addition, a third step to help with this issue was to change the smoothing settings within the KPP package. A few regularisations and smoothing options with the KPP package were investigated. These include reducing the shear mixing when the velocity shear is low and the Richardson number is large. They helped to further reduce the noise in the velocity fields. In 3D we used the same values, whilst acknowledging this could potentially impact the development of SMLEs, by weakening the fronts. We will add the above description to the appendix along the other model details.
Changes to manuscript: The details requested and given in the response above have been added to the Appendix (A1, lines 451-465).

Comment 5 (Materials and Methods)

Referee comment: While the 2D "no eddies" configuration is introduced, the description could clarify explicitly which processes are suppressed (e.g., lateral variations, baroclinic instabilities) and ensure the naming of the experiments is consistent (Arctic/Antarctic, summer/winter). Providing a concise summary table of the main experiments with initial conditions and key parameters would greatly enhance reproducibility.
Author response: We will add a sentence or two to further the explanation of the 2D experiments, explaining that baroclinic instability is suppressed in this set-up. In line with comments by reviewer 2 and the editor, we will change the naming of the Arctic/Antarctic experiments to be a differing stratification test, giving also a more consistent naming of experiments as asked for here. As we have said above, we moved some content from section 2 to the Appendix. Thanks for your suggestion of the table, but as we have moved some content to the Appendix, we now think the model parameters (atmospheric forcings) in the main text are much clearer to read and think a table is not necessary here.
Changes to manuscript: For the 2D experiments, we added lines 88-89. For the naming of the experiments, we have changed wording throughout (see response to Comment 2 from the Editor). In particular here the specific wording in lines 91-94 was changed, specifying the two main experiments are called Artic summer and Arctic winter and the other two experiments described in section 3.3 have a differing initial stratification that is Antarctic-like.

Comment 6 (Materials and Methods)

Referee comment: The methods section contains a large amount of technical detail, including grid spacing, viscosity and diffusivity settings, and atmospheric boundary treatments. While these details are important for reproducibility, some of the more intricate numerical specifications could be moved to the Appendix. This would streamline the main text, improve readability, and allow readers to focus on the key experimental design and scientific rationale, while still providing full information for replication.
Author response: As mentioned above in response to Comment 3 from reviewer 1, we have moved some of the information in the initial paragraph of 2.1 and parts of 2.2, making a reference to Horvat et al. (2016) for 2.1; this is also in line with reviewer 2.
Changes to manuscript: See section A1 in the Appendix where the more intricate specifications have been moved to.

Comment 7 (Results)

Referee comment: The Results section 3.1.1 provides a detailed description of SMLE development and their impact on the mixed layer during Arctic summer, including vertical stratification and buoyancy fluxes. While the simulations and analyses appear comprehensive, the presentation could be strengthened by improving the logical flow and emphasizing the physical interpretation. Currently, the text mixes descriptions of forcing, stratification, eddy development, and vertical fluxes in a single narrative, which can make it challenging for readers to follow the causal chain. Reorganizing the section to first describe the atmospheric and oceanic forcing, then the resulting stratification and MLD evolution, and finally the eddy dynamics and their restratifying or destratifying effects would improve clarity.
Author response: We agree, we have done some re-ordering of this section to ensure clearer flow, we did aim for the structure the reviewer has described originally, hence we have reworked this.
Changes to manuscript: Section 3.1.1 has been re-ordered to follow the logical flow that the referee describes, which we think has improved the clarity of the text.

Comment 8 (Results)

