

Summary

In this article, the authors examine the future of Thwaites (TG) and Pine Island (PIG) glaciers, two major ice streams in Antarctica, and the potential impact of two major uncertainties: the friction, through the effective pressure, and the ocean forcing, through the basal melt. They use the BISICLES ice sheet model, initialised to present day with an inversion, to run simulations over 1000 to 1500 years with different assumptions on 2 parameters: the critical height above flotation h_T , which controls how much and how far from the grounding line the friction is reduced, and a melt rate multiplier a_m , which increases or decreases the total melt under the ice shelves.

By comparing the friction uncertainty with the melt uncertainty, they find that PIG is more sensitive to melt, while TG is more sensitive to friction. They then have 2 results which are particularly interesting:

- Increasing h_T does not always increase the mass loss.
- A remaining ice shelf is important for the stability of TG.

The topic is timely: friction and effective pressure are among the main uncertainties for projections in the Amundsen Sea, and a key question for ice sheet modeler. The manuscript is short and clearly written, and the simulations represent a substantial effort, with millennial runs at 500 m resolution near the grounding line. I also really appreciate that the code, scripts, outputs and movies are all openly available. Forcing each basin separately is an interesting way to separate the responses of the two glaciers (even if it needs to be discussed, see major comment 2).

The 2 results above are new, and I think you should consider rewriting the paper to better highlight them. In its current form, it is not always easy to see what your study brings compared to Joughin et al. (2024). I suggest the following major revisions to better distinguish the two studies, and to improve the manuscript overall.

Major comments

1. As said just before, the manuscript would benefit from a rewrite, to better highlight the differences with Joughin et al. (2024).
2. I find the discussion too thin, with some crucial aspects only partially discussed or not discussed at all. I would like to see at least the following elements discussed:
 - a. Limitations of the study: you use a constant SMB, a particular melt parameterisation with specific parameters, a constant u_0 threshold, but also friction and stiffness fields kept constant during the whole run, and a calving

front whose treatment is not described. Also, both basins seem to reach a full deglaciation (≈ 45 cm for PIG, ≈ 60 cm for TG): how much do the domain boundaries and ice divides control this saturation?

- b. Bed-weakening: I think the mechanism “increasing h_T does not always increase the mass loss” needs its own paragraph or section. I’m not 100% convinced by the proposed mechanism, as I think other factors could explain it. I’ve looked at the videos, which seem to show it, but it would be better to have the explanation in the main text with appropriate figures. For example, your explanation (L135–141) relies on the ice flux to the GL, the surface slope and the thinning rate, so you could show them along the main flowline for 2 or 3 values of h_T . Another possible explanation could be the implementation of λ itself: increasing h_T increases the area where it is applied, but also how fast the friction decreases towards the GL (for ice initially above both thresholds, if $h_T = 100$ m, $\lambda = 1$ for $h_{af} = 100$ m, but if $h_T = 200$ m, $\lambda = 0.5$ for $h_{af} = 100$ m). Also, this effect only appears for h_T above ~ 250 m for PIG (L128–131) and ~ 300 m for TG (L149), so above the ~ 200 m you suggest yourself from Fig. 2. Is it then relevant for realistic values of h_T ?
- c. Water intrusion: this one is less essential, but I would have found it interesting to have more discussion on this point. You discuss the melt, but never mention that those water intrusions might also impact it. In Juarez-Martinez et al. (2026), they implement a melt upstream of the grounding line, which seems quite similar to the λ formulation. Your paper could explore this question further: is the effect of water intrusion bigger on the melt or on the friction? No need to go that far if you don’t want to 😊
- d. TG and melt: if I understand correctly, when you melt PIG, the other ice shelves (TG, but also Crosson, Dotson...) don’t receive any melt, and vice versa? This should be said clearly and discussed, as it directly affects your conclusion that TG is insensitive to melt over the next century, and that its evolution is “largely already pre-determined” (L218–220). With PIG or the smaller neighbouring glaciers retreating at the same time, TG might behave differently. Also, L221–223, you say that it will be difficult to infer the long-term behaviour of TG from 21st-century observations. I find this idea interesting, but it is not shown: you could test it with your existing runs, by checking whether runs that look similar after 50–100 years diverge after some centuries.
- e. The remaining ice shelf for TG (one of your 2 new results): its robustness should be checked, as it may depend on how the melt is applied close to the GL (Seroussi and Morlighem, 2018; see also my minor comment on Sect.

