Reviewer 2

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.

   We thank the reviewer for their insightful comments about the novelty of this method. We agree that our proposed method is not completely new, and that especially in the Data Assimilation models dH/dt has longer been used in the cost functions of those models. We thank the reviewer for the suggestions regarding UA and WAVI, and we will reframe our method from being new to being an addition to models using the spinup initializiation, in order for those models to be able to represent the observed mass change rates in their initialization method. We will furthermore give credit to the DA models that do use de mass change rates in their initialization. We will rewrite the corresponding parts in the introduction, specifically lines 65 – 85 thoroughly.

   We can assure the reviewer that our method converges, and this is indeed implied, but never explicitly mentioned in the main text. We thank the reviewer for pointing this out. Our model drift experiments act as a convergence criterion: as soon as we stop our inversion and keep the inverted fields constant, our model shouldn't drift (or at least, very little). We demonstrate that there is little drift, especially compared to the modelled mass change rates, in the supplementary information (Figures S5 and S6). To us, this is our convergence check, but we will add this to the main text as well, with an explicit discussion on our model convergence and what the model drift means for our future simulations.

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.

   We thank the reviewer for their sharpness and we will adjust the references where needed, following the comments below. We apologize for any misguiding and recognize the sloppiness in our references. We will adjust the manuscript according to the detailed remarks.

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.

We cannot access the lay summary anymore, we will fix this with the editor

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.

We will rewrite the framing of the initialization methods in the introduction entirely following major point 1. We will adjust the abstract accordingly, and add more proper references in the introduction (like Aschwanden et al 2019)

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

We agree, and we will change the framing of our method throughout the manuscript

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.

We agree with the reviewer on this point, we need to be very clear about the limitations of our setup and what that implies for the representation of a plausible future evolution. First, since we are not applying any forcing, we are not doing (realistic) projections, but rather realizations or future simulations. We will change this throughout the manuscript (i.e. 'projections' to 'simulations'). Second, we validate our initialization method against observed ice thickness, ice surface velocities and basal melt rates, but as stated in the major points, we do not discuss model convergence (even though our model converges). We will clarify this in the manuscript, in paragraph 2.3 about our initialization method. We will also include figures on the model drift in the equilibrium initialization as well. Thirdly, we are indeed obtaining possibly unphysically mass change rates through our initialization and we will mention this in the main text. Fourth, to a first order there are ice-ocean feedbacks in our simulations (melt rate – depth feedback: lower ice draft will be in deeper and warmer waters with higher basal melt rates, which will increase the ice draft) and we tested what would happen if the mass change rates would be half of todays rates (which is an estimate of what natural variability could cause the mass change rates to be by chance during the observations), and we added GIA in some of our sensitivity tests (without initializing, i.e. assuming wrongly that the earth is in equilibrium with the present day ice sheet) and our mass change rates are GIA corrected. All these points mentioned will be added to the manuscript.

Lastly, we will frame our results from a general point of view 'WAIS will likely collapse with present day mass change rates' to 'Our results imply that the thermal forcing present in the present-day ocean, when kept constant, will cause the deglaciation of large parts of the WAIS'.

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.

We will change this sentence to be less bold. As the previous remark and reply highlighted: we should be clear on the fact that our setup is limited to the ice dynamical response to current mass change rates, and that this is no realistic projection. We will change this to: 'After 2100, however, dynamical processes are highly uncertain, possibly accelerating GMSL rise significantly, which is in line with (Fox-Kemper et al., 2021; Payne et al., 2021).'

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.

We thank the reviewer, and we will rewrite the introduction according to the major points, and the detailed comments below.

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

We will changed this to 'Dynamical processes are not expected to accelerate or decelerate significantly before 2100.'

Line 26: What are "multiple meters"?

We cannot state a single clear number here so we will rephrase

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

Good point, we will add the references of Schoof 2007 and Schoof 2012 here.

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

We mean self-sustaining, that the glacier, given a certain climate and an initial perturbation, will move towards a new, much smaller initial state, to which it will equilibrate. The initial (small) negative of the perturbation will not be enough to move the glacier back to its old position. We will rephrase these two sentences to include this

We will rewrite these two sentences (also as a reply to the previous comment), to: 'One of such processes is the Marine Ice Sheet Instability (MISI; see Schoof (2012, 2007) and Durand et al. (2009)), which could drive the unforced irreversible retreat of marine-terminating glaciers but whose likelihood is uncertain. This process could drive the irreversible (i.e. the glacier will not return to its present grounding line position when the forcing is removed) and self-sustaining retreat of marine-terminating glaciers'

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.

