We thank the editor for providing a second set of editor's comments, addressed as follows:

"Darcy–Brinkman Framework: The use of the Brinkman term (for viscous shear) is
appropriate near the ice-water interface, but the transition to a pure Darcy flow in the
interior must be justified with a dimensionless analysis of the Damköhler or Brinkman
numbers."

We would agree with this point, and in fact have some doubts about the applicability of Darcy flow to mushy layer dynamics more broadly (the details of the porous properties of sea ice are likely strongly coupled to brine flow, which probably violates assumptions made in the derivation of the Darcy flow equations). It is important to note, though, that we do not employ Darcy flow in the manuscript. This is clarified in the first paragraph of Section 3 (see lines 130-136 in particular). Rather we use a parameterized flow simplification used and validated by previous authors.

The confusion may arise from the description in the Appendix, in which the parameterized model is introduced (in A6) later than the Darcy equation (in A5). We have added a clarifying remark to A5, see line 448.

"Scale and Context: While the model focuses on micro-scale convection, it lacks a
discussion on how these small-scale instabilities scale up. For example, could
microbially-induced convection be a precursor to larger instability? Differentiation
between these micro-scale processes and larger-scale features is necessary to provide
proper context."

This is for sure an interesting question, but we are far from being able to answer it without a much better understanding of mushy layer constitutive behavior and mechanics! That is, an understanding of fluid dynamics in mush layers would be required. (This is related to the above question about Darcy flow, for which we have some misgivings to its applicability.) While we are comfortable claiming, on fundamental physical grounds (backed by observations of lab ice), that a form of Rayleigh-Taylor instability should be observed, the detailed dynamics of this instability, including how it might trigger larger scale activity, are opaque to us.

"The manuscript currently lacks sufficient model validation against observed datasets and
requires more precise terminology regarding the porous media flow. In a research paper
your model must be benchmarked. This may be by comparing the model's predicted
vertical nutrient profiles or salinity "desalination" rates against existing data."

We have significantly reworked the conclusion section, largely in response to this point (with thanks to the editor).

We note that the mathematical model is not intended for stand-alone prediction. Rather it is meant as an in silico experiment. In the manuscript, we offer the hypothesis that microbes in the bottom part of the ice sheet can induce advective flow, communicating with the underlying ocean, through osmolyte production (and further can control the rate of this advection via the rate of osmolyte production). Absent direct measurements in sea ice, or lab ice for that matter, we instead offer results of a couple of hypothesis tests. First, measurements of freezing temperature of C. neogracillus lab cultures and their supernatants show significant impacts. Second, we construct a mathematical model which includes the mechanisms of the proposed hypothesis, and use that model to show that (within the model) introducing osmolyte-producing organisms can cause a drained, abiotic (model-derived) ice sheet to demonstrate biotically-induced advective flow at the bottom of the sheet.

We don't claim that the mathematical model, as currently constructed, should be used to make quantitative predictions of sea ice properties. At present, there are too many unknown parameters that would be required before making such an attempt. Rather, our aim is to propose a mechanism that may explain observations of extensive, active microbial communities within the bottom layer of the ice sheet as well as offer a possible explanation for observed high concentrations of DMSP. The modeling component of the manuscript should be regarded as a hypothesis test (as is the laboratory component), which, while supportive, is not claimed to be conclusive.

The Conclusion section did not reflect this point very well however, which may have led to the confusion. We've rewritten that section in order to make this more apparent to the reader.

The question about desalination is quite interesting (and actually something we are looking into at the moment). The mathematical model, as set up in the manuscript, is not suitable here as it relies on an already desalinated ice sheet as initial condition. Extension to desalination is a significant project in itself, as time dependence requires a model of heat flux from the underlying ocean, which is a rather separate issue than the ones considered in the manuscript.

"While this is a mathematical study, the research impact is limited if the upper boundary
condition is purely static. Please discuss how high-frequency atmospheric cooling
(thermal "shocks") interacts with the microbial-induced convection."

Similarly to the previous point, the current mathematical model is not intended, or suitable as currently formulated, to address time-dependence in external conditions - such an extension is actually a major project in and of itself. Rather, we have deliberately constrained the model to a constant-thickness ice sheet with constant external conditions, so as to better isolate the effects we wish to test (in the same spirit as constraining conditions for a lab experiment). Having said that, the editor's point is quite interesting - perturbations on ~24 hr time scales could interact with periodic behavior (~50-100 hrs) seen in the model results and quite possibly impact important outputs like convective layer thickness. We're already not confident in the model as predictive at this level though (see above discussion as well as lines 306-320 in the manuscript Conclusion section).

Our principal conclusion is that microbial activity, through osmolyte production, is capable of significant impact on sea ice structure and biological productivity. We back up our hypothesis with both lab and in silico tests. While, lacking the needed field data, we can't argue that we have made our case conclusively, on the other hand we don't see this as a point with limited impact! We are arguing that ice sheet physics and ice sheet microbiology should not be considered independent of each other. This is in fact the thrust of the final sentence (lines 335-336) of the manuscript.

"Permeability ($K$): You rely on a functional form of $K(\phi)$. Please ensure the
Kozeny-Carman constant used is specific to the columnar ice crystal structure, as recent
studies suggest the standard value of 5 is insufficient for sea ice."

Our model employs an empirical permeability coefficient $\Pi_0 = 10^{-8}$ m$^2$ calibrated specifically for sea ice, which absorbs microstructural parameters that would appear in classical Kozeny-Carman formulations derived for idealized granular media.

Interestingly, in any case, we note that our numerical results did not appear to be very sensitive to the details of the permeability parameterization (we tried a few different formulations). This seems to be because, in the model, osmolyte accumulates until permeability increases sufficiently to trigger convective flow, regardless of how permeability is computed. That is, permeability adjusts until flow initiates. The choice of permeability parameterization doesn't even affect osmolyte concentration all that much because of the cubic dependence on brine volume fraction. We have added a comment to this effect, see lines 268–270 of the revised manuscript.

"Notation: In Version 4, there is a mismatch in the symbol for brine salinity between
Section 2.1 and 2.3. Use a consistent $S_{br}$ throughout."

We're unsure of the meaning here as Section 2 has no subsections. We did find an inconsistency in volume fraction notation (phi_brine in Section 1.2 and phi_b in other places), and have corrected this.