Dear Dr McCormack,

Thank you for inviting us to submit a revised version of our manuscript. Please find below a point-by-point summary of the revisions we have made in response to reviewer recommendations, and other comments. Page and line numbers refer to the marked-up version of the manuscript.

Reviewer 1:

"The strength of the work is a simple and elegant mathematical design. There are, however some limitations, notably the use of the shallow ice approximation, which is a poor choice in the Siple Coast test case because the fast motion of glaciers there is almost exclusively caused by basal slip. The authors offer a discussion of other model limitations, but not this one. I doubt the model reproduces the actual geometry of the Siple Coast, but this is perhaps not so important, given the "first-order" nature of the study more generally."

This is true. The focus of this study was intended to be on the groundwater dynamics rather than the mechanics of the ice itself (we are currently working on a coupled model that includes a more sophisticated model for the latter). We required an ice sheet geometry for different grounding-line positions, and we selected the shallow ice model for the sake of simplicity. For the periodically advancing and retreating ice sheet considered in our study, where the groundwater dynamics are far from a steady state, the precise shape of the ice sheet has little effect on the freshwater lens or exfiltration rate compared to the permeability and geometry of the sedimentary basin. We therefore select the shallow ice model as it is the simplest ice sheet model (having a single parameter, the dimensionless accumulation) that can be solved subject to a flotation condition at the grounding line and no surface slope at the origin. While other approximate models (notably a 'shallow-shelf approximation 'with large basal slip) are possible, they really ought to be coupled back to subglacial hydrology, and that complication is beyond the scope of this study.

With this said, we agree that the manuscript would benefit from a fuller discussion of the use of this model. We have revised the manuscript to include a discussion of this rationale, by including at page 8, line 185
"… rate. **We use this model because it is very straightforward to solve and introduces minimal additional physics to the model.** We also assume…".
We have also added at page 26, line 514
"… subglacial hydrology. **Such a model would also permit the use of a more sophisticated ice sheet model than the shallow ice approximation used in this paper, such as a shallow shelf model (e.g. Morland (1987)) with a basal sliding law coupled to subglacial hydrology. However, the results of Sect. 5 indicate that, when periodic grounding line movement prevents groundwater dynamics from reaching a steady state, the precise shape of the ice sheet has little effect on groundwater flow compared to the permeability and geometry of the sedimentary basin. The resulting model uncertainty is therefore small compared to that resulting from e.g. aquifer heterogeneity, or lateral variations in basement geometry.**"

"The main goal is to provide a long-term perspective of freshwater lenses and trapped subglacial seawater. However, the exclusion of vertical pressure gradients is a significant limitation because past work have shown groundwater flows in Antarctica to be quite sensitive to those. The manuscript includes a discussion with references to the inferred hydrological budget of ice streams at the Siple Coast, but previous work has also modelled the vertical exchange. To give an example, Christoffersen and Tulaczyk (Annals of Glaciology, 2003) included glacial-interglacial simulations of groundwater exchange at the Siple Coast. There may be a relevant discussion in that thermally driven exfiltration is shallow compared to the horizontally driven exchange presented in this manuscript."

While our manuscript does discuss the neglect of non-hydrostatic vertical pressure gradients and consequent exfiltration arising due to the dynamic response of sediments to loading, we have not considered those due to basal freeze-on. However, we may justify their neglect on the grounds that these vertical pressure gradients typically exist only within the upper few tens of metres of till. Therefore, while this exfiltration is important for calculating basal water budgets, it is less important when modelling the dynamics of groundwater throughout the full depth of a sedimentary basin, which is the focus of our study.

We have added a discussion of this at page 26, line 544
"… the same k. **We have also neglected basal freeze-on, which could drive substantial exfiltration of water from sediments over the timescales of glacial advance and retreat, considered for instance by Christoffersen and Tulaczyk (2003b). However, the dynamic effects of this exfiltration are typically confined to the upper 5–50 m of sediment, meaning that the corresponding pressure gradients do not substantially affect the overall flow of groundwater throughout the depth of the ~1 km thick sedimentary basin. Such exfiltration could be considered in a possible extension of this model that includes heat transport, as discussed above**."