We note that PIG is indeed (heavily) buttressed, and removed it as example here. TWG is believed to be significantly less buttressed (or not at all). We will add 'Thwaites Glacier (TG) and Pine Island Glacier (PIG), although buttressed'. We will add that MISI is also relevant for buttressed glaciers, and add the reference of Gudmundsson et al 2012 to this part.

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).

We will change this to: 'It is suggested that those glaciers are experiencing continued, accelerated retreat in future simulations and might unground completely in the next centuries (Joughin et al., 2014; Favier et al., 2014; Seroussi et al., 2017). Others suggest that those glaciers have not reached this point yet (Hill et al., 2023; Reese et al., 2023)'

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

We will adopt the definition of irreversible from the IPCC glossary: 'A perturbed state of a dynamical system is defined as irreversible on a given timescale, if the recovery time scale from this state due to natural processes is substantially longer than the time it takes for the system to reach this perturbed state.' Collapse: fast deglaciation (not quantified) leading to an almost complete loss of grounded ice in a basin. We will clarify this

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.

We agree that ABUMIP is not a suitable reference is this context and we will remove this from the manuscript

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.

We will remove the word ‘irreversible’, and explain what we mean by it as a reaction to an earlier comment

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?

To avoid confusion, we will remove ‘unstable’ and ‘irreversible’, we reread the Favier paper and reformulated. We will reformulate this sentence to the sentence placed four comments before.

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 C 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.

We apologize for this sloppy part regarding our references, and we would like to thank the reviewer for pointing this out and their suggestions. We will thoroughly reread the references used here and rephrase and reorder this paragraph accordingly (and change the EGU abstract to the published paper) and reread Arthen C Williams 2017. We will add the timescale to Golledge et al 2017, We will change Coulon from the EGU abstract to the actual paper.

Line 50: Again, none of these studies supports your claim of “relative stability” (whatever that means). Feldmann C 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 C 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.

We understand the confusion of the reviewer, apologize again for our sloppiness and thank the reviewer again for their sharp eye and we will rewrite this paragraph accordingly. We will change this paragraph to:

‘Several modelling studies have assessed the potential for ASE collapse. One study (Joughin et al., 2014) argued that under present-day melt rates, TG might already be on a trajectory toward accelerated retreat; moderate retreat in this century will likely be followed by a phase of rapid collapse beginning in the next 200 to 900 years. Another study (Favier et al., 2014) used three ice sheet models to show that PIG is now undergoing a forced 40-km retreat (but makes no projections after this). The retreat could be reversed by sufficient ocean cooling (Favier et al., 2014). Both studies mention MISI as the main driver of retreat. Subsequently, studies suggest that TG and PIG are unstable under the current climate and could collapse on a timescale up to 2000 years (Golledge et al., 2021; Coulon et al., 2024) and others suggest that the two glaciers will collapse with additional forcing (Feldmann and Levermann, 2015; Arthern and Williams, 2017; Reese et al., 2023; Garbe et al., 2020). '

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

We agree, and we will remove the reference here.

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.

We will add Reese et al, 2023

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.

We agree and will add Aschwanden et al 2021 here and rephrase to :

'Seroussi et al. (2023) found that the uncertainties in sea level rise projections using ice sheet model increases exponentially with the length of the simulations, with the ice sheet models itself being the main contributor. Uncertainties in ice sheet modelling arise from four main sources according to Aschwanden et al. (2021): suboptimal ice sheet model initialization, incomplete physical understanding of important processes, numerical model uncertainty, and uncertainty in the climate forcing. With respect to initialization, some ice sheet models employing the so-called spin up initialization method, struggle to represent observed present-day mass change rates, because the inherent result of a successful initialization is a stable ice sheet without model drift. Representing these decadal-long present-day mass change rates right is essential for reliable projections, as these changes are the primary observable of the dynamic state of the ice sheet.'

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.

