Reviewer 3:

This is an interesting study that fills a niche by including a representation of subglacial channel drainage in an idealized model of a marine-terminating ice sheet. I have only minor comments for the authors (please see also the annotated pdf) that I hope will help set readers' expectations at the onset, mostly related to (1) the scope of model application including assumed geometry and (2) the rationale for choosing a single channel to describe subglacial drainage morphology. A few additional sentences in the manuscript could also be used to speculate/comment on the extent to which key results in the paper are a function of channelized drainage versus coupled drainage of any type (3).

Thank you for this comprehensive and constructive review. Our responses to the comments in grey are in black below.

1. Scope of application of model:

a. Please clarify in the abstract/intro the scope of application of the model. Is it meant to represent a marine ice sheet (i.e., Antarctica) and/or any marine-terminating glacier (e.g., Greenland outlet glaciers and other tidewater glaciers)?
It is meant to represent a marine ice sheet, with more relevance to those in Antarctica, which tend to have less seasonal changes in their subglacial hydrology environments. We will specify this in our abstract: "We develop a coupled ice–subglacial-hydrology model to investigate the effects of coupling on the long-term evolution of marine-terminating ice sheets."

b. Bed slopes are partly what define, in some conceptions at least, marine ice sheets and tidewater glaciers. A linear bed geometry is mentioned for the first time around Line 195 (unless I missed an earlier mention) and just for a subset of the model runs. Overdeepened bed geometry characteristic of marine ice sheets is not mentioned until after Line 225 (page 20). Establishing the range of geometries used early in the paper will complement the clarification in (a).
We are using the terms "marine ice-sheet" and "marine-terminating" strictly to refer to ice that is grounded below sea level and which flows into the ocean. Under this definition these terms refer equally to prograde and retrograde bed slopes.
We will add "For b we use either a linear prograde slope or an overdeepened bed (Section 2.5)." when introducing the bed variable b, at line 90.

c. Does the model apply to locations where surface melt is an important source to the glacier bed? The governing equations suggest yes, while the scaling (hydrology timescale ~ months) suggests no. Readers could work out whether the prescribed supply term listed in Table 2 is consistent with surface melt, but it would just be easier if the paper made a statement to this effect instead.

Our model parameterized all sources of water with the additional melt term, regardless of if it is from surface melt. Our scaling for the hydrology timescale was determined by the internal dynamics of the channel, so it is not tied with surface melt which motivates a shorter timescale. In our discussion where we discuss limitations, we will mention how our time scale is derived from channel properties rather than external forcing time scales that may be related to surface melt. At around line 460, we say: “we assumed a pseudo-steady-state in the hydrology component of the model in numerical experiments. This was motivated by a scaling analysis which used the properties of the coupled ice-hydrology system (in particular, x_0, h_0 and Q_0) to derive time scales of the hydrology system, rather than the time scale of external forcings. Therefore, the pseudo-steady-state assumption would not apply if the timescales of, for example, meltwater input to the system were shorter, perhaps due to fluctuations in the flux of surface meltwater reaching the bed.”

2. Conceptual model of subglacial drainage:
   a. Please justify adoption of channel-only hydrology approach further than the sentence around Line 60 where channels are said to exist.
      To better explain our motivation for using a channel model only, we will replace the sentence starting on 59 with “For simplicity, we do not model an adjacent distributed drainage system, which would provide meltwater to the channel and influence basal sliding. This approach aids in interpretation of the physics of coupling and is supported by previous modelling suggesting that the pressures in channels and adjacent distributed cavities are closely coupled (Dow et al., 2022; Kingslake and Ng, 2013).”

   b. The channel is assumed to exist all the way from the ice divide to the grounding line, hence temperate bed assumptions are implicit. How realistic are these requirements for the domains envisioned in Greenland and Antarctica?
      This is an unrealistic assumption as at some point away from the grounding line the bed will likely be frozen and/or the discharge would be so small that channels would not persist, so the channel would not exist all the way to the divide. This simplification was for convenience, and we will emphasize that when we describe the channel setup (line 128). Adjusting the model setup so that the channel initiates some distance from the divide would have a similar effect on the results as reducing Q_in or M; we examine the sensitivity to M in our sensitivity study. We will add: “We assume that the channel extends to the divide. To avoid this simplification causing the channel to become unrealistically large near the divide, we impose a very small Q_in.”