"A final couple of questions. Why not use reconstructed air temperature and precipitation records from Antarctic ice cores instead of a periodic function? Presumably, this would provide more direct evaluation of glacial-interglacial changes. "

As with the use of the shallow ice approximation, we have chosen a mathematically simple description of historic grounding line position. We have done so on the basis that a more complex model introduces so many other sources of uncertainty (e.g. converting from air temperature to grounding-line position) that we think it makes for a cleaner experiment to simply prescribe the evolution of the groundling line position. For the purposes of this paper, it is most important to accurately capture the approximate position and timing of the grounding line minimum, since this determines where and when the freshwater lens must displace intruded seawater.

While it would be possible to obtain a grounding line position by forcing an ice sheet model using real-world ice core data, this requires introducing additional physics (e.g. in modelling the ice flux across the grounding line) and may need heavy fine-tuning to recover the best existing estimates of the grounding line minimum. In addition, a numerical model forced using the existing ice core record up to the present day may exhibit dependence on the arbitrary initial condition imposed, whereas a periodic forcing ensures the existence of a periodic solution.

We have made a revision on page 19, lines 405-410:
"We choose to use **these** simplified model**s** on the basis that the exact form**s** of p_S **and** $\boldsymbol{x_g}$ **are** a relatively small source of model uncertainty compared to e.g. lateral variations in H and b, or heterogeneity in aquifer properties such as k and ϕ. **We have therefore prioritised capturing appropriate values for the grounding line maximum and minimum, and their timing. A more sophisticated approach could use ice core data for historic accumulation and temperature to force a model of both** $\boldsymbol{p_S}$ **and** $\boldsymbol{x_g}$**, but such a model may require excessive fine-tuning to reproduce existing estimates of the grounding line extrema.**"

"Also, how sensitive is the exchange of water at the top of the aquifer to the assumed impermeable basement? What if the basement wasn't impermeable?"

This is an interesting consideration. Our assumption that the basement is impermeable is based on the measurements of Gustafson et al. (2022), which show a significant increase in electrical resistivity below a certain depth, indicating that very little if any groundwater resides in the basement rock. Although this measurement is localised, Tankersley et al. (2022) likewise assume that the permeability of the basement is very low, and that this low permeability is key in confining groundwater flow.
It is an interesting possibility to consider a model in which a different boundary condition, such as a nonzero flux of water, is imposed at the basement. However, such a condition would be somewhat arbitrary in nature as little is known about the basement rock beyond its upper extent and low permeability. If the permeability of the basement rock were significant on the timescales of our consideration, the modelled exfiltration during basin shallowing and infiltration during deepening would be weakened, since some outflux would occur into the basement. However, assuming that the permeability of the basement is low, this effect would be small, and further mitigated if a decrease in basin permeability with depth due to sediment compaction were factored in.
The position of the basement is important for the results of the model, and it is worth noting that this is itself subject to uncertainty. This includes model uncertainty in the inversion of magnetic data by Tankersley et al. (2022), but also the uncertainty introduced by our taking a two-dimensional cross section of this data.

We have made a revision to discuss this at page 25, line 519, following on from the revision mentioned above:
"... subglacial hydrology. **Such a model ... basement geometry.**
Since basement geometry is important for the results of the model, particularly $\boldsymbol{q_E}$**, it should be noted that the use of a cross-sectional model introduces uncertainty even after transverse averaging. Moreover, the data of Tankersley et al. (2022) includes some model uncertainty introduced when inverting magnetic measurements. Future work is therefore required to explore the dynamics of**

subglacial groundwater flow in all 3 dimensions, which we have discussed in Appendix C. We have assumed, following Gustafson et al. 2022 and Tankersley et al. 2022, that the basement may be treated as impermeable, although an extension of this model could include a basement which is weakly permeable. The inclusion of basement permeability would weaken the effects of basement geometry on q_E by providing an alternative route for groundwater to leave or re-enter the sedimentary basin."