We will change ‘iterate towards a specific state’ to ‘capture conditions at a certain time’. We will change ‘tuned iteratively’ to ‘minimize a cost function between observed and modelled quantities’. We will remove the sentence about model drift if we cannot find proper references. We will remove the sentence about optimization targets and will add a discussion on the methods used in UA, WAVI, and by Dan Goldberg and Emily Hill. We will add the following paragraph:

‘The dynamic state does not necessarily resemble the observed mass change rates, even when using a cost function to iterate towards these rates. This can be counteracted locally where (sub)annual observations are available by doing a transient calibration as suggested by Goldberg et al. (2015). Also, the modelled mass change rates can be added to the cost function as was done for example by Rosier et al. (2021) in the ice sheet model Úa (see e.g. Gudmundsson et al. (2012)) to minimize model drift, by penalizing non-zero ice thickness change rates in the same way as was done for the ice surface velocity mismatch. Bett et al. (2023) used the ice sheet model WAVI (Arthern et al., 2015) and added the mass change rates from Smith et al. (2020) into the cost function but now penalizing model drift difference with the observed mass change rates. The WAVI inversion method described in Arthern et al. (2015) results in a grid based value for two free parameters relating to basal friction and the ice viscosity. Since both ice velocities and mass change rates are targeted simultaneously through the cost function, it is likely that there is a trade-off between errors in the two target quantities and that the modelled mass change rates do not necessarily agree with observations in all locations. A quantitative comparison between modelled and observed mass change rates is missing in Arthern et al. (2015) and Bett 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.

We will adjust the text accordingly and clarify that we mean the present-day state of the ice sheet, and credit spin-up models

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.

We will remove the reference and agree with the reviewer that the physical driver of change when using GCM forcing is valuable. Our method is a trade-off between modelling the physical driver of change and having full control on where the numerical same mass change rates as observed will be applied. Although we tune ocean temperatures to fit with mass change rates, this is completely different from using full GCM output.

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.

We do not claim that the nudging method is uncommon. We are not familiar with the 'force-to-thickness approach', and not with 'PSU'.

Methods

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

No, in our study, this is an inversion targeted variable. We will make this clearer in the table.

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

Correct, thanks for the remark. We will change this to 'When the ice slides over rough and non-deformable hard beds'.

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 $Cc > Cr$ would mean that the term becomes positive. Since it's sign is negative, and you multiply with $-Cc$, you end up with dCc >0, thus nudging your friction parameter further away from the regularization value. Or am I missing something here?

That is correct! Thanks, we changed the minus to a plus. This was wrongly copied over from the code. This equation is different compared to the earlier CISM paper because there have been updates in the code. The second term is still a dampening term that, during runtime, will decrease the change in the friction parameter if the ice thickness is already moving into the right direction. The third term is a relaxation term that 'pulls' the tunable parameter away from being over tuned to a modelers chosen target.

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?

No particular reason, this evolved from an earlier version of the CISM code. Since there is a parameter H_0 in this term, it does not have added value. Removing it and doubling H_0 would have the same effect.

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

When the ice sheets exhibit very little to no model drift when the inversion is stopped and the model is allowed to run forward with the inverted fields. This is achieved, and we will clarify better in the text that we tested this. We will add this to section 2.2, and also quantify this for the equilibrium initialization in the supplementary material.

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

Generally, we follow the logic that deeper beds should have weaker till. There might have been changes historically between ice covered and ocean covered, creating a weaker till layer. We took the exact values from PISM. A reference will be added, Aschwanden et al (2013).

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?

It does not, if we want the grounding line to be at the correct position (which we definitely want when doing stability analysis) we need a free parameter related to basal melting as well. We agree to the weakness the reviewer points out regarding the predictive value of delta T, and we note that the corrections needed are often small and average out over an ice shelf to something close to zero. However, since our basal melt scheme is based on a single parameterization (rather than a sub model, or a coupled ice-ocean model) we think it is justified to allow some tuning here, since the parameterization itself in combination with the dataset from Jourdain et al (2020) will not provide us with the right basal melt rates. Typically the tuning is within the accuracy of the dataset itself. We agree with the reviewer that the deltaT tuning is somewhat arbitrary and will make this more clear in the paper. We will add to paragraph 2.1.2:

'In a forward run, we keep δT constant. When the grounding line retreats, we interpolate basin-average (basins as identified by Zwally et al. (2015)) values of δT newly floating cells. Full interactive Ice-ocean coupling using CISM combined with an ocean model is under development. '

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?

Thanks, we removed the minus in front of the deltaT, which is erroneous. In addition, this equation differs for the same reason why we changed our friction inversion as it proved to make the convergence during the initialization faster. We will add a sentence to both equations stating this. We will add:

'This equation is slightly different compared to Lipscomb et al. (2021), as we found that this equation yields more accurate results in terms of ice thickness, and converges faster.'

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

We agree to show the delta T values, as well as our inverted coulomb c, they will be added to the supplementary online material in a four panel figure: coulomb c and delta T for the whole AIS and for the ASE region, for both the transient and the equilibrium initialization.