   c. Why the choice to employ a single straight channel to represent the entire drainage system when more realistic and only slightly more sophisticated options exist (and have been developed by one of the coauthors)?
      The motivation is simplicity. Neglecting a distributed component avoids introducing several poorly constrained parameters and allows us to fully identify and explain the physics underlying the model’s behaviour. One disadvantage of this approach is that it requires us to assume the channel effective pressure is representative of the effective pressure of a wide-enough portion of the bed that it impacts basal sliding. We consider this a reasonable compromise, in the interest of simplicity; i.e. including a cavity system would avoid this assumption, but it would make it more complicated to clearly explain the

model's behaviour. We prefer the approach of thoroughly explaining the behaviour of a simpler model that admittedly lacks some potentially important components (just like all models), and then moving forward to include more complexity, which will be pursued in future work.

d. Is the pressure-melting term included in channel governing equations. If not, why not? Either way, what are the implications of including/neglecting this term on the simulations with adverse bed slopes?
It is not included in the model. While the grounding line is on a reverse bed slope, one would expect the inclusion of the pressure melting term to slightly suppress channel size upstream of the grounding line because melt rates would be reduced (or if the pressure gradient were sufficient in magnitude, freeze on would occur). Consequently, one might expect a reduction in channel size whenever the water pressure gradient is high. Such locations can be seen in the middle column of panels in Figure 5, where the yellow curves reach a maximum. Whether or not this reduction in melting (or onset of freeze-on) would make a significant impact on the results is unclear, but future work could include this term and examine this question.

3. Results and implications:
   a. A local peak in effective pressure upstream of the grounding line, appearing in both one and two-way coupled simulations, is a result highlighted in this paper. To what extent is this a reflection of the assumed drainage-system morphology (i.e., a channel), which produces high steady-state effective pressure at high discharge? The authors note that a similar peak in effective pressure was observed in GlaDS (distributed and channelized) simulations in another study, but was this a consequence of channels dominating the drainage in this region? In other words, is a peak in N upstream of the grounding line possible in the absence of channelized/channel-dominated drainage? If not, observed peaks in N upstream of grounding lines might be an interesting basis for inference on the subglacial drainage system, provided other causes (e.g., bed roughness) could be ruled out.
   Our results cannot answer the important question of whether or not a channel is required for the peak in effective pressure to form. Future work should examine this. However, based on our findings, we hypothesize below that you would indeed see a peak in effective pressure upstream of the grounding line in the case of a distributed cavity system.

   A key difference between models of channels and models of distributed cavities is the drainage-system opening term (Eq. 7). In channels this opening is caused by melting, which increases the flux and the total potential gradient, and in cavities opening is additionally from sliding over basal topography, which decreases with effective pressure. A key component of our model solutions that leads to the peak in effective pressure is the large size of the channel near the grounding line. Cavities should also be expected to grow large near the terminus because closure is reduced due to low effective pressure (as it is in channels) and opening is increased due to low effective pressure. Therefore, because other aspects of channel and cavity dynamics are similar (e.g., flux increasing with drainage-system size and total hydraulic gradient), we expect this to lead to a peak in effective pressure similar to those found in this paper. However, because this is speculative and requires examination with a model that includes cavities, we do not include discussion of this in the paper.

b. Given the key result that a dynamically coupled channel produces grounding line retreat whereas a static treatment of effective pressure does not, I'm left wondering if this is a general feature of introducing coupled hydrology, or whether this is a function of the hydrology being a channel with its characteristic relationship between discharge and effective pressure in steady state. A conceptually simple test would be to change up the drainage morphology (from a channel to a sheet-like/diffusive system), but that seems like an unreasonable amount of extra work for one paper. Instead, perhaps the authors can comment, from their experience modelling both drainage system types, on whether the results would differ qualitatively with a different assumed drainage system morphology.
We agree this is an important next step. We hypothesise on the results in our response immediately above.

Specific comments:
Title: Could title be more precise, e.g., does any coupling to any form of hydrology model enhance retreat? Is two-way coupling required? Is representation of channelized drainage required? I more precise declarative title would be useful.
We have modified our title to "Two-way coupling between ice flow and channelized subglacial drainage enhances modeled marine ice-sheet retreat".

4-5: single channel? channelized system? specify dimension here: 1D profile or 2D plan view?
We will specify the dimensions in the abstract as a 1D profile.