Referee comment: The analysis of the ML heat budget and eddy impacts (Section 3.1.2) is thorough and provides valuable insight into the mechanisms by which SMLEs redistribute heat between open and ice-covered regions. However, the presentation could be improved by emphasizing the causal interpretation and quantitative comparisons more clearly. For instance, the roles of MHT and Qnet are described in detail, but it would be helpful to explicitly highlight how the presence of eddies amplifies meridional heat transport compared to the 2D simulation, and how this relates to changes in ice melt or ML warming. Additionally, the text could more clearly distinguish between contributions from shortwave, longwave, and sensible fluxes in both regions, linking them directly to the eddy-induced heat redistribution. This would strengthen the physical interpretation and make the connection between eddies and observed heat budget changes more immediate for the reader.
Author response: We thank the reviewer for their insight on how to improve clarity and flow when discussing the eddy-impacted heat budget. Detail requested by the reviewer was added and the text restructured, to add clarity and strengthen the interpretations we provide. In addition we assimilated text from section 3.1.3 and 3.2.3 into sections 3.1.2 and 3.2.2 respectively. This has helped add clarity the reviewer asked for where needed and has tightened the text.
Changes to manuscript: Specific text here was added within section 3.2.1 on how eddies amplify meridional heat transport, how this relates to ice melt and ML warming, distinguising the contributions from shortwave, longwave and sensible heat fluxes and linking these to the eddy-induced heat distribution. To support this text and figures from previous section 3.1.3 has been assimilated into section 3.1.2 and the former section has therefore been removed.

Comment 9 (Results)

Referee comment: The Antarctic results (Section 3.3) are presented clearly, with useful comparisons to Arctic simulations, but the section could benefit from emphasizing the physical interpretation of the differences. For example, the text could more explicitly link the faster ML deepening and weaker stratification in Antarctic winter to the smaller eddy impact on sea ice formation, and clarify why the summer eddy impacts are relatively insensitive to initial stratification. Including brief quantitative comparisons or ratios directly in the text (e.g., differences in MLD deepening or lateral density gradients) would help readers quickly grasp the relative magnitudes. Finally, a short discussion of the potential effects of neglected wind forcing on Antarctic results would strengthen the assessment of model limitations.
Author response: These comments are in line with those of reviewer 2, both have asked for a re-work of this section. There is actually a similar impact to sea ice under Antarctic-like conditions, so we will clarify that this deeper MLD in the Antarctic-like weaker stratification doesn't lead to a significantly different impact on sea ice formation (+/- 3%). We clarify the insensitivity to summer stratification in section 4, we will add more information here too. We will quantify some more specific values of MLD and lateral buoyancy gradients (and give more specific references to the figure in section 3.3). We touch on potential effects of neglected wind forcing overall in section 4. We will also reference other locational differences that we don't consider here that could alter the impact of SMLEs in different environments such as the Antarctic environment, suggested by reviewer 2, like atmospheric forcing or topographic effects.
Changes to manuscript: The details added are as follows. Whether the faster ML deepening leads to a different impact on sea ice formation (lines 347-352), clarifying why the eddy impacts are relatively insensitive to the initial stratification in summer (lines 326-329), including brief comparisons in the text of MLD deepening and lateral density gradients (line 336 and lines 344-345 respectively), discussion of potential effects of neglected wind forcing and other locational differences (lines 410-419).

Comment 10 (Technical Corrections)

Referee comment: In Section 2, "Materials and Methods," it is recommended to split the content into two subsections for clarity: 2.1 "Model setup" and 2.2 "Residual-mean framework." This would improve the organization and make it easier for readers to follow the methods.
Author response: Agreed, done.
Changes to manuscript: We have split Section 2 "Materials and Methods up into two subsections, named 2.1 "Model set-up" and 2.2 "Residual-mean framework".

Referee 2 comments

Comment 1 (General Comments)