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

Yes, this value for the non-local slope parameterization is in the same order of magnitude as the values presented there, and what is more, this has been tested specifically for CISM in the Lipscomb et al 2021 paper. We will add this reasoning to the text.

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.

We will add a discussion on Antarctic calving in the text as well. We will add: 'There are several calving laws in the literature (e.g. (Yu et al., 2019); Wilner et al. (2023); Greene et al. (2022)). However, there is no agreed-upon best approach to Antarctic calving (this holds for the Greenland Ice Sheet as well, see for example Benn et al. (2017)), and most calving laws struggle to reproduce the observed calving front at multiple locations simultaneously (Amaral et al., 2020).'

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

We will adapt this to: 'We use the same transient initialization procedure including the mass change rates from Smith et al. (2020) in the mass balance equation like in Eq 1.9'

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

UFEMISM does use inversion, we thank the reviewer for this suggestion, and we will use this in the main text

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

We will add a separate UFEMISM section in the manuscript and expand this part of the methods to include a better UFEMISM description here.

Methods: Please show the nudged fields you use for your simulations somewhere.
We will add those to the supplementary materials, and discuss the difference in inverted fields between the mass change rates run and the former initialization in the main text

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

Thermodynamics: with the Robin solution. GIA: assumes that the bed is in steady state at t=0. This is not necessarily realistic but the best we can do with CISM right now (and with, many other models). Both statements will be added to the text like: 'The thermodynamic profile is initialized with the Robin solution, and no GIA is used in our default experiments i.e. we assume a static bedrock.'

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

By assessing the modeldrift when the inversion is stopped. This is done in a similar way in both models. This explanation will be added to the text. We will add: 'This typically takes 10^4 model years for the model to converge, which we assess by calculating the model drift left in the modelled ice sheet when stopping the inversion (i.e. by continuing the simulation with the inverted fields kept constant). If the model drift is about two orders of magnitude smaller than the observed and modelled mass change rates, and there is no spurious grounding line movement in key regions (like PIG and TG), we accept the initialization'

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

Smith et al 2020, reference will be added

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

In our understanding, our nudging procedure is an inversion. We will rephrase it to 'Stopping the nudging procedure and keeping the nudged fields constant.

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

We will remove this citation .

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

We will change this sentence to: 'For example, cooling of the Southern Ocean sea surface (Bintanja et al., 2015) and reduced Antarctic Bottom Water formation (Williams et al., 2016).'

3. Results

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

We will make them smaller.

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

We will add a UFEMISM discussion like the discussion of the CISM initialization

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?

Yes, we will do this in the supplementary material for both the equilibrium and transient initialization. On average means on average per shelf, so we calculated both for Thwaites and Pine Island separately what the average ocean correction is, since the inversion is grid cell based. We will clarify this

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?

Correct, our wording in this paragraph is fuzzy on that point, but this is indeed the point we tried to make. We thank the reviewer for this remark, and we will rephrase this. We will add: 'In the equilibrium initialization, ocean temperatures in the Amundsen Basin on average must be reduced by ~0.5 K compared to the thermal forcing dataset from Jourdain et al. (2020) to reproduce the observed ice shelf geometry. In the observations of Smith et al. (2020), the shelves are thinning rapidly. Simulating large negative mass change rates here requires higher ocean temperatures.'

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.

We will add figure S4 to the main text. We will add fields of inverted coulomb c and delta T of both initializations to the supplementary material. We will rethink the need for a supplementary video and if we will keep it, we will extend the length.

4. Results

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

We will change it to 'realizations'

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?

Before the glacier reaches this ridge, we see linear mass loss comparable to the observed rate and accelerated mass loss when the glacier ungrounds here, despite the ridge of 100 km. This identified ridge is the 'last resort' for Thwaites glacier: as soon as the grounding line passes this one, the accelerated collapse happens. We will add this explicitly to the manuscript as: As soon as the grounding line passes over the line AB in Fig 2A, even the higher ridge at approximately 100 km downstream of the present day grounding line location cannot stop TG from collapsing. This second ridge is in the cross section of similar height as the ridge AB, but less extended in the cross-flow direction and surrounded by throughs, see Fig S7.

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.

We will change this accordingly.

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

We will change this accordingly as well

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.

The grounding line constantly moves so this would make the comparison difficult. But we will assess the velocities along some flow lines following the grounding line to see if we can find doubling there as well. We will change the text accordingly to: 'Ice velocities in the main channel of TG exceed 4000 m/yr, about twice the current modeled values at that same location (Fig. 3e).'