Reviewer 2:

I appreciate the layer of water between the ice and the aquifer is thin, but a discussion about typical length scales and modelling assumptions would be useful. To aid discussion, it would perhaps be useful to add a new symbol to denote the underside of the ice, as it is also being referred to in the schematic of Fig. 1. That is, the caption refers to the layer of water between the ice (at z=?) and the aquifer (at z=S).

The thin layer of water is representative of a 'shallow hydrological system 'including (e.g.) subglacial channels, lakes and till, whose depth is altogether on the order of metres to tens of metres. In contrast, the thickness of the sedimentary basin ranges from hundreds to thousands of metres. We therefore make a modelling assumption that the shallow layer of water has zero thickness. For this reason, we think that adding a new symbol to denote the underside of the ice would be more confusing than helpful, as the model makes no real distinction between this and the upper surface of the aquifer. We have added a sentence to better explain this at line 64:
"... hydrological system. **We treat this layer in the model as having zero thickness, on the basis that the corresponding 'shallow 'hydrological system has a depth on the order of metres to tens of metres, whereas the aquifer is hundreds to thousands of metres deep (Gustafson et al., 2022).**"

Although it is part of the definition of an aquifer, I would suggest to add "permeable" before 'aquifer" on line 53, for clarity.

We have implemented this suggestion.

I understand ice is assumed to deform but the sediment is not. Can something more be said about when such an approximation is valid?

We regard the aquifer as rigid sedimentary rock, and assume that any sediment deformation is confined to the shallower layer above (discussed above) where it could facilitate 'sliding'. We believe that this is a sensible assumption at first order on the timescales of our consideration. This assumption does not extend to the till comprising the upper few metres of the sedimentary basin, which we treat separately as part of the 'shallow 'hydrological system, and which is likely to deform substantially in response to ice and groundwater dynamics. We have added a sentence at line 73 to clarify the latter:

"exfiltration $q_E(x,t)$. **This assumption of rigidity does not extend to the deformable till comprising the upper few metres of the sedimentary basin, which we regard as part of the basal hydrological system."**

I understand that the frequently used sharp-interface approximation, following other works, has been applied. Can a comment be made about how sharp the boundary between the seawater and the aquifer ahead of the grounding line is in practice?

The salinity profiles obtained by Gustafson et al. (2022) suggest that, in reality, the salinity varies smoothly from fresh to saline throughout the aquifer rather than displaying a distinct sharp interface. Our conclusions involve discussion of the limitations of the sharp-interface assumption, and possible approaches to accounting for mixing of saltwater and freshwater (lines 550–558). We have made a revision to refer to this discussion earlier on at line 77:
"is negligible (Bear, 2013; Mondal et al., 2019). **Since the measurements 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. For now, however, we proceed with the sharp-interface assumption as a useful and tractable first-order model.** The aquifer therefore..."

How has (5) been obtained? The +/- subscripts are hard to follow.

Equation (5) states that the velocity of the interface is equal to the Darcy velocity of the water on either side thereof, ensuring that mass is conserved across the interface. We have made a revision to better explain this at line 100:
"... where the subscripts ± **denote the Darcy flux components on either side of the interface, which are not continuous in general**. However, the pressure $p(x,z,t)$ must be continuous. **Equation (5) ensures that the velocity of the interface equals the Darcy velocity of the fluid on either side, so that mass is conserved across the interface."**

The switch between dimensional and dimensionless variables is hard to follow. Presumably the equations are in dimensionless form but the figures are dimensional and still use the same notation for all the variables?

This is correct: from Sect 2.2 onwards, variables are dimensionless unless otherwise stated, but figures are dimensional. We make this choice for readability and ease of interpretation, rather than introducing additional notation to distinguish dimensionless and dimensional variables. We have added a sentence to explain this at line 151:
"**From here onwards, variables are dimensionless unless otherwise stated, but dimensional values are used in figures, based on the scalings provided in Tables 1 and 2 unless otherwise stated.** Under these..."

I was surprised that the depth of the aquifer is of a similar vertical length scale to ice depth. This would be worth pointing out specifically, perhaps when the nondimensionalisation is made.