Referee comment: Greig & Ferreira is a well-written and topical study that will be a useful contribution to the community. They show that submesoscale mixed layer eddies play a key role in the heat and sea ice budgets of the marginal ice zone, and investigate the sensitivity to background stratification and seasonal forcing. I think there are many interesting results, however I find the overall framing of the Arctic/Antarctic conditions to be a bit strange. The only difference between the "Arctic" and "Antarctic" simulations is the initial stratification. So why not just frame the study as testing the sensitivity to background stratification? There are so many differences between the Arctic and Antarctic marginal ice zones in the real world: the forcing, large-scale circulation, winds, ice type/thickness, etc. Of course, these things are not represented in your idealized setup, which is fine. But I just think it's a bit misleading to present these simulations as representing an Arctic/Antarctic contrast. If this is your intent, then why choose these specific locations from EN4 to initialize the model (there can be very significant regional differences in the mean density structure within the Arctic and Antarctic, respectively)? My point is just that I think the results would be more accurately framed as testing the sensitivity to background stratification. This is a very valid and useful thing to do, and it's great to mention how this links broadly to the Arctic versus Antarctic, but I think this could be done with a bit more nuance (and I think these results are actually useful outside the context of comparing the Arctic to the Antarctic).
Author response: We thank the reviewer for their overall positive assessment of the manuscript. We agree that additional clarifications and context can be given, and some re-structuring would be helpful to improve the manuscript. We agree that phrasing these Arctic/Antarctic experiments as sensitivity to a background stratification is best, and we acknowledge this previously read as simply locationally different experiments. This has been rephrased throughout the paper, renaming these 2 experiments (previously named "Antarctic summer/winter") as a sensitivity test to differing stratification, with Antarctic-like stratification used for both seasons. This is contrasted with the main experiments that most of the paper focuses on (i.e., summer and winter Arctic-like stratification).
Changes to manuscript: Please see changes specified under Comment 2 from the editor who made a similar point.

Comment 2 (General Comments)

Referee comment: Also, to clarify, you are using the standard MITgcm sea ice package without any representation of sea ice floes? I feel this needs to be addressed directly. Sea ice floes in the marginal ice zone have similar length scales to submesoscale ocean flows, so most idealized modeling studies that deal with submesoscale sea ice-ocean interactions use either a simplistic representation of floes or a discrete element sea ice model. And typically, the relevant dynamics that are identified relate explicitly to floe-flow or floe-floe interactions. This needs to be discussed as it is important to understanding how your results relate to previous work. All

this said, I want to emphasize that I think these are interesting simulations and it's a very nice paper dealing with an important subject!
Author response: Indeed, we did not include representation of sea ice floes as some effects of ice floes have been explored in previous studies and we aimed to isolate the thermodynamical ocean-ice processes. This limitation is addressed in section 4, referencing other studies and future work.
Changes to manuscript: This was addressed in section 4 (lines 414-419).

Comment 3 (Introduction)

Referee comment: Line 22-26: Perhaps also worth mentioning that ocean heat has been invoked as a leading driver of the recent, rapid Antarctic sea ice loss (e.g. Purich & Doddridge, 2023). Purich, A., E.W. Doddridge (2023). Record low Antarctic sea ice coverage indicates a new sea ice state. Communications Earth Environment, 4, 314.
Author response: Agreed, added.
Changes to manuscript: Thank you, this was added in the introduction (lines 22-23) as well as in the References (lines 566-570).

Comment 4 (Introduction)

Referee comment: Line 46: You are citing the pre-print rather than the actual published paper for Giddy et al. (the year should be 2021, not 2020). Giddy, I., et al. (2021). Stirring of sea-ice meltwater enhances submesoscale fronts in the Southern Ocean. Journal of Geophysical Research: Oceans, 126, e2020JC016814.
Author response: Thanks, amended the reference.
Changes to manuscript: The reference was amended (lines 43, 525-526).

Comment 5 (Introduction)

Referee comment: Line 51 (and throughout): Need to be clearer about whether you're referring to lateral or vertical heat transport by SMLEs.
Author response: In this paper we mostly refer to lateral transport by SMLEs, as we don't quantify vertical here. We will clarify this here and in the results section 3.1.2, explaining that the ML heat budget was calculated within the final MLD, so there aren't large SMLE vertical heat fluxes across the bottom surface. Hence we refer almost always to meridional heat transport (MHT), that is shown in Fig 6.
Changes to manuscript: Clarification was made in the introduction (line 55), and in section 3.1.2 and 3.2.2 (lines 182-184, 258-263).

Comment 6 (Introduction)

Referee comment: Lines 70-71: Yes, I agree that more studies on submesoscale sea ice-ocean interactions have focused on mechanical effects. But Horvat et al. (2016) highlighted thermodynamic melt of sea ice floes by submesoscale eddies, and Gupta & Thompson (2022) considered both thermodynamic and mechanical interactions. These papers are cited elsewhere in the manuscript, but should at least be acknowledged
Author response: Agree, we have cited these papers in the paragraph of the introduction talking about studies including thermodynamic effects as the reviewer mentions, and as suggested we re-cite references when talking about how the thermodynamic aspect relates directly to the specific research gap.
Changes to manuscript: These studies are acknowledged again when focussing on the exact research gap (lines 67-69).

