Reviewer 1

The manuscript ‘Present-day mass loss rates are a precursor for West Antarctic Ice Sheet collapse’ by T. van den Akker and co-authors presents a new method to improve the initialization of ice flow models that better captures their current trend and should improve the reliability of projections of future ice sheet evolution. This is a very nice improvement and the method should be applicable to a range of models, therefore having a large impact on this community. The results, based on an ensemble of simulations and the use of two different ice flow models, show that the current mass loss observed in the Amundsen Sea sector is a precursor to large and rapid changes in the future.

I find the idea presented interesting and simple to implement, therefore making it a very nice improvement to other models. However, I find the manuscript confusing at times, some reorganization and changes in the figures/tables would help a lot. I also think the conclusions are not always supported but the results and the manuscript should be clarified to better integrate both models and ensure a direct comparison of the results.

We thank the reviewer for their insightful comments above about the applicability of our method, and the detailed comments below.

Major points:

The current mass loss in West Antarctica is said to be a precursor to large future changes, but there is no direct comparison or quantitative analysis of what would happen otherwise (without the current rate of change) and how it pre-determines the future changes. This should be better justified and quantified, as demonstrated by model results, or this conclusion should be removed from the manuscript.

We thank the reviewer for this comment and understand the confusion about the equilibrium spinup and the spinup using the observed mass change rates, as they are presented in the manuscript. Simulations initialized without the current mass change rates, i.e. the equilibrium spin-up in the manuscript, only develop marginal large mass change rates at all and the WAIS does not collapse. This is because the ice sheet model CISM is designed to behave like that. After an equilibrium spin-up, the model should be stable and in steady state, because we only stop our inversion once our ice thickness does not change anymore, and we therefore cannot get closer to the observed values. So by definition, an equilibrium spin-up results in a modelled ice sheet with $dH/dt \approx 0$. Since we employ unforced future simulations to test whether or not the observed mass change rates are sufficient by themselves without climate change to let Thwaites Glacier and Pine Island Glacier collapse, our equilibrium initialization does close to nothing: since dH/dt is close to zero and no forcing has been applied, this is a model drift experiment. A typical model drift simulation with our equilibrium model setup shows a slight thickness increase on the Pine Island glacier, but this model drift is very small compared to the observed and modelled mass change rates (see Fig S6 in the supplementary material).

To make a direct comparison between the equilibrium initialization without dH/dt, and the main experiment with dH/dt, we will add the model drift experiment of the equilibrium initialization (i.e. 1000 years continuation from the equilibrium initialization with fixed inverted fields) as a figure in the supplementary information. Furthermore, we will move the observed and modelled mass change rates in the first five years of the default experiment to the main text. Additionally, we will show the inverted friction parameter and ocean temperature correction in both initializations in the supplementary material.

Another point is that this new method is used by two different ice flow models, which is great to show its easy implementation and possible impact on a range of models. However, the results from the two models are barely compared and used in a different way. It is therefore difficult to assess if similar results are achieved: for example, showing how spatial mass loss are very different without this method but very similar after including it in both models would show the similarity. In order to quantify the impact of these changes and the improvements made, it would be important to compare the models more directly and systematically, both for the initialization and for the sensitivity experiments.

We thank the reviewer for their positive take on using two ice sheet models. We agree that the results of UFEMISM are not discussed as extensively as we discussed our CISM results. Initially, we developed our method especially for CISM, and when we found the WAIS collapse in all of our forward simulations, we wanted to make sure that this feature was not restricted to CISM before drawing too firm conclusions. For this reason, we added the UFEMISM runs to show that adding dH/dt also for that model shows a collapse. In other words the result is not a CISM artefact. As a secondary goal we hereby show that the method can be easily implemented in another ice sheet model.

We thank the reviewer for the suggestion of including the spatial patterns of mass loss, and we agree that more should be written about the UFEMISM simulations. We will add a figure like Figure 2 in the main text, but with the UFEMISM results, and a section in the results discussing the UFEMISM results in detail.

It is also not clear which experiments have been performed by which model and why such choices were made, and why only the results from CISM are shown most of the time, with barely any result from UFEMISM. This should be clarified to help the readers follow easily the results and conclusions. Similarly, the manuscript is confusing at times and could be made more straightforward (see detailed comments below).

We agree with the reviewer, and as stated above, we will add a section and a figure in the results section showing in detail the results of the main UFEMISM experiment.