13: This is a little confusing since only steady state hydrological variables were mentioned above.
We will eliminate the reference to steady-state profiles in the abstract and instead just refer to profiles, as the variables described also follow the same profile shapes during the transient experiments.

28: True enough, but these refs relate largely to temperate alpine glaciers or Greenland, whereas this paper focuses on the Antarctic ice sheet by virtue of its focus on marine ice sheets (I think?). Would be preferable to include more relevant examples (Clarke ok).
We will also include Alley et al. (1994) and Stearns et al. (2008) as more relevant sources for Antarctic ice streams.

48: Nice framing of this work in context of what is same as/different than other studies. Might be worth citing Hoffman and Price for early exploration of two-way coupling, though in the H&P case the two-way coupling pertained to the cavity/distributed system instead of a channel.

We have cited Hoffman and Price (2014) here.

51: Ok, but unsatisfying without justification. Please add a few sentences of motivation for this approach (with citations) for the domain of interest. Abstract led me to think the application was Antarctica (marine ice sheet), but intro then refers to marine-terminating glaciers, which made me think of Greenland. Would help to know precisely what the scope of application is: marine ice sheets + marine-terminating ice-sheet outlet glaciers + tidewater glaciers?
We will specify here that we are considering marine-terminating glaciers and ice sheets.

52: None of these three coupling mechanisms seems specific to marine-terminating ice sheets. Tweak wording.
We have changed this sentence to “Our experiments include up to three points of coupling between the ice and hydrology models”.

54-55: Not clear to me how geometry would *not* modulate hydraulic gradient. Geometry is required as an input to any hydrology model, so I'm missing something here. I thought this point would be, rather, that the hydraulic gradient evolves with water pressure and is not taken as a static function of ice geometry (as is done in some of Fowler's work).
That is true, the ice geometry evolves with the water pressure, but this is accounted for in the third coupling mentioned in this paragraph. The second coupling is the one that every subglacial hydrology model should require - ice geometry affecting the hydraulic gradient. While it may be obvious, ice geometry being an input to the hydrology model is included here for completeness.

58-59: ...and these convey most of the discharge and/or these play a dominant role in setting the effective pressure in the drainage system at the spatial scales of interest? Something a bit beyond the existence of channels would help motivate the conceptual model of the drainage system adopted here.
Yes, the work from Dow et al. (2022) suggests that the changes in channels influences the surrounding subglacial environment, for regions up to 100 km of either side of the channel. Our response to comment 2a describes the edits we have made to better describe our motivation for this modeling choice.

60: presumably the domains assume entirely temperate conditions
Correct, in response to another comment (2b, above) we now highlight this assumption in the methods section.

63: for cases of both forward and adverse slopes?
We don’t look at sensitivity to slopes directly in our steady-state experiments, but our transient experiments indicate that this is the case; as the grounding line retreats through regions with an adverse bed slope, the peak in effective pressure remains. Rather than try to explain this in this paragraph, which is mostly about outlining the structure of the paper, we will defer this discussion to section 3, where we will include an additional sentence saying:
“This peak in N persists throughout the retreat over the prograde and reverse bed slopes.”

81: define b here or preferably in (2)
We will define b after Eq. (2).

83: define A bar compared to A
This was a typo - we will consistently use just A.

106: Please note whether N is capped at zero in (5) or (6).
While we don't explicitly enforce a zero-limit on N, N does not become negative in our parameter space. If it were negative, these equations would not apply anymore. We will add "Neither N-dependent sliding laws apply in the case when N<0, so we avoid that scenario in simulations" to clarify this.

114: Curious, given Kinglake's previous work, why the SGD model is restricted to a single channel, vs a channel or channels embedded in a distributed system. At the lateral scales of interest, are the dynamics of a single channel expected to be representative of subglacial drainage? Might help to cast conceptual model in terms of narrow swath of marine ice sheet with a width equal to average channel spacing, if this is the case. It may also be the case that the results are insensitive to (potentially) more realistic assumptions; if this is the case it's worth stating up front in the model description.
See our response to comment 2c above. Additionally, at the end of section 2.2, we will add: "Our one-dimensional ice model can be considered to represent a narrow region of an ice sheet, narrow enough that ice stresses and the effective pressure at the ice-bed interface do not vary in the across-flow direction. Furthermore, we assume that a subglacial channel carved into the ice base exists at the ice-bed interface, is aligned with ice flow, and extends in the along-flow direction from the ice divide to the grounding line. We also assume that the basal effective pressure in this narrow region is equal to the effective pressure in the channel."

