Dear Dr McCormack,

Thank you for inviting us to respond to these reviews. Please find below our point-by-point response to the reviewer comments along with the corresponding revisions made to the manuscript. In response to the Reviewers we have made changes to clarify our explanation of the method, and our reasoning behind certain modelling choices (in particular the sharp-interface assumption, and our choice of ice sheet model). We have also sought to add further discussion of results and model limitations, or make existing discussion clearer, where the reviewers have indicated a need for this. Line numbers where used, refer to the draft manuscript seen by the reviewers.

Best regards,
Gabriel Cairns

Reviewer 1

Note: I was not a reviewer for the first round of comments, so I don't have detailed feedback on whether the first round of suggestions were implemented to those reviewers' satisfaction. However, regarding the sharp interface assumption, I do think this point could be discussed a bit more thoroughly, e.g. giving the Peclet number for this system. This is a moment where the model disagrees with the available observations; it would be nice to add some heuristic discussion (if possible) about how adding mixing might alter the conclusions. (I realise it is far too much to implement in this work, but in what direction are the effects more likely to drive the qualitative results?)

We have expanded our discussion of the sharp-interface assumption at lines 75–80 to more thoroughly justify the use of this assumption, with reference to the estimated size of the Peclet number.

"We use the sharp-interface approximation, a frequent assumption in saltwater intrusion problems, which asserts that mixing between freshwater and saltwater through molecular diffusion and hydrodynamic dispersion is negligible (Bear, 2013; Mondal et al., 2019). Since the inversion results of Gustafson et al. (2022) suggest a smooth variation in salinity through the aquifer rather than a distinct sharp interface, we include in Sect. 6 a discussion of how a similar model could account for these mixing processes, **although it is possible that this smoothing could be in part due to the inversion method used.**
The sharp-interface approximation is warranted provided that the Péclet numbers associated with molecular diffusion and hydrodynamic dispersion are large (Bear, 2013; Dentz et al., 2006; Koussis and Mazi, 2018). For a molecular diffusivity $D=10^{-9}$ m^2 s^{-1} and a hydrodynamic dispersivity length $A=10^{-2}$ m, which are appropriate for salt in groundwater in sedimentary rocks (Aquilina et al. (2015)), these Péclet numbers are respectively
$Pe_{diff} = [z]^2 / D\,[t] \approx 300, \qquad Pe_{disp} = [z]^2 / A[x] \approx 200,$
suggesting that the sharp-interface assumption is reasonable here."

We have also modified the paragraph at 551–559, to expand our discussion of how including mixing in the model might affect the conclusions:

“Our model has made the assumption of a sharp interface dividing freshwater and saltwater, **on the basis that the Péclet numbers associated with diffusion and hydrodynamic dispersion are large**. This greatly simplifies the task of solving the problem, at the cost of being unable to account for the smoothly varying salinity modelled by Gustafson et al. (2022), **although this smoothness may in part a result of the model used to invert the magnetotelluric data.** To account for the effect of mixing, it is possible (though more complicated) to solve the full problem of density-coupled salt transport, known as the Henry problem, (e.g. Croucher and O’Sullivan (1995)) or to introduce a “mixing layer” around the sharp interface where these effects are accounted for (Van Duijn and Peletier, 1992; Paster and Dagan, 2007). **In steady-state seawater intrusion problems, the inclusion of saltwater-freshwater mixing reduces the extent of seawater intrusion by inducing a circulatory flow in the saltwater-saturated region (Cooper (1959); Koussis and Mazi (2018)). We therefore expect that including mixing would result in a more retreated “interface” (which would need to be newly defined as e.g. the 50% salinity contour) than that achieved with the sharp-interface assumption at the same permeability. This would lead to a lower permeability estimate than that obtained above. The process of saltwater mixing would also reduce the trapping of saltwater in pockets via entrainment into the freshwater flow.”**

On a related note, the conclusions have become quite a sprawling discussion section, so could perhaps be formally split up - it's a little jarring to be directed to the conclusions when considering model limitations in section 2.

To distinguish discussion of e.g. the limitations of the model and possible extensions from our conclusions, we have split up the “Conclusions” section so that lines 489–559 are now referred to as “Discussion”, and the “Conclusions” section consists of lines 560–569.