We agree that this reuse of the vertical length scale is worth pointing out, and have made a revision to do so at line 148:
"…and [z]. **Since both the aquifer and ice sheet are is hundreds to thousands of metres deep, we may use the latter for both the aquifer and ice sheet.** We also assume…"

Fig 2: What about the streamlines within the freshwater-saturated aquifer is more interesting than those in the saltwater-saturated aquifer?

In the steady state, there is no flow in the saltwater-saturated region, and hence there are no streamlines to be plotted in this region. We have added a sentence stating this explicitly at line 231:
"… steady state. **There is no flow in the saltwater region in the steady state."**

Fig 3: Are the steady state solutions those obtained with x_g(t)=\bar x_g? I can infer that from line 248, but it would be clearer to state this explicitly.

This is correct. We have added a sentence to clarify this in the caption of Figure 3:
"… (low permeability). **Instantaneous steady states are plotted for x_g= \bar x_g, \bar x_g $\pm \Delta x_g$.** Parameters are…"

How can saltwater get to a saltwater pocket in steady state? Does this follow some kind of time-dependent forcing of the grounding line? This isn't made clear at the beginning of Sec. 4, only becoming clearer later on.

Generally speaking, in order for a saltwater pocket to form, the groundwater flow must evolve from an 'initial condition 'where saltwater is present sufficiently far into the aquifer to become trapped in the pocket, such as the examples provided in Figure 5. This is not unreasonable, as without an ice sheet the entire aquifer would be saltwater-saturated. However, in practice, there is always a grounding line history which determines this 'initial condition'; moreover, the 'initial condition 'and subsequent pocket formation are highly sensitive to this grounding line history, as seen in Figure 6.

We have added a brief explanation of this at line 288:
"…Figure 4. **Such a pocket may be formed, for example, if the ice has advanced sufficiently rapidly to its current position that saltwater is still present relatively far inland once the ice has halted (see later Figures 5 and 6).** These pockets…"

Figure 4: A label describing the basal geometry (in the legend) would be useful.

We have added a description of the basal geometry in the caption on Figure 4:
"…illustrated. **The geometry is given by S = -1000 m, b = (-2500 + 1000 exp(-((x- 125 km) / 12.5 km)^2)) m.** (b) Freshwater…"

Figure 8: I found it hard to interpret the two black curves as they're unlabelled (why is it two, not one, for examples). It also took some digging to understand where the basement geometry is coming from. Perhaps state the location explicitly here.

The black curves are the upper (*z=S*) and lower (*z=b*) surface of the aquifer, which is consistent with previous figures. These are estimated by taking an averaged cross-section along a historic flowline of the ice, as described in the previous Figure 7. We have modified the caption of Figure 8 to make this clearer:
"Potential steady states for the basement geometry ***z=b* and *z=S* (black curves) estimated along a historic flowline of the ice in the Ross Sea, West Antarctica, as shown in Figure 7. The freshwater-saltwater interface *z=s* and ice surface are plotted** for various values of the ice sheet parameter α."

- *Line 18: Add commas before and after "which drain much of West Antarctica".*
- *1 Caption, final line: Change "z= S" to "z= S(x)".*
- *2 and 4: It is technically incorrect to refer to the cyan line as S(x)+H_i(x) (there's an ice shelf there too), so I suggest to just refer to it as the ice surface, which would be consistent with Figs. 3 and 5.*

We have implemented all of these suggested changes.

<u>**Comment by Giacomo Medici**</u>

"Line 6. "Two-dimensional groundwater flow". Add text in the discussion section on assumptions and limitations underneath the choice of a 2D model."
We have added a discussion of the limitations introduced due to the choice of a two-dimensional model as part of our responses to reviewer comments above. The chief source of uncertainty is the inability of a two-dimensional model to account for lateral variations in sedimentary basin geometry. This discussion is found at lines 513–523 of the marked-up manuscript.

"Lines 30-34. Add relevant and recent literature on tracer and hydraulic tests in sedimentary deposits of glacial origin made by clay, sand, breccias and conglomerates:
- Tracking flowpaths in a complex karst system through tracer test and hydrogeochemical monitoring: Implications for groundwater protection (Gran Sasso, Italy). Heliyon, 10(2).
- Forms of hydraulic fractures created during a field test in overconsolidated glacial drift. Quarterly Journal of Engineering Geology and Hydrogeology, 28(1), 23-35."