Comment 7 (Introduction)

Referee comment: Another highly relevant paper to this work, which is not cited, is Brenner et al. (2023). Brenner, S., et al. (2023). Scale-dependent air-sea exchange in the polar oceans: Floe-floe coupling in the generation of ice-ocean boundary layer turbulence. Geophysical Research Letters, 50, e2023GL105703.
Author response: We thank the reviewer for this reference. Indeed, this is a highly relevant paper and builds on the Shrestha & Manucharyan (2022) paper referenced, important to include within our introduction.
Changes to manuscript: We added reference to this study within the introduction amongst other studies of mechanical effects of SMLEs as appropriate (lines 52-53).

Comment 8 (Introduction)

Referee comment: Introduction: a general comment on the introduction is that you do not do much to distinguish between the Arctic and Antarctic. Yet, a major focus of the simulations is comparing Arctic and Antarctic conditions. As I mentioned at the start, I'm a bit hesitant about this framing. But if you are going to proceed with this, then you should at least outline some of the differences between the poles in the introduction (and this could still be discussed briefly even if you do alter the framing around background stratification).
Author response: As in our response above, we have now shifted the focus and wording throughout the paper to a sensitivity to background stratification experiment and have also outlined differences between poles in the introduction.
Changes to manuscript: Again, please see the changes outlined under Comment 2 from the Editor. Further we briefly discuss differences between poles in lines 60-62.

Comment 9 (Materials and Methods)

Referee comment: Lines 87-101: Since the model setup is essentially the same as Horvat et al. (2016) and much of this paragraph is just paraphrased from that paper, I think it is fine to condense this a bit and cite their paper for the details (or move to an appendix).
Author response: We will condense this and cite their paper, as well as moving some more information to the appendix on the model parameters, as suggested by reviewer 1.
Changes to manuscript: We have moved the majority of the model setup description to the Appendix (A1) and kept the citation of Horvat et al. 2016 in section 2.1. Please see also response and changes outlined under Comment 3 from Reviewer 1.

Comment 10 (Materials and Methods)

Referee comment: Lines 107-110: See my initial comments about the framing of these simulations as Arctic versus Antarctic.
Author response: Agreed, amended text.
Changes to manuscript: Please see response and changes outlined in Comment 2 from the Editor about the framing of Arctic and Antarctic simulations. Further, the wording was changed on lines 91-94 (in line also with Comment 5 from Reviewer 1).

Comment 11 (Materials and Methods)

Referee comment: Line 113: Does ERA5 have MLD as an output?
Author response: No, it doesn't, it is only atmospheric data (no ocean component, SST and sea ice concentration are prescribed by observations), and here we meant ORAS5. We did also do a sensitivity analysis to the MLD in section 3.4, to show how this choice matters.

Changes to manuscript: See line 101 describing the corrected data source for MLD of ORAS5.

Comment 12 (Materials and Methods)

Referee comment: Line 119: How are the surface fluxes computed in the sea ice-covered part of the domain?
Author response: We will add an explanation of surface heat fluxes under the ice, which is essentially a conduction and latent heat flux as well as shortwave penetration through the ice. However, for full details we will reference the MITgcm manual thsice section, and the sea ice model by Winton.
Changes to manuscript: Lines 112-114 provide details of the sea ice model used in the MITgcm as cited above in the response and a description of the surface heat flux under ice.

Comment 13 (Materials and Methods)

Referee comment: Section 2.1: First off, this could be Section 2.2 and you could have the earlier part of the Methods section be "Section 2.1 Model Set-up." But more importantly, this section needs to be introduced better. You need to start by stating what you are doing and why. i.e. you are using the isopycnal streamfunction and EOS to isolate the eddy-induced circulation, etc. Otherwise the section begins very abruptly and it's not even clear how this relates to the aims of the study until the end of subsection.
Author response: We've split section 2 into sections 2.1 and 2.2. We have also re-worked this section so it is less dense and clearer, moving more detailed parts to the Appendix (A2). In doing this we provided references in section 2.1 to how the framework relates to the study's aims.
Changes to manuscript: Please see the author response which describes how we have split the sections and how we have re-worked this section for clarity.

