

Minor revisions rebuttal 'Present-day mass loss rates are a precursor for West Antarctica Ice Sheet collapse'.

Replies are written in [blue](#)

Reviewer 1

This is my second review of the manuscript "Present-day mass loss rates are a precursor for West Antarctic Ice Sheet collapse" by T. van den Akker and colleagues and I want to thank the authors for the detailed response and extensive revisions to the manuscript. I find this new version to be a lot more clear and easier to follow, which significantly strengthens the conclusions. The changes respond to most of the comments suggested and the new version is a lot more accurate. I mostly have minor comments listed below. The two things to be careful about is to add more discussion about the calving and be clear about the sliding and friction laws used, as there seems to be some mixed up here.

[We thank the reviewer for their second read of our manuscript and the detailed comments and suggestions below. The line numbers in the reviewers comment were off, so in some cases it was hard to find which sentence the readers was referring to. We added the new line numbers in the corrected manuscript in our answers below.](#)

Below are some detailed comments, line numbers refer to the manuscript with track-changes.

l.14: this should mention both global and local scales and not just local scales [we will change this to 'a global and local scale'](#)

l.18: "physics" -> "physical" [we will change this](#)

l.27: "Dynamical processes": be more specific or at least mention that it is something different from the current state (because they are already dynamic things ongoing) [we will change this to 'the resulting GMSL from non linear processes'](#)

l.56: remove "dynamical" [We will rewrite this sentence according to the suggestion of reviewer 2](#)

l.290: "with a regularized" -> "and a regularized" [This was not mentioned at line 290 but at 280, and we will change it it as 'as a regularized'](#)

l.289-295: Discuss also calving in this paragraph. [We will add the sentence 'The calving front is maintained at its present day position. This conservative approach has been chosen because calving physics as well as the impact of ice berg melange of ice shelves are still poorly understood, and overly retreating calving fronts enhance mass loss which make the results overly negative.'](#)

Table 1: put "inverted quantity in this study" in parenthesis both times [we will do this](#)

l.331: "It is based" -> "It depends" or "It is a function of" [This was found at line 158, and we will change this to 'It is a function of'](#)

l.340: "with linear" -> "with a linear" [Ln 160, and we will change this accordingly](#)

l.358: "not represented" -> "not included" [Ln 178, and we will change this to 'not included'](#)

l.383: "assumed" -> "assume" [Ln 185: we will remove the 'd'](#)

l.388: "with increasing" -> "leading to increased" [Ln 191, we will change this](#)

l.397: I think what is said here is about the water column thickness in the cavity. Ln 199: correct, we will change this to 'the water column thickness in the cavity'

l.410: "it thins" -> "the ice thins" Ln: 213. We will change this accordingly

l.486: "by model drift" -> "by model drift from the initialization" Ln 234: we will add this suggestion.

l.559: "against to an extensive" -> "against an extensive" Ln 276: we will remove 'to'

l.561: "from collapsing PIG" -> "from collapsing" Ln 278: we will remove 'PIG'

l.616: Budd and regularized Coulomb are two different sliding laws, so this is not accurate (it looks like Budd sliding according to Eq. 1.10). Ln 295: We will remove the part between parenthesis

l.750: Provide the RMSE number to facilitate comparison. We assume this is about Ln 334, we will add the velocity RMSE of 156 m/yr

Fig.2: Given the very similar patterns at year 1 and 5, and would suggest remove one of these two years and instead adding a similar figure for the UFEMISM model as the third subplot. We would like to highlight with this figure that CISM does not get the dh/dt pattern as initial 'shock' which disappears in a few timesteps, but is actually able to have this pattern persist. Adding the UFEMISM dh/dt here would distract from that. We will add to Ln 370: 'The overall pattern of the modelled mass change rates changes little during the first timesteps of a continuation simulation, highlighting that CISM is able to exhibit the observed mass change rates robustly'

Figure 4 caption: I think the location is shown on figure 3 Correct, we will change this and remove 'in the main text'

l.981: "The ice thickness change shown in Figure 7" ... it would be great to remind over which period these thickness changes happen. We will add 'after 1000 years of unfocred forward run'

Figure 7 caption: "The original" -> "The initial" we will change this

l.1011: the sensitivity of what? Ln 505: We will rewrite this sentence to 'To assess the sensitivity of our results to those choices and parameter values we varied them within the range we viewed as physically realistic.'

l.1019: "relative mass loss and resulting GMSL rise relative o the initial state": a few different initial states are used for some experiments (when some ice parameters are changed and a new spin-up is required), so it would be good to clarify which initial state is used (always the default one or the one corresponding to each experiment). I suspect it is the later, so what is the impact of using slightly different initial states? It is the latter, and the impact was small to negligible, we verified that the difference in terms of velocity, thickness and grounding line position with the original spinup was small and we performed model drift experiments to be sure the WAIS did not collapse using these slightly altered initial states. We will add to line 1019: 'For some simulations, marked with an '*' in Table 3, a new initialized stated was necessary. We made sure that these new initial states were close to the default initial state in terms of thickness and velocity RMSE and grounding line position. We performed the same model drift experiments to verify that for these new initialized states, the WAIS would not collapse without adding the present-day mass change rates. '

l.1026: I would not call that a fast phase but rather a collapse or fast retreat phase. Ln 520: We will adopt 'Fast retreat phase'

l.1066: "with about" -> "by about" Ln 543: we will add this

Figure 8 caption: "resulting" -> "equivalent" and "reach 50%" -> "need to reach 50%"