Technical comments:

- l.20: it is not clear how the current mass loss is a precursor to future large changes, the mechanism or reason causing this should be mentioned.

We will change this to: ‘Our results imply that the thermal forcing present in the present day ocean state, when kept constant, will cause the deglaciation of large parts of the WAIS’

- l.28: MISI is not really uncertain, it is a mechanism reproduced in a number of models and analyzed in depth by many theoretical papers. Its likelihood however is uncertain. This should be rephrased.

We will change this to: ‘One such process is the Marine Ice Sheet Instability (MISI; Pattyn et al. (2018)), which could drive the unforced irreversible retreat of marine-terminating glaciers but whose likelihood is uncertain.’

- l.29: ‘unbuttressed outlet glaciers’ I thought this was applicable to both buttressed and unbuttressed glaciers, even if the rates of fluxes are impacting by the buttressing.

The theoretical concept of MISI, as presented by Schoof 2007 and Schoof 2012, assumes a grounding line without backstress i.e. where there is no buttressing ice shelf. We agree with the reviewer and with Gudmundsson et al 2012 that MISI might occur for buttressed ice shelves as well, and we will remove the discussion on buttressing from this part accordingly.

- it remains unclear whether or not these glaciers are already engaged in an unstable retreat, recent studies suggest it might not have reached such a point yet (Hill, Urruty et al., TC, 2023; Reese, Garbe et al., TC, 2023).

We will add: ‘Others suggest that those glaciers have not reached this point yet (Hill et al., 2023; Reese et al., 2023)’

- l.37: higher than what?.

We will change this to ‘high’

- l.38: ‘a model study’ -> ‘model studies’ (several studies cited).

We will change this to ‘model studies’

- l.50-55: another study based on a model ensemble and investigating the marine ice sheet instability mechanism worth discussing here is Robel et al. (2019).

we will add ‘Robel et al. (2019) argued that the possibility of MISI amplifies uncertainty in sea level rise projections.’

- l. 60-65: it would be good to also add the uncertainty study by Seroussi et al. (2023).

We will add ‘Seroussi et al. (2023) found that the uncertainties in sea level rise projections using an ensemble of ice sheet models increases exponentially with the length of the simulations, with the ice sheet models itself being the main contributor, rather than the forcing.’

- l.67: maybe ‘to iterate toward a specific state’ -> ‘capture conditions at a given time.

We will add ‘capture conditions at a given time’

- l.72-73: the problem is rather matching a single time vs a period of change (Goldberg et al., 2015).

We will add ‘This can be counteracted locally where (sub)annual observations are available by doing a transient calibration as suggested by Goldberg et al. (2015)’

- l.81: mention that this initialization is done by running very long spin-ups.

We will add ‘consists of a long run with’

- l.84: How does that impact the initialization?

It does not impact the initialization directly. What we mean here is that after the initialization, when an ice sheet modeler would want to simulate present-day conditions, they could use the thermal forcing anomaly of an ocean model to simulate the recent warming in the ASE. However, not all ocean models are capable of simulating this warming, so using this output will in those cases lead to underestimating of the melt rates and therefore to an underestimation of the modelled mass loss. We will rewrite this section to clarify these points.

- l.88: Explain the DA biases, which ones and why?

The DA biases targeted here are 1) the pattern mismatch of the thinning rate due to the use of a general cost function and 2) unwanted model drift. 1) is the result of treating the mass change rates error together in the cost function with the velocity error: this will help a modeler to simulate mass change rates overall, but it is virtually impossible to model the observed mass change rates correctly everywhere since there can be a trade off in velocity errors and mass change errors. This error then might become unwanted model drift that might influence the future evolution of the modelled ice sheet. We will rewrite this entire paragraph to make this clear, and to credit the DA method more for applying the mass change rates in another way in their initialization procedure.

- l.90-100: it is not clear when each model is used for the different experiments, when only one model and when both are used? Also why are experiments done with one or both models? I don’t know if this is the best place but it should be clarified.

We understand the confusion, and note that the way we framed UFEMISM is flawed. We will make clearer in multiple positions in the manuscript the supportive role of the UFEMISM simulations (described in the major point above).

- l.110: maybe this should go in the UFEMISM section

We agree and will make this change.

- l.133: explain what you mean by `the size of each term'.