I also wondered about the effects of residual trapping at a pore scale, rather than an aquifer-wide one; I appreciate it is too late in the review process to ask for much more than a comment here.

We do not expect pore-scale residual trapping to be relevant to this problem, because residual trapping occurs as a result of the surface tension between two immiscible fluids, whereas saltwater and freshwater are miscible and thus have no surface tension between them. We have added a comment at line 80 following on from our changes to the discussion above:

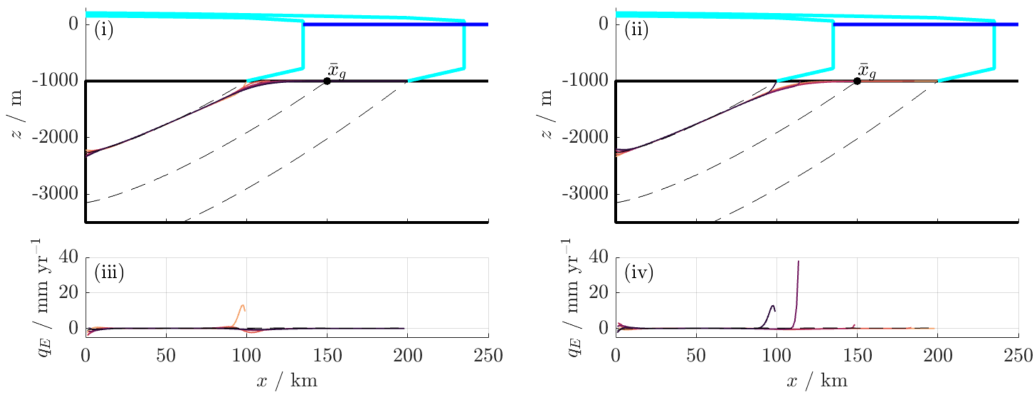
“…**reasonable here. Although we assume a sharp interface between the two regions, the fluids are miscible, meaning that there is no surface tension between them. We can therefore ignore the possibility of residual trapping at the pore scale (Bear, 2013).**

The aquifer therefore…”

Minor comments:

Line 273: An example with K = 0.01, while perhaps not that different from K = 0.1, would help illustrate the proposed convergence of the solutions as K goes to 0.

An example with *K*=0.01 is shown in the following figure (as in Figure 3, the columns correspond to advance and retreat respectively):



This example supports our hypothesis that the saltwater-freshwater interface approaches the quasi-steady state for the most retreated grounding line position as K tends to 0.
Figure 3 is already large and fairly dense with information, and we do not believe that adding the above example with *K*=0.01 would add much to the figure which is not already seen in the *K*=0.1 example. Replacing the latter with the former is also undesirable because the different interface positions are easier to distinguish in the *K*=0.1 example. We therefore propose to include the above figure in the Supplementary Materials, with reference in the caption of Figure 3:
"...For full solutions see Supplementary Animations S1–S3. **An example for K=0.01 is shown in Supplementary Figure S1.**"
as well as in the main text at line 275
"...position. **(For an example with K = 0.01, which supports this hypothesis, see Supplementary Figure S1).**"

Line 275: Could you clarify this comment about "finite time" - at least at the surface of the aquifer, there is an instantaneous freshening/salting as the grounding line moves back and forth. Do you mean at depth? But then the seawater intrusion isn't instantaneous at depth either.

By "finite time", we mean that, during grounding line advance, the freshwater lens grows at a finite rate, determined by the flux of freshwater $-q_E$ into the aquifer from above. Therefore, it takes finite time for freshwater to penetrate to any given depth below the surface. In contrast, during grounding line retreat, the saturation of the region $x>x_g$ ahead of the grounding line throughout its depth is instantaneous. We have made a change of wording at line 275 to clarify this, following on from the comment added above:
"... see Supplementary Figure S1). This is because, during grounding line retreat, seawater instantaneously displaces any freshwater from the newly exposed region

***$x>x_g$*. However, during re-advance, the freshwater lens grows at a finite rate, determined by the infiltration *$q_E<0$*, and depending on *K*.** This is the key…"

Line 365: Why was K = 0.5 chosen as an example given that previous illustrations took powers of 10?