2.1). If partially floating cells receive no melt, a small band of ice always remains at the GL whatever a_m , and it could give exactly this local buttressing. Can you show where this remnant ice shelf stays and how much melt it receives? And when does the no-shelf run start to diverge from $a_m = 10$: in the first century, which would contrast with Gudmundsson et al. (2023), or later?

3. The 2 melt parameterisations for PIG and TG have slightly different shapes, with a different p value. Could this explain the difference of sensitivity between the two glaciers? To give an order of magnitude (if I understand the units correctly): with your a_0 and p , the melt for a draft of 1000 m is about 90 m a^{-1} for both ice shelves, but for 2000 m it becomes about 360 m a^{-1} for PIG and only 180 m a^{-1} for TG. So when the GL retreats on deeper bed, the melt increases twice as fast for PIG, with no upper limit over 1000–1500 years.
4. How realistic are your experiments? The parameter ranges (for both h_T and a_m) are not really justified in the method section, and not discussed at all. But with different ranges and values, you might find quite different conclusions, so I would like to see a justification of these ranges. Similarly, the effect of your initial state/drift is never discussed. A different initial drift could accelerate or slow down the retreat by itself (with or without melt), and also change the relative importance of the two sensitivities explored here.
5. On friction, as it's my speciality, I have several comments concerning the methods, results and discussion, which I group here. First, the regularised-Coulomb law of Joughin has the disadvantage of not being a pure regularised-Coulomb law: in the Weertman regime, you still depend on λ , so on the effective pressure/height above flotation, while it should be independent of it. In this limit, it looks more like a Budd-type law. Second, using a constant u_0 threshold has the advantage of not having to calibrate 2 parameters, but it removes the dependence of the Weertman/Coulomb transition on the effective pressure. So what you describe as a Weertman/Coulomb transition at $h_T = 200 \text{ m}$ (L247–248) might be a real mechanism, and variations of N not due to the proximity of the grounding line might also occur further upstream (see Jager et al., 2026). Third, the optimal value $h_T = 200 \text{ m}$: you could also discuss it in the method section, I let you see. If it's useful, you could have a look at our preprint (Jager et al., 2026), where we find a similar or slightly lower value (around 150 m). Fig. 2 also uses the BedMachine geometry before relaxation, while λ is applied to the relaxed geometry: does this change the picture? And Fig. S21 shows a nice correlation between h_{af} and the distance from the GL: could this dependence be on the distance from the GL rather than on h_{af} ? It would be interesting to discuss it. Finally, on the λ formulation: I personally don't like it too much, because it hides

the physical meaning and doesn't give a sense of the order of magnitude. Actually, in the Coulomb limit, it is equivalent to $\tau_b = C N$ with $N = \rho_i g \min(h_{af}, h_T)$ and a C calibrated locally ($C = \tau_{b,0} / N_0$, with $\tau_{b,0}$ and N_0 the initial values). So I would much prefer to see it written this way, as in Jager et al. (2026) following Joughin et al. (2019), where we find that N tracks τ_b/C quite well near the grounding line with $C \approx 0.1$. It would also allow you to show the map of C , and check whether the values are physically plausible.

6. Additional simulations: I don't think you need many new simulations, but a few would make some of your conclusions much stronger:
 - a. melt applied on all ice shelves, for a few combinations of a_m and h_T (see 2.4);
 - b. the same p for both glaciers (see 3);
 - c. a control run without melt perturbation (e.g. keeping the balance melt from the initialisation), to quantify how much of the retreat comes from the initial drift (see 4);
 - d. a few runs with another formulation of λ (e.g. without the normalisation by the initial h_{af} , or with an N formulation), to see if the effect of large h_T remains (see 2.2).

If you prefer not to run them, toning down the corresponding statements would also be fine for me.

I hope these suggestions are relevant and will help improve the manuscript. My major comments may be somewhat lengthy, but I find the two new results really interesting, and I think they deserve to be put forward. A word on the references: I suggest one of my own preprints (Jager et al., 2026) only because I think it is relevant to your discussion of h_T and friction in the same region. Please don't feel obliged to cite it; if it doesn't help your manuscript, just ignore it. You'll find some minor comments below and, at the end, the references I've mentioned. Hope it helps, and good luck with your revisions!

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Minor comments

- L19–20: MISI and retrograde slope: not directly, there is first an area where the bed is flat.
- L36–41: What about Budd-type laws? Should N be introduced here?
- L41, L77, L81, L83: the citations “Joughin et al. (2010a)” and “Joughin et al. (2019)” point to the wrong papers in the reference list (a Greenland velocity paper and a

Jakobshavn mélange paper). I think you mean Joughin et al. (2010b) and Joughin, Smith and Schoof (2019, GRL).

