Van Akker et al. run two different numerical models to analyze the commitment of the Antarctic ice sheet by initializing with current rates of ice thickness change. Overall, this is a very interesting and worthwhile study. However, I think it requires quite some better framing of the results and more explanation:

1) This method of including observed rates of ice thickness change in the spin-up is not new. Inversion-models such as WAVI and Ua do use observed rates of ice thickness changes (dh/dt) as part of their inversion constraints. The method as presented here is quite ad hoc. It might contain an error – see below. And it is not discussed if the method ever converges, at least the authors do not give any convergence criterion on when they stop their initialization.
2) Claims about existing studies are made, but not backed up by relevant studies. For example, the claim that numerical models struggle to reproduce observed mass change rates accurately has apparently been concluded from the initMIP project (Seroussi et al., 2019). Only, initMIP never assessed this. See more comments in the text. Along these lines, MISI, stability, collapse and similar words are used, but never defined, and in places used incorrectly or misguidingly. Please see detailed comments below.

Lay Summary:

Be careful here, you are saying the glaciers will collapse with the timescale of centuries, but you do not mention when that starts. This can be misinterpreted in the press as you stating that "WAIS is collapsing in the next few centuries". Also, your sensitivity tests show a very inconsistent start of this procedure.

Abstract:

Line 13: the claim that most models struggle to reproduce observed mass changes is backed up in the introduction using Seroussi et al., 2019. This initMIP paper did however not assess how well models represent current rates of ice loss, neither was this part of the initMIP experiments and aims.

Line 14: I disagree that this initialization method is new.

Line 19-21: I do not think you can conclude this. You are not modelling the physical reason for the current rates of mass loss, but you are forcing them to occur through your initialization method, which is not validated or assessed in any quantifiable way. You are also not including any feedbacks with climate drivers (e.g., ice-ocean feedbacks) that could enhance or dampen the ice evolution, even for constant climate. You are not considering any effects of variability in climate forcing on the system, and also your GIA initialization is not discussed/validated, so it is unclear how close to reality you are with this.

Moreover, you state “At least a meter in the coming centuries” – what do you mean with coming centuries? Be more specific, it is 2200,2300,2500,... initialization of collapse can be as late as in 2500 years in one of your sensitivity experiments.

Introduction:

The introduction needs substantial reworking. There is a misconception of what MISI is, how inversion methods work, and a number of misrepresentations of existing literature and citations of wrong references for statements.

Line 25: This sounds odd as dynamic losses make all of the current mass loss in contrast to surface melting.

Line 26: What are “multiple meters”?

Line 28: You want to credit the original MISI papers here as Pattyn 2018 only summarizes a known concept.

Line 28: What do you mean with “unforced, irreversible retreat”?

Line 29-33: The explanation of MISI is wrong. It is stated that it occurs only for unbuttressed glaciers, and then referred to TWG and PIG as examples of such. But PIG, and to a lesser extent TWG, are not unbuttressed glaciers (e.g., Fürst et al., 2016; Reese et al., 2018). Even more, we would not care about ocean-driven ice shelf melting if they were unbuttressed.

Line 34: Importantly, these papers do not show anything like “already undergoing MISI-like retreat”, they only suggest it based on finding continued retreat in their modelling studies (Joughin) or irreversible retreat, but for stronger than current forcing (Favier).

Line 42: How do you define irreversible? How do you define collapse?

Line 42: Importantly, the ABUMIP experiments in Sun et al. 2020 are no projections. They are something like a highly unrealistic worst case, as they remove ice shelves instantaneously and assume that they cannot form again. To use this to conclude that further retreat of PIG and TWG will collapse WAIS over several centuries is wrong. What ABUMIP can maybe provide is a highly unrealistic lower bound on timescales of collapse.

Line 43-45: Importantly, this study (Joughin) does not show anything about irreversible retreat. They discuss retreat. Please make sure to clearly define irreversible retreat, and then read the papers carefully to put that each one in context with your definition of irreversibility.

Line 45: Favier does not show that PIG is currently undergoing irreversible retreat. They show that it would, if forcing is applied. They do not make experiments that analyze the current forcing. What do you mean with “unstable” here – if the retreat could be reversed, is it still unstable?

Line 48: “Several studies suggest that PIG and TWG are unstable under the current climate and could collapse on a timescale of a few centuries.” The Joughin study was mentioned before, Arthern & Williams 2017 make no claims about the glaciers being “unstable current climate” or a collapse within a few centuries. Golledge et al., 2017 does find, coming from the last glacial maximum, an eventual ice loss of much of the marine regions of WAIS under constant current climate. However, they find that after 1500 years constant current climate. Coulon et al., 2023 is an EGU abstract that does not appear to use current climate, but model WAIS under various RCP scenarios.