We are aware that there is extensive literature on the various methods used to investigate groundwater flow and hydraulic properties of aquifers, of which the suggested papers are a good example. However, we have limited the focus of our study to groundwater flow beneath current marine ice sheets, so that the aquifer is either covered by ice or ocean at all times. Aquifers which are exposed to the air (e.g. following retreat of a land-terminating ice sheet) are subject to different physics (for example, recharge is determined by precipitation rates rather than the imposed pressure at the ice bed). We therefore exclude such aquifers, which comprise the majority of well-studied glacial deposits including those referred to in the suggested papers, from our study. As a result, we do not believe that the suggested literature is directly relevant to the focus of our paper, although it would be interesting to incorporate such research into future generalisations of this model to a broader class of settings.

[Add]

"Line 510. "Complex model" to develop in the future. Do you mean a model with multiple units to account for the heterogeneities of the system?
Line 510. "Complex model" do you also mean more attention on the anisotropies? You mention heterogeneities in the manuscript, but not anisotropies"

In this sentence a "more complex model" refers to separately modelling the dynamics of the shallow hydrological system. We have changed the wording at line 510 to help to clarify this:
"A more complex model **that relates** q_E and p_e by **separately** accounting for the dynamics of the shallow hydrological layer, and **that considers** both freshwater and saltwater, would provide ..."

We have also made changes to include discussion of anisotropy (as distinct from heterogeneity) in the permeability *k* as a source of uncertainty at the following:
Line 413: "This includes model uncertainty (e.g. possible spatial heterogeneity in ϕ and k, **dependence of k on ϕ, and possible anisotropy in *k*)**"
Line 484: "Our chosen value of *k* represents an idealisation of a sedimentary aquifer which may in reality be highly heterogeneous **and anisotropic."**
Line 518: "The resulting model uncertainty is therefore small compared to that resulting from e.g. aquifer heterogeneity **and anisotropy**..."
Line 627: "When the porosity ϕ (x,z) and permeability k(x,z) are heterogeneous **(but k remains isotropic)**,..."

"Lines 655-755. Add the recent literature suggested above on the glacial environment."
As discussed above, we do not believe that the suggested literature is directly relevant to the strict focus of our study.

"Figure 1. Do you need an approximate spatial scale for your conceptual model?"
Since the horizontal and vertical lengthscales are discussed elsewhere (e.g. at line 66, or later at Table 1), we think that adding a spatial scale to Figure 1 would be unnecessary and result in a more cluttered Figure. We have therefore opted to keep this Figure unchanged.

"Figure 3. Very busy figure, consider to split it in two parts."
After consideration we believe that the current format is best, as it is helpful to see advance and retreat together to appreciate the periodicity of the solution. It is also helpful to directly compare the solutions for different K, as the main purpose of the Figure is to illustrate the difference and resulting saltwater trapping. Finally, depicting q_E directly below the timeseries of s(x,t) gives some idea of how the flow behaves, and is consistent with the previous Figure 2 and following Figure 4.

"Figure 6. There is room to make the figure larger."
"Figure 11. Same here, there is room to make the figure larger. The figure would benefit from that."

We have made both of these figures larger in response to this feedback.

<u>Comment by Matthew Tankersley:</u>

"I noticed a few small errors in the citation of my work which I thought I would inform you of. The sedimentary basins we discussed were modeled with airborne magnetics data, not imaged with radar data as mentioned in the text. I think this is an important distinction as the modeling aspect, as opposed to direct imaging, introduces a lot more uncertainty which your readers should be aware of, and magnetic and radar techniques are quite different. No worries, but if you're able to change your mentions of radar to magnetics that would be great."

We have corrected references to radar measurement in the context of the paper by Tankersley et al. (2022) to refer instead to modelling using airborne magnetic data. These are found at lines 22, 378, and 385 of the marked up-manuscript.

<u>Other corrections:</u>

A typo in Equation (19) (an incorrect leading minus sign) has been removed.

Grammar has been improved at lines 286 and 560, and a typo has been corrected at line 303.