The value *K=0.5* was selected because it is one of the higher values of *K* which leads to an eventual 'pocket' steady state in the example of Figure 6. A higher value of *K* is favourable in this instance because it enables quicker relaxation to the steady state, allowing the final states to be compared as seen in Figure 6(d). Choosing e.g. *K*=0.1 would produce the same final state but would also require a longer relaxation time to do so, which would affect the readability of Figures 6(e) and 6(f). We have added a sentence to explain this at line 365:

"…parameter *K*. **In this case we select values of *K* that illustrate a difference in final states whilst being relatively fast to approach these states, for the sake of readability of the figure.**"

Line 376: Perhaps "more realistic" rather than "real-world" given the number of model simplifications

We agree that this wording is more appropriate and have made the suggested change.

Some floating "s at lines 527, 550, no closed bracket line 573

Thank you for pointing these errors out. We have corrected them.

<u>Reviewer 2</u>

1. Abstract: the last sentence is ambiguous and could be clarified. Also, it could be beneficial to zoom out and briefly state the significance of results.

We have decided to remove the last sentence of the abstract (at line 12) due to its ambiguity. We have also added a sentence at the end of the abstract discussing the significance of our results:
"…becomes deeper. **Our results highlight the potential importance of groundwater flow in sedimentary basins for the subglacial hydrology of ice streams.**"

2. Smith et al. (2020) is not a great reference for motivating contributions to future sea-level rise. What about a paper like Seroussi et al. (2020)? Or both?

We agree that when considering future contributions to sea level rise, a paper such as Seroussi et al. (2020) modelling future evolution of the Antarctic ice sheet is a better reference than the observations of recent historic sea level contributions by Smith et al. 2020. We have therefore changed the reference at line 15 accordingly.

3. line 20: could add 'potential' between 'important' and 'contributor', to reduce the certainty of the statement to a level comparable with the evidence.

We have made the suggested change of wording to reflect this.

4. line 100: could define effective pressure. It is implicit, but could be clarified.

We have modified the sentence at line 100 to clarify the definition of effective pressure: "...is equivalent to stipulating that the "effective pressure"**, defined as $p_e = \rho_i g H_i - p$,** is zero."

5. line 140: it could be valuable to explain a bit more about which grounding line position and aquifer thickness are good scales. Is it the initial value? Could be more clearly stated.

We have added additional wording at line 147 to clarify the basis for the choice of scales:
"We assume that **the present-day grounding line position, measured relative to the onset of the sedimentary basin (Peters et al., 2006), and the approximate aquifer thickness at the measurement sites of Gustafson et al., 2022,** provide suitable horizontal and vertical lengthscales *[x]* and *[z]*."

6. The nondimensionalization is a little hurried. I think specifying clearly the variables that are scaled and by what would be valuable. This is clearly needed since the first equations after the nondimensionalization have h and H in them. This is confusing if you just scaled the height H by H.

We have added more detail to our explanation of the non-dimensionalisation process, to clarify our use of notation and make clearer the variables that have been scaled and what they have been scaled with. This is found at line 151:
"We then scale

$$x, x_g \sim [x], \qquad z, h, H, b, s, S \sim [z], \qquad t \sim [t], \qquad p_S \sim \rho_f g[z], \qquad \boldsymbol{q_E \sim [z]/[t]}. \qquad (21)$$

For example, we let $x = [x]\,\hat{x}$, where $\hat{x}$ is non-dimensional, and do likewise for each of the variables listed with a corresponding scale in Equation (21) (i.e. $z = [z]\,\hat{z}$, $h = [z]\,\hat{h}$ etc.). We then work with these non-dimensional variables but drop all hats ˆ from the notation. From here onwards..."

7. Section 2.3: with zero effective pressure and a focus on ice streams, it is hard to imagine that the shallow-ice approximation is the right limit of the Stokes equations. There will likely be more than 'negligible bed slip'. At this stage in the review process, the best I can hope for is a clearer description of why this model was chosen, the drawbacks, and later in the paper, how it affects your results.