We will add 'the relative magnitude of each term compared to the sum'

- l.134: 'linear interpolation'.

We will add this

- l.135: explain why you use these values and how you chose them.

We chose them as they are the default values in PISM. We will add the sentence: 'We chose targets of 0.1 for bedrock below -700 m asl and 0.4 for 700 m asl, with linear interpolation in between, similar to Aschwanden et al. (2013)'

- l.146: why do you use p = 0.5? How is that constrained or calibrated?

This is a modelling choice, which is not well constrained by observations. We tested other values between 0 and 1 and found 0.5 to be a reasonable compromise based on inversion freedom (higher p decreases the effectiveness of the friction inversion) and physical intuition (there should be some basal friction decrease for ice resting on a bed far below sea level). We will add: 'A value of p=0.5 is chosen in this study to include some hydrological connection but prevent instabilities during the initialization when using p=1, which was mentioned in the study by Lipscomb et al. (2021) and Leguy et al. (2021).'

- l.155: is this the same \tau or a different o? What is the impact since it is a different physics and orders of magnitude?

The \tau is different. We will distinguish the two variables in the revised text.

- l.169: How is that scaled? Is it done linearly, or bilinearly or something else?

The grounded and floating fractions are determined bilinearly. The method is described by Leguy et al. (2021), which is cited earlier in the paragraph.

- l.171: 'including a few applications' … ' e.g., Yu et al.. (2019)'.

We will change this to 'There are several calving laws in the literature (e.g. (Yu et al., 2019); Wilner et al. (2023); Greene et al. (2022)).'

- l.172: I think it is the same problem for Greenland? Or if not you could explain why it is different between Greenland and Antarctica.

We will change this to: '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).'

- l.176: why was 1 m chosen? What would be the impact of choosing a higher threshold?

Conservatively, we did not want to remove too much ice. Since we make conclusions about the collapsing WAIS, we did not want it to be an artifact of a too aggressive calving threshold. A higher limit would decrease buttressing faster and probably increase the rate of collapse. A small threshold ensures that WAIS collapse is caused mainly by sub-shelf melting and not additional calving. We are in the process of developing calving schemes that could be used in a future study to evaluate sensitivity to calving laws.

- l.181: it would be easier if the main experiment had a name or something easy to refer to.

We will refer to it as the'default experiment'. We will change this in the text.

- l.184: the sliding is also different.

Correct, we will note this.

- l.185: 'targeted high resolution': what does that mean?

High resolution only in the places that are dynamically interesting, for example near grounding lines. We will change this to: 'at variable resolution, e.g. simultaneously high resolution at the grounding line and lower resolution in the slow moving interior (Berends et al., 2021).'

- l.197: what else is different between CESM and UFEMISM?

There are several other (small) differences, of which the main difference is the use of triangles instead of rectangles as discretization. Furthermore, UFEMISM uses a different, CFL constraint, time stepping scheme, and a slightly different initialization procedure containing nudging followed by a relaxation time. The latter is absent in CISM.

- l.207: 'with observations like the result of the equilibrium' -> 'with observations, similar to the result of the equilibrium'.

We will change the text as suggested.

- l.210-215: this explication is a bit confusing, since this is a key element of the method, you should add a schematic equation to explain it more clearly.

We will add Eq 1.8 and 1.9, schematic mass balances of an ice sheet featuring a correction term. We will use those schematic equations to, in this paragraph, make more clear what we do to arrive at the observed dH/dt

- l.226: ' would make the ice sheet theoretically more stable': why is it more stable? In your set-up or in the experiments? If you want to show that this method provides more mass loss, what is the best way to demonstrate that? Adding something like that would go a long way.

In the setup without using a dH/dt, we are spinning up to a quasi-steady state in which dH/dt is close to zero. In regions where the observed dH/dt is negative, our inversion will typically compensate by cooling the ocean and/or increasing the basal friction, possibly in ways that are inconsistent with the current ice state. Thus, the spun-up ice sheet is likely to be too stable, and will react differently and possibly slower to forcing. As stated in the reply to the second major point, we think this is a very interesting question and we thank the reviewer for raising it, this will definitely be the scope of future research. Since we focus purely on the effect of the present-day mass change rates in this paper without further forcing. We will rewrite, as stated above as reaction to the first major point, sections 2.2 and 2.3 and clarify the characteristics and difference between the two initializations.