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

In our simulations, PIG is 'attacked from the side by TG'. So TG collapses and takes PIG with it. We therefore argue that it is more interesting to look at the evolution of TG in more detail, since a collapsing TG takes PIG with it. We will clarify this in the text as: 'PIG is 'dragged along' by a collapsing TG. Both glaciers also collapse when the observed mass change rates are only applied to their single basins (not shown). '

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.

In all experiments where the basal melt is switched off (yellow lines in Figure S8a, the ice sheet starts to regrow. We will add: 'We observe two features in these 'Zero basal melting' experiments: the ice sheet never grows back with the same rate as it collapsed, and the grow back rate is lower when the basal melt rates are switched off later during the simulation.'

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

We will change this to: ' We furthermore tested the effect of an instant cool-down at 250 years (brown lines) and 500 years (red lines) during the simulation. None of the cool-down experiments where enough to regrow the ice sheet. We furthermore tested a percentual decrease in basal melt rates at certain timesteps. Both 25% and 50% decrease (yellow and brown lines) in basal melt rates are not enough to stop the collapse, only to slow it down. A 75% (red lines) decrease in basal melt rates turns the mass change rates from negative to eventually positive, but only when applied before 200 years in the simulation.'

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.

Since there is accelerated collapse on a retrograde bedslope in all our simulations, we assess the collapse to be at least MISI – like. We cannot show, like in Schoof et al 2007 (which is the reference we will use here), mathematically that this is the same instability as in the paper. We will clarify this in this paragraph.

Line 375: This is also shown by for example by Fürst et al., 2016. We will read this reference and add it to the text.

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.

We will remove the word ‘pure’, because we agree with the reviewer that the theoretical MISI formulation of Schoof 2007 is not applicable to most real life cases, since there are multiple differences with the theory presented by Schoof (to start, this is a 3D case, and the grounding line is not necessarily (back)stress free. We tried to show that, when the forcing is completely removed (in this case the ocean stops melting the ice shelves) the collapse does not continue. In the theoretical case of Schoof, MISI is not influenced at all by (ocean) melt and is self-sustained on retrograde beds since the GL flux will always increase. We will rewrite this paragraph to clarify.

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.

We will do this.

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

We will add ‘via the ocean temperatures’

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

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?

we will add a paragraph on GIA, the rates in CISM, its effect on MISI and some observations in our missing processes paragraph, and at the beginning of section 5.

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?

Depending on the parameter we either rewrite the necessary parameterizations or redo the initialization, as mentioned later in this paragraph. We make sure to use the exact restart (rewriting parameterizations with free parameters so they resemble the main initiliazation) as much as possible to eliminate the effect of a different initialized state as much as possible

Line 396: Relative mass loss to what?

Compared to the initialized state, this will be added

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.

Yes, the timescale of the start of the 'fast phase'. The 'fast phase' itself however, progresses roughly the same way in all simulations (the 250 year steep part in the curve). We will clarify this

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

We thank the reviewer for this great suggestion, and we will add those to Table 3.

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

We will rewrite the introduction (mainly targeting the framing of our method and the literature review), and change this part accordingly

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

We did not assess this, because we did not have another mass change rates dataset with this spatial coverage at our disposal. We tried to lower the mass change rates with 25%, and we did not see a large influence. We also decreased our SMB and basal melt rates with certain percentages during the run and did see a slowdown but not a prevention of the collapse (which we choose to be the main point of our study).

How much would variability in ocean forcing affect the timescales?

We think this will have a moderate effect on the timescales, since changing the basal melt rates with 25% or 50% siginificantly slows down the collapse

A discussion of caveats is missing. This will be incorporated in the manuscript in section 'Missing processes'.

6. Conclusions

As mentioned before, your method is not particularly new. See earlier replies

We will change this in the introduction and we will also change this here.

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

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

We note a slight increase in collapsing timescales, and we will add this as nuance to the conclusion

Line 465: What means "irreversible for current climate"? Irreversible is a concept from non-linear dynamics, are you applying this here? Irreversible in the sense that the current configuration of TG and PIG cannot co-exist with the current ocean/atmosphere.

We could not test if applying the negative of the perturbation that caused the mass change rates will regrow the glacier, so we will remove the word and claim 'irreversible'

Line 469: I think you want to consider natural variability in ocean conditions and how that influences your argument.
Great suggestion, we will add this

Data and code availability:

Consider publishing your scripts and model outputs on a repository such as zenodo. We will do this