We agree that a version of the shallow-shelf approximation (SSA) provides a more appropriate model for the glaciological context, without affecting the simplicity of the ice sheet model. Specifically, using the SSA combined with a Weertman sliding law, neglecting extensional stresses, and assuming a prescribed grounding line position, results in a similar equation to that obtained under the SIA. We have rerun our code using this SSA model, and updated Figures 2–6 and 8–11 and Table 3 accordingly. The resulting modelled ice thickness is very similar to that previously obtained using the SIA, resulting in a small quantitative change to our results but no significant qualitative change.

Other choices of sliding law are possible, such as a regularised Coulomb sliding law (e.g. Schoof, 2005), but these typically involve some dependence on the effective pressure at the ice bed. Incorporating the effective pressure into the sliding law ultimately requires a model of shallow hydrology, which we consider beyond the scope of this paper. Indeed, our assumption of zero effective pressure is incompatible with several sliding laws (e.g. Budd, regularised Coulomb), which are singular at zero effective pressure.

We have updated our description of the ice sheet model to reflect this change:
"…by Eq. (7). **To model the ice sheet, we use the shallow-shelf approximation (MacAyeal, 1989; Schoof, 2007), assuming a Weertman sliding law with exponent 1/3 and that extensional stresses in the ice are negligible. We also assume that the accumulation is spatially uniform, and that the dynamics of the ice sheet are quasi-steady for a given x_g. This leads to equations of mass and momentum conservation**

$$H_i u_i = ax, \qquad \beta u_i^{1/3} = \rho_i g H_i \partial/\partial x\,(Hi + S),$$

where u_i is the (vertically uniform) ice velocity, a is the constant accumulation rate measured in m s^{-1}, and β is a constant in the sliding law measured in Pa m$^{-1/3}$ s$^{1/3}$. The latter equation represents a balance between the driving stress and the basal friction, which has a power-law dependence on the
velocity under the Weertman sliding law. These together lead to the dimensional equation

$$(\beta/\rho_i g)^3 H_i^4 \left(\partial/\partial x\,(H_i + S)\right)^3 = ax.$$

In dimensionless variables, after scaling Hi with [z], Equation (7) and (30) become

$$p_S = r_i H_i, \qquad H_i^4 \left(\partial/\partial x\,(H_i + S)\right)^3 = \alpha x,$$

where

$$ri = \rho_i/\rho_f \approx 0.917, \qquad \alpha = a\beta^3[x]^4 / (\rho_i g)^3[z]^7.$$"

Because we have removed the need for the Glen's law parameter *A*, and introduced a new sliding parameter *β*, we have also updated the references to *A* at Table 1 and at line 200 to reflect the new ice sheet model.

We have also added a discussion of why this ice sheet model has been selected at line 201:
"We have chosen the above ice sheet model because it is simple and quick to solve for a given grounding line position and is physically appropriate for an ice stream context, where fast ice flow is dominated by basal slip (Cuffey and Paterson, 2010). This is because our main purpose is to model the dynamics of groundwater, rather than those of the ice sheet. A more general model could replace the Weertman sliding law with other sliding laws. However, such sliding laws generally involve coupling to the effective pressure in the shallow

hydrological system, the modelling of which is beyond the scope of this paper. We include in Sect. 6 a discussion of the limitations of this model, and of how it might be generalised."

In the discussion section, at lines 514–518, we have expanded our discussion of how our ice sheet model might affect our results:

"**Such a model would also allow for coupling between the ice sheet model and subglacial hydrology. This could be achieved, for instance, by replacing the Weertman sliding law used in the ice sheet model above with a sliding law coupled to the effective pressure p_e (e.g. a regularised Coulomb sliding law (Schoof, 2005)). The use of such an ice sheet model would likely lead to ice that is thinner and shallower near the grounding line, leading to more saltwater intrusion in the steady state and a higher likelihood of trapping seawater in pockets, as well as leading to smaller exfiltration / infiltration. However, the same modelled ice sheet may be steeper inland, resulting in a larger $|q_E|$ further inland. It is also possible that feedbacks could exist between groundwater flow and the ice sheet. For example, groundwater exfiltration could lower the effective pressure in the shallow hydrological system, which would affect the sliding of the ice and hence the profile of the ice sheet, whose gradient feeds back into q_E. Since the SLW site is relatively near the grounding line at the present day, we expect that an ice sheet model which predicts shallower ice here would lead to a thinner freshwater lens, and hence a higher predicted permeability.** However, the results of Sect. 5 **suggest** that, when periodic grounding line movement prevents groundwater dynamics from reaching a steady state, the shape of the ice sheet **is less important than** the permeability and geometry of the sedimentary basin."