We will add this

l.1156-1157: “Previous initialization methods Compared to data assimilation initialization methods.” This sentence is rather unclear. First what is the difference between “previous initialization methods” and “data assimilation initialization methods” and second the data assimilation initialization methods are also not able to capture mass change rates in most cases. So this part is really confusion and should be removed or clarified. We agree that this sentence is confusing. We will rewrite this sentence to ‘Previous initialization methods, both the spinup and data-assimilation initialization method, were unable to accurately represent the observed mass change rates.’

l.1209: should be section 8 (not 7) We note the error and will change this to 8

l.1212: mention under present-day conditions. Ln 625: we will add this

l.1219: The last sentence is kind of a very weird way to conclude the paper, I would not end with these words. We agree, and will remove this sentence

Reviewer 2

Dear van den Akker et al.,

Thanks for including the comments, very much appreciated. The manuscript has substantially improved and I am happy with it to be published subject to minor corrections outlined below.

We thank the reviewer for the second read, and the useful, detailed and constructive comments below. Our answers are given in blue

Reply on comment on abstract, line 13 (reviewer 2): To derive the statement from Aschwanden et al., 2019 that models struggle to reproduce present-day rates of ice loss is unfair because models were never asked to try and reproduce present-day ice loss in ISMIP6 project (which the Aschwanden study is based on). The initialisation was left to the modellers. Hence, this does not show that models cannot reproduce trends. A valid conclusion that can be drawn from Aschwanden et al. and other studies is that “Recent studies highlighted the need for ice sheet models to be benchmarked against reproducing present-day, observed mass change rates.” I suggest you simply change the sentence to this statement. Thanks for this constructive point, we will change Ln 13 as suggested

Line 31, new manuscript: definition of “dynamical irreversible retreat (i.e., the recovery time is substantially longer than the response time)” – not only that, but you have hysteresis behavior. Once retreated, the system is not expected to recover even if the climate forcing is reduced below the pre-collapse value – in your response this was stated as “i.e. the glacier will not return to its present grounding line position when the forcing is removed”. Maybe go back to this?

We will change this back to the response of the first rebuttal, as suggested

Line 41, new manuscript: “model studies” neither Jenkins et al., 2016 nor Thoma et al. 2008 model the response of PIG ice dynamics to CDW intrusions (none employs an ice sheet model). We will remove the word ‘model’

Line 52, new manuscript: “while others suggest that the two glaciers will require additional forcing to collapse (Feldmann and Levermann, 2015; Arthern and Williams, 2017; Reese et al., 2023; Garbe et al., 2020)”. This seems to be a misunderstanding of the authors of these studies: neither Arthern & Williams nor Feldmann & Levermann make statements that additional forcing for collapse is needed. Reese et al., 2023 even finds collapse under current climate in line with the studies cited in the sentence before, Garbe et al., identifies the tipping point between 1 and 2 degree above pre-industrial levels, which again includes the current climate state. Reformulate to “A number of recent 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; Reese et al., 2023; Garbe et al., 2020).” This will impact the following sentences as well.

We will rewrite this to: 'A number of recent studies suggest that TG and PIG are unstable under the current climate and could collapse on a timescale up to 2000 years (Garbe et al., 2020; Golledge et al., 2021; Reese et al., 2023; Coulon et al., 2024). Lipscomb et al. (2021) projected accelerated retreat leading to a collapse only when ocean thermal forcing increases by 1–2 K relative to a preindustrial or 20th century equilibrium. However, there are few historical observations of Southern Ocean temperatures, so it is unknown how much the ASE has warmed in the past century, and if the warming needed to drive such a retreat has already happened.'

Line 63, new manuscript: “These models often struggle to represent the observed mass change of recent decades, because the result of a steady state initialization is a stable ice sheet without model drift” – instead I would say “Since they are initialised to a stable ice sheet without model drift, they do not represent the observed mass change of recent decades.” We agree to this comment and will replace the mentioned text by the suggestion

Reply on comment on Line 85 (reviewer 2): Sorry for the jargon, with PSU I meant the Penn State Model developed by Dave Pollard. Force-to-thickness I meant a flux correction as described in Aschwanden et al., 2016 “Complex Greenland outlet glacier flow captured.” We thank the reviewer for this clarification

New figure 1: Panel (d) shows several locations along Thwaites grounding line where your inversion derives “zero” melt, is that correct? That is correct. These are locations where CISM underestimates the ice thickness, and the basal melt scheme tries to compensate that by lowering the basal melt rates. Basal melt rates below zero are not represented in CISM.

New figure 2: This is a CISM run, right? Figure 2a are the observations, figure 2b and c are the mass change rates modelled by CISM after 1 and 5 years of running forward. We will add to the title ‘Observed and modelled present-day mass change rates’

New line 448: Holland et al., 2019, did not model things, but analyse model results. We will remove ‘model’

New figure 7: Change “future state” in the caption. [We do not see a fitting alternative, and we would like to keep the caption as close to the caption of Figure 3 as possible](#)

New line 611: You could reformulate to “Replication of our experiments with other ice sheet models could substantiate whether the collapse of Thwaites and Pine Island Glaciers is indeed unavoidable without a considerable reduction of basal melt as indicated in our study.” and remove the last sentence. [We agree to this suggestion and will remove the last sentence and change the second to last sentence as suggested.](#)