Line 50: Again, none of these studies supports your claim of “relative stability” (whatever that means). Feldmann & Levermann 2015 do not make statements about stability of the current state of TWG and PIG, rather they show that irreversible retreat, after a perturbation in the ASE, is possible. Arthern & Williams 2017 is the same study you just cited in the sentence before to support the opposite claim, Garbe et al., 2020, again, find hysteresis and that WAIS collapse could occur at around 1 to 2 ∘C of global warming above pre-industrial levels (which is the case at the moment), because they make quasi-equilibrium simulations, they cannot make statements about timescales of collapse. Rosier et al., 2021, does not make any statements about the current state of PIG under current climate conditions, they rather show that 3 tipping points could exist, and Reese et al., does show that WAIS eventually collapses irreversibly under current climate.

Line 54: Garbe does not make a claim about ocean thermal forcing.

Line 57-58: Reese et al., 2023 makes exactly this claim of continued retreat and eventual acceleration due to MISI under present-day climate conditions.

Line 62-63: The claim that numerical models struggle to reproduce observed mass change rates accurately has apparently been concluded from the initMIP project (Seroussi et al., 2019). Only, initMIP never assessed this.

Line 65 and following. The authors do not seem to have understood how inverse methods work. It is not "data assimilation methods that are used to iterate towards a specific state", and uncertain parameters are not "tuned iteratively". References for the erratic model drift are required. "So far, matching observed mass change is not used as optimization target": actually, the rates of observed rates of ice thickness change have been added also to inversions, for example in WAVI or Ua. Also, the authors miss out on transient inverse calibration (see papers by Dan Goldbergs). They miss out a mix of methods where a similar approach to their own, to modify the surface mass balance during the initialization with a specific target, has been done for example in Hill et al., 2023.

Line 78: Spin-up models do not necessarily aim for their initial state to be close to equilibrium, in particular not when they initialize with a glacial cycle.

Line 83: Naughten 2022 does not analyze trends in historical Amundsen Sea warming in GCMs. Conclusion cannot be drawn. It should be noted that the advantage of using the GCM forcing is that you represent the physical driver of changes, which is something that you do not do.

Line 85: It should be noted that nudging in the spin-up process to obtain observations is not uncommon, this is used in the PSU. Also, the force-to-thickness approach where the surface mass balance is adjusted to obtain present-day geometry is something that has been applied over Greenland in PISM.

Methods

Table 1: delta T is supposedly also from Jourdain et al, 2020?

Line 124: When the ice is frozen to the bed, no sliding occurs by definition.

Equation 1.2 This equation is different from the one given in Lipscomb et al. 2021. There, the second term was mentioned to create a dampening, so why do you introduce the last term? I think, actually, the sign of the last term is wrong. If you assume that the first two terms in the brackets are zero, then $C_c > C_r$ would mean that the term becomes positive. Since it's sign is negative, and you multiply with $-C_c$, you end up with $\frac{dC_c}{dt} > 0$, thus nudging your friction parameter further away from the regularization value. Or am I missing something here?

Out of curiosity, why do you have a 2 in the second term? I suppose this is some kind of ad-hoc approach, so why add a 2?

Is there some kind of convergence criterion, or when/how do you decide to stop this nudging process?

Line 135: How do you arrive at these values, why do they make sense?

Equation 1.6: A general comment here is that I think this is not a good idea to tune the δT values this way: the way you set them, there is no reason a priori why your values should have any predictive value. In contrast to the sliding parameter, there are some observations and numerical ocean modelling studies available which could be used to inform γ_0 and δT so that you have at least some hope to have the right parameter values. Does the nudging of the sliding parameter not give you enough degrees of freedom to obtain an initial state as you wish?

Additionally, the equation differs from the one in Libscomb et al., 2021. Why did you choose to do this? I think, again, there is a problem with the equation. In the first term, if $H > H_{obs}$ this term is positive, which means that you will decrease δT , hence decrease melting, which should thicken the ice further. Similarly, the second term, if $\frac{dH}{dt}$>0, then you will decrease δT (we can assume here the case where H close to Hobs), reducing melting and thickening the ice further. Are you sure this is how the equation is intended and implemented?

Is the ocean temperature corrected on a basin-scale, or locally? Make sure to show your final values of δT somewhere in the paper.

Line 157: Do you think this choice is justified when comparing with values of γ_0 from Jourdain et al., 2020?

Line 172: Greene et al., 2022 does not apply a calving law to Antarctic ice shelves, other than removing ice in locations where calving occurred in observations. The Amaral study is for Greenland, you want to have here a reference to an Antarctic study as the calving of ice shelves and tidewater glaciers will be quite different.

Line 182: Refer to the section instead of saying “later”.

Line 191: UFEMISM does not use inversion, or? This could be misunderstood, just say “We nudge…” and “UFEMSIM simultaneously nudges …”.

Section 2.2: More detail on the difference between UFEMISM and CISM would be great.

Methods: Please show the nudged fields you use for your simulations somewhere.