8. figure 2: does the solution become singular at x = 0?

We have added a sentence at line 231:

"...balancing one another in the steady state. **The infiltration $q_E<0$ is large in magnitude but finite at $x=0$, because the ice overpressure gradient $\partial P_S/\partial x$ is zero at $x=0$ whilst changing over a relatively short distance.**"

9. paragraph at line 270: I think the relationship between qE and K could be clarified with a figure.

Since the aim of Figures 3(a–c)(iii) and (iv) is to illustrate how q_E is affected by K, we do not think that another figure would add much in the way of illustrating this point. We have however made changes to the wording of this paragraph to emphasise the link to these figures and the equation q_E:

"The exfiltration flux q_E also depends strongly on K, as shown in Figures 3(a)–(c)(ii). **Equation (27) shows that q_E is multiplied through by a factor of K, but also depends on the position of the interface s, which is itself affected by K.** When K is large **(Figures 3(a)(iii) and (iv)), q_E therefore follows s in being close to its** instantaneous quasi-steady state value, resembling that shown in Figure 2(b)(i). However, when K is small **(Figures 3(c)(iii) and (iv))**, exfiltration **is small apart from** a prominent peak near x_g during grounding line retreat, where freshwater is rapidly flushed out of the aquifer **as**

seawater saturates the region beyond the grounding line. Seawater intrusion **could therefore significantly influence the shallow hydrological system via its effect on** q_E, as we shall see again in Sect. 5."

10. paragraph at line 360: it could be valuable, given the venue at The Cryosphere, to describe some of the implications of the hysteresis.

We have added more discussion of this hysteresis at line 373:
"...possible steady states. **This hysteresis would affect the exfiltration q_E into the shallow hydrological system, because the presence of a saltwater pocket influences q_E (e.g. as seen in Figure 4). This in turn would modify the distribution of water in the shallow hydrological system beneath an ice sheet and could therefore feed back on the dynamics of the ice via basal sliding."**

11. section 5: I think this part of the paper could be its own paper. That would allow for more discussion of the results in all sections. Currently, the text continues to be hurried.

We believe that keeping Section 5 works better when combined with the rest of this paper, since the analysis of the model in idealised scenarios in Sections 3 and 4 helps provide insight into the results of the more complex experiment in Section 5, and the results of Section 5 are still obtained under a number of idealising assumptions that are discussed in the paper. It would be interesting to conduct an experiment similar to that in Section 5 using a more complex model which relaxes a number of these assumptions, which we leave for a future study.
We believe that we have addressed the need for more discussion via our responses to the other reviewer comments.

12. How does this model compare to the SLW salinity measurement? It seems like text would be devoted to this point – did I miss it?

The paragraph at lines 443–447 discusses the results for the present-day freshwater lens depth, provided in Table 3, and compares them to the salinity results of Gustafson et al. (2022). We have made changes to this paragraph to improve the discussion of this point:

Table 3 shows the present-day depth of the transient freshwater lens $\boldsymbol{d_{f,SLW}}$ at the location of SLW in the above solutions. This increases with the permeability k, reaching the full depth H of the layer for the very high value of 1×10^{-10} m^2, where the solution is near quasi-steady. **Modelled salinity profiles found by inversion of magnetotelluric data by Gustafson et al. (2022) suggest that the groundwater salinity reaches that of seawater around 600m below the ice bed, and 50% of this salinity about 400m below the ice bed. These profiles show a smooth increase in salinity as a result of either mixing of freshwater and saltwater or due to the inversion process itself, as opposed to our results obtained assuming a sharp freshwater-saltwater interface. This introduces some uncertainty in how best to infer the 'freshwater lens depth' from these results. However, it is clear that the high permeability value $k = 3 \times 10^{-12}$**

m^2, for which $d_{f,SLW}$ = 400 m, gives the results that can be considered to agree best with those of Gustafson et al. (2022). We discuss how this model might be adapted to allow smoothly varying salinity in Sect. 6.