Equation 7 and 8: The form of the closure term limits the possible assumed cross-sectional geometries. Worth stating what these are in the model description. Please comment on the decision to neglect the pressure-dependence of the melting temperature, or point out which term includes this effect if not neglected. P(T) seems potentially important for adverse bed slopes such as those characteristic of marine ice sheets and tidewater/fjord-terminating glaciers.
We will specify at line 123: "The closure term in Eq. (7) assumes a circular channel geometry."

121: basal melt only for this application, or is surface melt envisioned?
Surface melt is not envisioned in this case. We have added a statement at line 123: "We assume that M comes directly from the subglacial environment rather than from the ice surface."

127-128: This is interesting because it means the model must assume temperate bed conditions all the way from the ice divide to the grounding line. Might be worth stating this explicitly before and commenting on whether such conditions are common in real-world environments the model is intended to represent.

As mentioned earlier, this may not be realistic as the bed would be frozen further upstream from the grounding line.

131-132: Interesting choice to include this in the model. Presumably the channel is aligned with ice flow and has no tortuosity, hence x in governing equations? Worth stating channel alignment explicitly.
Yes, this assumes that the channel is aligned with ice flow. We will state this explicitly at line 89, as described in our response to the comment at line 114.

138: to be complete, should probably state hydraulic gradient depends on bed gradient too as part of geometry.
Yes, we will add this addition. We will change that sentence to: “The hydrology depends on ice geometry and dynamics. For example, the hydraulic gradient is a function of the bed gradient and the ice overburden pressure gradient. The ice overburden pressure is in turn determined by the ice thickness h, while the velocity…”

157: Primes?
We will refer to the dashes as primes instead to avoid confusion.

162: I think this means that seasonal surface melt is not envisioned as an important contributor. Worth stating earlier if true.
Yes, this is based on Dow et al. (2022) which suggests that the high pressure channels are fed primarily by basal melt

185: transient ice-flow only (as implied by scaling) or transient ice flow and drainage?
The transient experiments simulate fully transient ice-flow and pseudo-steady-state hydrology. We will modify this sentence to clarify:
“We conduct a series of steady-state simulations, followed by a series of transient simulations in which the ice evolves transiently and the drainage system evolves, but is assumed to be in a pseudo-steady-state.”

Table 1:Some detail in the caption would help readers navigate this table, e.g.: does "fixed" mean constant in time (makes sense for geometry, means steady state for hydrology?), does transience apply to both ice flow and hydrology? Would be nice to clearly indicate in table/caption which tests involve two-way coupling.

We will change ‘fixed’ to ‘constant’. The hydrology evolves in the transient experiments, so in that sense it is transient, it is just in a pseudo-steady-state. We think that changing the title of the right-hand column to explain this distinction would be confusing, particularly when the two more important distinctions are 1) between the overall steady state experiments (S1, S2) and the transient experiments (T1, T2), and 2) between the experiment with constant hydrology but evolving hydrology (T2) and the experiment with transient ice and transient (albeit pseudo-steady-state hydrology).

195-196: I think this is the first mention of bed slope. What does the bed look like in other expts? Is the classical adverse-sloping bed of a marine ice sheet ever considered?
There is an adverse-sloping bed in our transient experiments, but in our steady-state sensitivity tests, we did not use an adverse-sloping bed because, in the absence of buttressing, there are no steady states on an adverse bed slope using the SSA model. The marine-ice sheet bed topography is that from the past experiments (e.g. Schoof, 2007; Brondex et al., 2017).

198: I hope the implications, if any, are discussed later. In the presence of velocity gradients in the refined ice grid, a mismatch between ice and hydrology grids seems potentially problematic for realism of coupling.
This is a great point. We will include an analysis of the dependence of our results on the choice of grids in a new section in the appendix (Appendix D). That analysis shows that when the grid resolution is low (lower than we have used in the rest of the paper) a slight discontinuity in the solutions appears at the junction between the two grids. This numerical artefact is insignificant in solutions that use higher resolutions.