Section 2.3: How is the thermodynamics initialized in both models? How is the GIA model initialized if you use one?

Line 204: How do you decide that the method has converged? Is this done for both, CISM and UFEMISM in a similar way?

Line 215: Which dataset do you use for observed mass change rates?

Line 229: In my understanding this is not an inversion.

Line 251: Dow is an EGU abstract. You want to avoid citing non peer-reviewed literature (that is also not accessible) unless really necessary.

Line 259: Swart et al. Is a model intercomparison project description paper, not an original citation for this.

3. Results

Figure 1: The blue boxes on b do now allow to see PIG and TWG grounding lines.

Figure S2: What are the basal melt rates for UFEMSIM?

Line 204: Can you show a map of temperature corrections. What means "on average"? So do you have a local correction in each ice shelf grid cell?

Line 305: I disagree that the velocities are the problem. I think this is simply because you aim to have an equilibrium in a region where in reality ocean temperature drive melting that thins the ice shelf substantially, so you have do dial down the ocean temperatures and reduce melting – in fact, I think this is your motivation to include the observed rates of ice thickness change in the initialization, just looking at it from a different angle?

Importantly, you also never show the fields you generated in your inversions. Also, a figure showing the rates of mass change in present-day compared to observations for both models (currently S4) could be moved to the main text. Your supplementary video would also be useful to have for both models and for longer than 50 years.

4. Results

I would not call this "Future states" as you are not making projections.

Line 330: Not sure I understand why you pick this line, and how you say that afterwards "accelerated collapse begins"? In Figure 2c, it looks like there is a similarly high bed peak just upstream of this?

Figure 2c: The x-axis is not “grounding line displacement from t=0”, but distance from the grounding line at t=0, or? Otherwise, the red line would be odd.

Line 346: From looking at Fig 2c, it could also be more after 450 to 500 years?

Line 349: Twice the currently modelled value in the same location, or at the grounding line at each point in time? That the ice speeds up upstream when the grounding line retreats would be expected, it would be more interesting to compare velocities at the grounding line during the collapse and before.

Line 254: You only discuss this for TWG, can you also discuss the exact procedure of retreat for PIG?

Line 366: Looking at Figure 8, I do not think you can conclude that the collapse can be halted when switching melting off at all times. Most lines show a slowing ice loss trend (which is not surprising as the forcing is reduced), but the trend seems to still go towards ice loss. I think you would need to model substantially longer time scales, and see that in all cases the ice starts growing again, to make your claim about stopping the collapse.

Line 369: Again, the graph you present is not sufficient to make claims about “stopping the collapse”.

Line 371: How do you conclude the MISI resemblance exactly, be a bit more specific. Also, Schoof and Hewitt 2013 is, I think, not an appropriate citation here.

Line 375: This is also shown by for example by Fürst et al., 2016.

Lines 371-379: What do you mean by “pure MISI” - this is only defined for steady states anyways, so would never apply to the real Antarctic ice sheet anyways? As said before, I disagree that your reversal experiments show whether the collapse can be stopped. This whole paragraph needs to be re-considered after extending your simulations, or you can omit it.

Line 382: Holland et al., 2019 does not claim it is natural variability, they find an underlying anthropogenic trend. You state this in the sentence after, so this part seems to contradict itself. Maybe just move the references to the end of the sentence.

Line 387: I find this conclusion a bit odd as basal melting is not directly anthropogenically forced.

Since Figure S8 is discussed quite extensively, I suggest bringing it into the main text.

In this section, a discussion of bedrock uplift would be good, as this is considered one mechanism that could slow down, or halt MISI. How much uplift do you see, how does this compare to estimated rates for WAIS?

Section 5

How are these experiments initialized? Do you just change the parameter, or do you run the model again through your initialization procedure with your new set of parameters? How does this affect your experiments?

Line 396: Relative mass loss to what?

Line 401: I disagree, as your melting experiments show that the timescales of retreat depend also on the melt rate applied. They can hence not only be controlled by bed topography or internal system dynamics.

Table 3: Can you add the maximum and average rates of ice loss during “collapse”?

Line 425: Please see introduction about how you can compare these studies. It would be interesting to also compare with Feldmann & Levermann 2015, Reese et al., 2023 mass loss rates here, and analyze differences.

How much does the choice of the dataset of rates of ice thickness change influence your results?

How much would variability in ocean forcing affect the timescales?

A discussion of caveats is missing.

6. Conclusions

As mentioned before, your method is not particularly new.

Line 461: Add here your maximum timescale of collapse, and you should mention that this really depends on modelling choices.

Line 464: Except for your lower melt experiments where the phase appears to be longer.

Line 465: What means “irreversible for current climate”? Irreversible is a concept from non-linear dynamics, are you applying this here?

Line 469: I think you want to consider natural variability in ocean conditions and how that influences your argument.

Data and code availability:

Consider publishing your scripts and model outputs on a repository such as zenodo.