13. I like the conclusions section, it is a nice wrap up of the paper, much like an expanded discussion section. The paper would benefit from more discussion generally.

As above, we believe that we have addressed the need for further discussion via our responses to the other reviewer comments.

<u>Reviewer 3 (Lu Li)</u>

This paper addresses an important problem related to understanding long-term groundwater flow in sedimentary basins beneath marine ice sheets. It is technically sound and represents a valuable contribution to the field. Thanks for this interesting work. I really enjoyed reading it. The manuscript has undergone thorough review by several referees and community members, and the authors have responded effectively to the comments. As a result, the paper has been significantly improved and is now ready for publication. A few minor edits related to the geophysical aspects could be made in the final version.

Much of our understanding of the geometry and properties of sedimentary basins (e.g., salinity) comes from geophysical data. It would be useful to distinguish the direct observation and model product. Sedimentary basin thickness can be modeled using magnetic data: the magnetic field is directly observed, while the basin thickness is a model product targeting the magnetic basement (assume sediment is non-magnetic). Similar to the approach used in Gustafson's work, magnetotellurics (MT) does not measure salinity directly. Instead, it measures electric and magnetic fields, which are used to invert for electrical conductivity. Salinity is then estimated from the conductivity results using Archie's empirical law.

It is somewhat difficult to determine whether there is a smooth transition from freshwater to saline water, as the inversion method used in Gustafson et al. (2022) is designed to produce smooth models that fit the data. It would be good to know the limitations of using geophysics (especially when it is used to constrain the numerical model!). Here is a review on the use of geophysics to investigate sedimentary basins in Antarctica (see Aitken et al., 2023), which hopes to help bridge the geophysical and modeling communities.

We have modified our discussion at lines 75–80, 443–447 and 551–559 of the smooth variation in salinity, to include the fact that the observed smoothness may be in part a result of the inversion method used – see response to Reviewers 1 and 2 above.

Below are several suggestions:

Line 22: 'Airborne magnetic measurements' to 'Airborne magnetic investigation'. Maybe *also add a sentence in the beginning to lead this paragraph: Geophysical data provide constraints on sedimentary basin geometries and properties (Aitken et al., 2023).*

We have made the suggested changes to introduce these geophysical methods, to refer to the suggested literature, and to more accurately describe the nature of these methods:

"**Geophysical data can provide constraints on the geometry and properties of Antarctic sedimentary basins (Aitken et al., 2023; Li and Aitken, 2024). For example, airborne magnetic investigations** of the sedimentary…"

Line 77: 'the measurements of Gustafson' to 'the inversion results of Gustafson'
Line 386: 'come from airborne magnetic measurements by Tankersley et al. (2022)' to 'come from Tankersley et al. (2022) which is derived from airborne magnetic data'

We also have made these changes of wording.

Line 387: We cannot direct measure bathymetry from satellite, the primary source of bathymetry from Bedmap2 is active seismic sounding. Just would like to point out that there is a major update of bathymetry beneath the Ross ice shelf using airborne gravity data (Tinto et al., 2019), that's a better option if you want to some future work, or you can use Bedmap3 (Pritchard et al., 2025). You can just say we use the bedrock topography and bathymetry data for S(x) from Fretwell et al., (2013).

We have made the suggested change of wording at line 387 to correct this.

Line 388: It's okay to do interpolate the basement geometry in here. Just want to point out there is a first order of sedimentary basin thickness product available (Li and Aitken, 2024), if some future work would like to do the simulation in other areas in Antarctica.

We have added reference to this research in the introduction (see above).

Line 392: 'measured' to 'modelled'
Line 552: 'for the smoothly varying salinity observed in the measurements of Gustafson et al. (2022)' to 'for the smoothly varying salinity modelled by Gustafson et al. (2022)'

We have also made these suggested changes of wording.

<u>Other corrections:</u>

We have corrected the values of k, φ and K in Tables 1 and 2 to those used to produce results in Section 5. A typo has also been corrected at line 451. Small changes of notation have been made at lines 436 and 444 for consistency.

<u>References:</u>

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