203: Does that mean there is just one bed geometry for all experiments?
We use two bed geometries, one linear slope for the steady-state experiments, and an overdeepened bed for our transient evolution. We will add the following where we define b: “For b we use either a linear prograde slope or an overdeepened bed (Section 2.5)”

Table 2: Why stiffer than for standard temperate ice? Add footnote or note in caption if value is taken from precedent or other work. Would be nice in text to relate this to envisioned source: basal melt only? Otherwise reader needs to convert to some intuitive annual melt rate to make inference about source.
We are using the range of parameters used in the experiments from Schoof (2007), and will include this as a source. We also provide justification for the other ranges used in our sensitivity analysis (see response to reviewer 2 for more details).

225: Why do this instead of perturb, e.g., accumulation, which would be a more realistic cause of thinning/retreat? Takes too long?
We have since modified our perturbation to be more similar to that of Brondex et al. (2017), where we change a buttressing parameter at the ice-ocean boundary condition.

228: Oh! That's good. Please state range of bed shapes used much earlier in the MS.
We will add the following where we define b: “For b we use either a linear prograde slope or an overdeepened bed (Section 2.5)”

Figure 2: Why present Coulomb before Budd? Other parts of text and Fig 5 show Budd then Coulomb. Easier to understand if things come in consistent order.
Our new sensitivity plots show both Budd and Coulomb in one figure.

244-245: This makes sense. I was initially wondering why effective pressure instead of flotation fraction was preserved, given the unphysical possibilities that come with fixed effective pressure.
Thank you.

Figure 4: I always think it is helpful to show the domain geometry. You could do this by plotting h as panel (a) but adding it to the bed slope to that the surface and bed of the domain are shown together at the top of the stack of variables.
Since the grounding line positions are different between these models and we wanted to more closely compare the shapes of these profiles with respect to the grounding-line position, we leave the x-axis nondimensional, and as a result, avoid adding in the bed geometry as the different glacier lengths would create additional offsets in the y-direction. We do show domain geometry in later figures.

271: No boundary conditions needed on S given BC on N at GL and on Q at divide? Asking because steep gradients in N remind me of difficulties with BCs on S near glacier termini.
There is a boundary condition for S at the divide that we neglected to elaborate on. We assume that dS/dx at the divide is 0. However, this has no effect because the ice velocity is also zero at the divide. We will clarify this in the main text with the remaining boundary conditions.

290-291: But they do have points of inflection in these variables just downstream of those in the S1 expts. I don't follow this text as it implies to me that there are no inflection points in N, S in the S2 case.
We are referring to the subtle change in curvature right by the grounding line. We will modify this sentence to be "The main difference is that these variables do not have a point of inflection immediately upstream of the grounding line as seen in the results from the coupled model (S1)."

301-302: Good point. Hadn't thought about this as a reason to include advection, but it makes sense that this has to be a closure mechanism in the absence of freeze-on.
Thank you.

351-353: This is a good example of why it is ~nonsensical to impose temporally fixed beta/N/slipperness distributions in time-dependent simulations.
Yes, we sought to highlight that.

367-369: in discussion of one- vs two-way coupling, it might be worth explicitly reminding the reader that this result is different from that of Hoffman and Price (where two-way coupling made a big difference) because of the drainage system morphology: two way coupling should be really important for cavities/sheet where sliding is the main opening term, but much less important for channels where melt is the opening term.
Our transient experiments highlight the importance of two-way coupling. We have modified the sentence to be:
"Our hydrology-only experiment shows how imposed ice geometries and velocities can produce similar profiles of N, though our transient experiments, discussed later, highlight how two-way

coupling specifically between ice geometry, basal hydraulic gradient, subglacial-channel size, and effective pressure) specifically between ice geometry, basal hydraulic gradient, subglacial-channel size, and effective pressure) is needed for realistic retreat.”

471: I still don't understand how this is different than any other hydrology model.
We just mean to say that, to our knowledge, previous work has not coupled together a subglacial channel and an ice sheet using these three couplings. We have shown that these couplings lead to interesting behaviour that has potentially important implications (e.g., S grows large at the grounding line and N peaks upstream of the grounding line due to this set of three couplings and this has implications for, respectively, 1) how we parameterize N at and immediately upstream of the grounding line, and 2) the evolution of ice sheets in models with static bed properties). Therefore, we thought it was worth pointing out that this set of couplings is novel in the opening of the conclusions.

In other words, this sentence is intending to highlight the novelty of the couplings, rather than the novelty of the hydrology model.

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