- l.235: is the 'default evolution' similar to a case with constant climate conditions?

Yes. We do not apply forcing, we only let the ice model run with the present-day mass change rates. We do not modify the forcing from the spin-up to the forward run; we let the model run with the same forcing and inversion parameters as obtained during the spin-up, i.e. we run forward from the present-day disequilibrium.

- l.237: in which case is it negligible?

The model drift is negligible, so our forward experiments are not hindered by spurious thinning or thickening. We will change this sentence to: 'Complementary to this default experiment, we verified that the model drift is negligible with forward runs in which the mass balance correction term CR remains included in Eq 1.9 (Fig. S6). In this case, if the spin-up was successful, the modelled ice thickness change rates should be approximately zero'

- l.246: which parameterization is that? Add a reference or equation to explain it.

The PISM parameterization, we will add the same reference (Aschwanden et al 2013) here as well

- Overall I am confused about the forward simulations. The abstract mentioned projections, but I only see the constant climate conditions with and without the mass correction or something like that. I would be important to clarify and describe these experiments with more details.

Thanks for this comment, we noted that our forward simulations are in fact not projections. Since we do not use any forcing, the word projection is confusing, and we thank the reviewer for pointing this out. We will remove the term, and replace it with 'simulations'

- Fig.1: the model used to produce these results should be mentioned at the beginning of the caption. Remind that e and f are observations and which ones were used to calibrate the basal melt for the model.

We will change this accordingly to ‘Initialized state of the Antarctic Ice Sheet using CISM’

- l.287: one important method is the difference between the two initialization methods. It should be shown more and not just in the supplementary material. It should also be shown for the two models to compare the impact in both cases and how both methods display similar trends or not with the new initialization.

We will add a figure showing the difference in inverted quantities (Coulomb c and the ocean temperature perturbations) in the equilibrium initialization, to show what the effect of adding the mass change rates is on the inversion. Since the focus of this manuscript is the transient initialization and the continuation run with present day mass change rates, we would like to keep figure S3, the evaluation of the performance of the equilibrium initialization, in the supplementary material. As stated before, there is per definition very little trend in a simulation starting from a successful equilibrium initialization, since we do not change the forcing. The only trend in a simulation starting with the equilibrium initialization is the model drift, and we will add a figure in the supplementary material to show that this is very small, similar to the model drift experiments with the transient initialization.

- l.287: how low is the bias? Add numbers.

We will add this from the figure caption and change this paragraph to: ‘The thickness error over the WAIS is low (RMSE = 19.3) , and the GL closely follows observations, with an average 1.5 km difference (calculated as the average distance between the modeled GL position and the closest observed GL position). The modeled GL for PIG is shifted seaward by 5–10 km. The modeled basal melt rates and melt patterns beneath floating ice agree well with the values from Rignot et al. (2013) and Adusumilli et al. (2020), with an integrated melt flux within the range of the two datasets (CISM; integrated flux of 106 Gt/yr, Adusumilli et al. (2020); integrated flux of 94 Gt/yr and Rignot et al. (2013); integrated flux of 149 Gt/yr).’

- l.290: same here, how well does it agree? Add numbers.

See reply to the previous comment

- l.294: are you referring to the models from the ISMIP6 ensemble using data assimilation or spin-up?.

Both, we will add this to the text as ‘The root mean square error (RMSE) of the ice surface velocities is comparable to other ISMIP6 models (Seroussi et al., 2020), of which the range is 100 – 400 m/yr, where many are optimized to match observed velocities and use a variety of spin-up and data assimilation methods.’

- l.308: how do these other corrections compare?

When not initializing with dH/dt, our inversion procedure under the ice shelves needs to cool down the ocean more. The assumption of steady state is inconsistent with the observationally based thermal forcing, which implies high melt rates and thinning. The ocean must cool to reduce the melt rates and achieve a steady-state thickness roughly consistent with observations. In the equilibrium initialization, ocean thermal forcing corrections were largely (on average) negative under Thwaites and Pine Island shelves, they get closer to zero in the transient calibration (in the order of 0.2 – 0.5 K). We will add this to the manuscript.

- Fig.2: why is the grounding line in the Siple Coast and most of Filchner-Ronne not changing at all? How does that compare to previous results?