Comment 14 (Materials and Methods)

Referee comment: Also, this technique is pretty widely used to diagnose eddy transports so you can probably condense some of the detail and just cite past work (e.g. Abernathey et al. 2011) or move to an appendix.
Author response: We have moved material from this section to the Appendix (A2), since it contains a lot of technical details of calculations.
Changes to manuscript: We have moved material from this section to Appendix A2 to condense it and cited relevant studies on lines 120 and 126.

Comment 15 (Results)

Referee comment: Lines 186-187: This is interesting, and what does this mean for the heat transport? If the front is salinity-dominated then the sign of the buoyancy flux is not necessarily the same as the sign of the heat flux.
Author response: Yes indeed the heat transport is from open ocean under ice (heat budget, section 3.1.2 and figure 6). The front is salinity dominated only very close to the surface, over the top few metres, and temperature dominated below that over the bulk of the mixed layer. Therefore over most of the depth where eddies transport heat northwards within the heat budget, the density gradient is temperature-dominated.
Changes to manuscript: Thanks to the reviewer for raising this, the information provided in the response that addresses this is given in the manuscript through section 3.1.1 and 3.1.2 (lines 151-154, 164-165, 197-198).

Comment 16 (Results)

Referee comment: Equation 5 & Lines 219-221: As I asked before, how are the surface fluxes computed in the icecovered part of the domain? This is important to interpreting the heat budget.
Author response: See above response to this query (Comment 12, Reviewer 2).
Changes to manuscript: As above, see specification in section 2.1, lines 112-114.

Comment 17 (Results)

Referee comment: Lines 242-243: The main narrative thread has gotten a bit lost by this point. So just to clarify, the 3-D simulation has a lower sea ice volume due to the meridional heat transport by SMLEs (rather than the vertical heat transport)? I think it's important to clarify lateral versus vertical fluxes.
Author response: As mentioned in our response to Comment 5 from Reviewer 2, we refer to meridional heat transport by SMLEs for our heat budget discussions (Fig. 6). Where vertical heat transport is mentioned, it is specified to be small or negligible. For the narrative points the reviewer mentions here, sections 3.1.2 and 3.2.2 have both been re-worked for clarity and also in line with Comment 8 from Reviewer 1.
Changes to manuscript: As per the respose, please see exact changes specified under Comment 5 from Reviewer 2 (lateral vs. vertical heat fluxes), and Comment 8 from Reviewer 1 (narrative tightening).

Comment 18 (Results)

Referee comment: Lines 267-268: I think this is important, lateral temperature gradients are weak because the temperature is near the freezing point, so you can have eddy buoyancy fluxes that are not accompanied by significant heat fluxes.
Author response: Yes, we will re-work this sentence to emphasise that here.
Changes to manuscript: Yes, we have specified this in a few places (lines 240-243, 273-275, 402-405).

Comment 19 (Results)

Referee comment: Line 331: "temporal" changes sounds more natural than "time" changes to me.
Author response: Agreed, changed.
Changes to manuscript: Corrected (line 287).

Comment 20 (Results)

Referee comment: Lines 351-353: This gets at my initial comment about framing this as Arctic versus Antarctic. If the initial stratification is similar in summer at both poles, then these simulations will show similar results. But in the real world, there are many things that could contribute to differences in SMLE activity between the poles (forcing, winds, ice characteristics, topographic constraints, etc). In particular, the winds are very different. I know these things aren't represented in your simulations, but it seems like you're stating that the effect of SMLEs is the same at both poles in summer, and I think this is misleading. You address this a bit in lines 359-361, which is great. But I think you could expand upon this. Winds are known to be much stronger in the Antarctic, so you could state this directly, or even speculate on how this might manifest in the SMLE story.
Author response: As well as the changes in wording of section 3.3 as mentioned in above responses (changing Antarctic section to a differing stratification), we also added some text to

section 4 to refer to further differences in other model set ups and in reality than are considered between the Arctic/Antarctic or indeed any different locations.
Changes to manuscript: Thanks to the reviewer for their insight here. Throughout the paper we have changed wording to be based around tests to initial stratification (and other background conditions such as initial sea ice thickness), that are represented in sections 3.3 and 3.4 (again please see changes specified under Comment 2 from the editor). In particular, we speculate on this in the summary in section 4 (lines 410-419).