- L43–49: You could cite Gillet-Chaulet et al. (2016), and the work of Wolovick et al. (2023) and Höyns et al. (2025), who show that single snapshot inversions can give more confidence in one law compared to another (through the regularisation). See also Jager et al. (2026).
- L46: “Other” compared to which ones? Gagliardini et al. (2007) also depends on N , same for Budd et al. (1979).
- Eq. 1: For $u_b \gg u_0$, $\tau_b \rightarrow \lambda C u_0^m$, and for $u_b \ll u_0$, $\tau_b \rightarrow \lambda C u_b^m$. So λ , and then h_{af} , is still there in the Weertman limit (see major comment 6). Also, why $u_0 = 50 \text{ m a}^{-1}$? The value $h_T = 41 \text{ m}$ from Joughin et al. was obtained with $u_0 = 300 \text{ m a}^{-1}$ (L83), so it is not directly transferable here.
- Eq. 2: A more proper way, with some physical meaning, would be to use an N formulation, e.g. $N = \pi g \min(h_{af}, h_T)$. C is then calibrated with N included, which gives the consistency with the initial state you want (L84–85) without having h_0 in the formula. Same for Fig. 2: $h_{af} = h - h_f$ is good, but you could also add a second axis showing the corresponding N (in MPa). Note that which water density to use in h_f (sea water, or a mix with fresh water) is still an open question.
- L64: Sorry, I’m not sure: is the mesh always refined close to the GL? Even after 1000 years of retreat, do you still have high resolution at the new GL position?
- L67–69: It would be nice to specify here, in the main text, the data used for this initialisation.
- L85: Would you not have $\lambda \geq 1$ if the GL advances, for example? It would be physically plausible. And what about cells that are floating at the beginning and ground later ($h_0 - h_f < 0$)?
- Sect. 2.1: What about the GL? How is it treated? Do you apply friction/melt on GL elements? Is the GL always at the edge of elements? Related to this, in Fig. S6 the GL retreat rates seem to jump in steps to preferred values.
- Sect. 2.3: I don’t understand whether you have an evolving forcing. For the atmosphere, the SMB is constant. But for the ocean? Does d_{tc} evolve? And which value of d_{tc} do you take? I only found $d_{tc} = 0 \text{ m}$ in the supplement (Text S3). a_0 and p are in Table 1, but it would help to recall all three here.
- Sect. 2.4 and L111: Why the 0–500 m range for h_T ? Fig. 2c seems to show a transition around 200 m. Same for a_m : why this range? (see also major comment 4)
- L114–115: Really fewer points for PIG? The marker sizes look similar.

- L136–139: OK, it might be that, but nothing shows it here, no? Maybe you could show the evolution of dh/dt and velocity somewhere. And if you had a stronger melt rate close to the GL, could it change that?
- L158: Why not go up to 500 m for these experiments? It would show if a stronger melt close to the GL changes the mechanism of L136–139.
- L179: So a_m goes from 1 to 8, not from 0.1 to 10? (see also Fig. 7). Also, L179, L197–198 and L216–217 give three different numbers for the TG sensitivity to a_m (doubling from 1 to 8, four-fold from 0.1 to 10, doubling for a tenfold increase). It would be nice to harmonise them and to give absolute values.
- L182: The sentence is not clear.
- Sect. 3.1 vs 3.2: Why subsections by glacier in 3.2 but not in 3.1?
- L253–255: But these hydrology models do not represent those water intrusions, so they would not be able to simulate it?
- Fig. 2 caption: “standard deviation on the mean”: do you mean the standard error?
- Figs. 5 and 6: Why do PIG and TG end at different times? And would it not be easier to have both glaciers in the same figure to compare them? Or to combine Fig. 5 with Fig. 7?
- Fig. 7: I don’t see $a_m = 0.1$ here? It looks like it goes from 1 to 8. I don’t understand which values you ran: 0.1, 0.2, 0.4, 0.8, 1.6, etc.? Also, the red-line experiment is described in only one sentence in the method section (L106–108); it would deserve more details.
- Fig. S8: Why do the lines in (b) stop in the middle?
- Fig. S15: OK, so you have some simulations with $a_m = 0.1$ (and below 1). Why are they not shown in the main text?

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Juarez-Martinez et al. (2026): [full reference to add]

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