Because presently there is little to no thinning in these areas, and since we are only applying present-day observed mass change rates, these areas remain stable. We will mention this in the discussion as 'Areas where little to no mass changes are observed in Smith et al. (2020), such as at the Siple Coast and in Victoria Land, remain stable in our simulation'

- l.340: 'sea level change equivalent' . We will add this.

- l.333: what is so special about this ridge? There is another one at about 100 km that is just as high?

In our simulations, this particular ridge is critical for the ice dynamics. Before the glacier reaches this ridge, we see mass loss comparable to the observed rate, but after the glacier ungrounds from this ridge, the mass loss accelerates. The acceleration does not slow when it reaches the ridge at 100 km. This next ridge of 100 km is in this cross section comparable to the first ridge that we identified, but less extended in the direction perpendicular to the cross section: this second ridge is surrounded by deep throughs while the first ridge is a substantial 'bedrock row' stretching almost through the entire Thwaites flow line perpendicular to the flow direction. We will add: '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.'

- l.353: 'a large ice shelf has formed instead': how stable is this ice shelf? How thick/thin is it? I don't remember seeing a mention of what is done for the calving for both models and I am wondering how this result would be impacted by the choice of calving?

Good point. The shelf is about 200 m thick, and the calving front has not moved. A couple of sentences describing the characteristics of this ice shelf will be added: 'A large confined ice shelf has formed instead, with an ice thickness of several hundreds of meters close to the grounding lines, thinning to tens of meters in the direction of the (non-moving) calving front.'

- Fig.3: Again, why just show results from CISM here? If the idea is to look at the similarities between the two models and the possible timeline of collapse, comparing results from the two models would go a long way. It would be interesting to see if the new initialization method allows to better reconcile results from the two models.

We thank the reviewer for this remark, we will add a more extensive discussion of the results of the UFEMISM runs, also as a reply to earlier comments.

- l.370: are there figures showing these results?

Yes, in the Supplementary material. We will add this figure to the main text, because we refer to it extensively

- l.376: again here, how is that impacting by the choice of calving?

A calving algorithm that moves inward with the thinning ice shelf, with more ice removal, will decrease this buttressing capacity. We will add this explanation to the manuscript.

- l.384: Which former case is discussed here? The previous sentence discussed melting caused in part due to the variability and in part to anthropogenic forcing, so it is a little unclear what former refers to.

The former case refers to the case where natural variability forcing caused the CDW to be present under the shelves. We will reword the sentence.

- l.395: Why is it incorrect? It is not clear if this is just a different evolution and how the “correctness” can be established. What are the conditions or criteria to decide whether or not this is correct.

We agree that the word ‘correct’ is not suitable in this context. We will remove the word ‘correct’ .

- l.396: “set of model choices”: which choices are being considered? There should be a clear list of choices and parameters.

The model choices are shown in Table 1, we will add a reference to that table here.

- l.410: “weaker” and “stonger” should be discussed and quantified. What parameters are changed?

This is gamma_0 in Eq 1.5, we will add the reference here.

- l.414: by how much is it delaying the collapse?

Several centuries, we will add the exact timing.

- l.415: where are these results shown?

We will show these in the new UFEMISM paragraph and figure.

- l.421: in what sense is “linear” used here? Is it the same rate of retreat? Or evolution of grounded/floating areas? And which parameters impact it?

We agree that ‘linear’ is not the right word in this context. We mean that the retreat rate is similar to what is observed today. We will clarify this point.

- Fig.4: why are only 2 results from UFEMISM shown on this figure? Also, would there be a way to organize the runs by colors and symbols according to parameters changed or something more intuitive? It is a bit difficult to read in this format. The legend mentions some broad categories but is not very clear (e.g. what does forcing refer to since there is also a basal melt category, which is a big part of the forcing).

UFEMISM will have its own paragraph where we also make clear that CISM is the main model used for this analysis and to develop this method, and UFEMISM is the model to show that the method works in another model as well, and that the WAIS collapse is not a CISM-only feature. We thank the reviewer for the suggestion on the formatting, we tried several ways of showing these results and judged this way to be the most visible one. We will add a description of which runs falls in which category.

- l.486: what about the availability of the code to reproduce the results presented and reproduce the figures presented in this manuscript?

This will be uploaded.

- Supplement movie: Clarify time in supplement movie (e.g., 10001.0, not clear what this time corresponds to).

We will change this movie and reassess its added value to the manuscript.