Comment 21 (Results)

Referee comment: Lines 379-381: This is interesting. If we assume that the APE reservoir scales with the MLD and lateral buoyancy gradient, then we might expect the eddy-induced overturning to be greater in the "Antarctic" i.e. deeper MLD case. So why is streamfunction weaker in the Antarctic setup? Is the lateral buoyancy gradient weaker?
Author response: Indeed the lateral buoyancy gradient is weaker although the MLD is deeper in the case with Antarctic-like stratification. This is in line with the Fox-Kemper et al (2008) scalings referred to for the eddy-induced stream function, though a more quantitative comparison with this Fox-Kemper parameterization is left to a future study.
Changes to manuscript: A few lines were added to clarify this at the end of section 3.3.2 (lines 353-358).

Comment 22 (Summary and Discussion)

Referee comment: Lines 418-450: I think it's really nice to list the key outcomes, but these are a bit dense. I think you should include all of this information in the Conclusions section, but you could consider putting some of this in the text (rather than bullet points) and really consolidating your key bullet points to 3-4 streamlined takeaways.
Author response: Yes, we have consolidated our main points to a few takeaway points, which relate directly to a tighter narrative that we have now achieved throughout the paper. We don't have all the information in the summary that was there previously, but due to the tighter narrative elsewhere now, we would argue it is not needed (there is now also added information in the summary too, see e.g. response to Comment 20 from Reviewer 2).
Changes to manuscript: We have streamlined the previous bullet points to a few key takeaways (lines 392-409).

Comment 23 (Summary and Discussion)

Referee comment: I also feel you need to more directly highlight the lateral versus vertical transports when discussing the eddy heat fluxes. Is it correct to say that the eddy heat transport is primarily lateral in summer and vertical in winter? Or is the meridional heat transport just weaker in winter compared to summer (but still larger than the vertical fluxes)?
Author response: We have clarified vertical versus lateral heat transport in-line. As above mentioned, here, we talk about SMLE lateral heat transport only when we reference the results figures. In winter, lateral eddy transport is counteracted by eddy-modified vertical heat transport, which in turn is dominated by the convection process of MLD deepening and heat lost at the surface. This is summarised as one of our key takeaway points.
Changes to manuscript: See lines 402-409 where we have reworked our key takeaway points for the winter case.

Comment 24 (Summary and Discussion)

Referee comment: Lines 456-467: These seems a slightly odd note to end on. I agree that we need SMLE parametrizations that are designed for (or optimized for) the polar regions. But this

work has not dealt directly with parameterizations. I think it's useful to speculate about the implications of the results for parameterization development, but this would make more sense earlier on in this section rather than as the final sentences. In my opinion, it's more impactful to end with a strong statement about the significance of this work, rather than invoking some other unpublished work that the reader does not have access to.
Author response: We agree, we will place move this earlier on, and end on a different note, more relating specifying the impact of the results of this paper.
Changes to manuscript: This paragraph on parameterizations now comes higher up (lines 426-436), whilst the final paragraph was added to emphasise the key results of this paper (lines 437-440).

Comment 25 (Appendix)

Referee comment: Lines 470-474: It's a bit random to me to have such a small appendix given how much detail you gave about the model setup in the main text. If you're going to keep all of that detail in the Methods section, then you may as well just include these two sentences there. Or alternatively, you could move a more significant amount of those details here and streamline the Methods section in the main text.
Author response: Agreed, we have moved more information to the Appendix in line with comments from both reviewers. Details of the model set-up were moved to A1, whilst some of the more technical details from section 2.2 were moved to A2.
Changes to manuscript: We have kept the Appendix and have moved a significant amount now to the sections A1-A2.