Review of a manuscript "The stability of present-day Antarctic grounding lines Part A: No indication of marine ice sheet instability in the current geometry" by Urruty et al.

This is a revised version of an earlier submitted manuscript. I thank the authors for engaging with my comments. Some of them have been answered. However, there are several issues that still have to be resolved and clarified on both conceptual and presentation levels before the manuscript can be published. In general, the manuscript is still written in terms of "stable/unstable" ice-sheet geometry irrespective whether the climate conditions (surface accumulation and submarine melting) are changing with time or not; and the general thrust of the paper is still focused on the ice-sheet geometry with unclear and somewhat confusing description what has been done to the climate conditions and modifications to them. To this reader, there is a disconnect between what has been done in the study and how the study steps and the results have been interpreted and described. What follows below, first addresses conceptual aspects, and then the presentation aspects (in some places they're mixed together).

In very broad terms, my understanding of what has been done is the following:

1. The observed present-day geometry of the Antarctic Ice Sheet is assumed to be a steady-state one or close to a steady state. This geometry and observed velocities are used in inversion procedures in Elmer/Ice and Úa to construct basal conditions.

2. The climate conditions (the RACMO surface accumulation and PICO submarine melting)[1] are modified in such a way that when the constructed field is used in the mass-balance equation, the resulting changes of the ice thickness with time h_t are fairly small or close to zero (depending on the model). Let's call this created field $\dot{A}_{ar}$ – "alternative reality" climate conditions.

3. In $\dot{A}_{ar}$, the submarine melting component is modified again by applying "small perturbations" for 20 years; after that these "small perturbations" are removed and models run for another 80 years.

4. The temporal evolution of the ice flux through the grounding line and the grounding line position simulated during these 100 years are used to establish whether the constructed steady state is stable or unstable.

If this is incorrect then clearly the manuscript does not convey what steps have been taken and it needs to be completely rewritten. Assuming that it is indeed what has been done, the text needs to modified to accurately describe these steps, the assumptions that were made at the outset, and interpretations of the results.

Among these steps, the central one is step 2 – the construction of the field $\dot{A}_{ar}$. This field is such that, if it is held constant in time, the present-day ice-sheet geometry is in (or close to) a steady state with respect to it (at this point, it does not matter whether this steady state a stable or unstable).

[1] I appreciate that there are inconsistencies between the observed geometry and ice velocities, and that RACMO and PICO are not the actual surface accumulation and submarine melting, and the need for what the authors call the "relaxation", which also contributes to the field $\dot{A}_{ar}$.

Observations show that the present-day surface elevation, ice thickness and the grounding line positions change with time, hence, the present-day geometry is not in steady state with respect to the present climate conditions. If it were, then according to the mass balance $\vec{\nabla} \cdot (uh) = \dot{A}_{pd}$, and $h_t = 0$, where $\vec{v}$ is the ice velocity, and $\dot{A}_{pd}$ is the present-day ice-sheet mass balance. Because the observed $h_t \neq 0$, the present-day conditions differ from the constructed $\dot{A}_{ar}$. Consequently, the authors need to a) articulate this point that they have constructed an "alternative reality" climate conditions; and that each model has its own "alternative reality" climate (as figs. S14-S15 indicate); b) clearly describe how $\dot{A}_{ar}$ have been constructed for each model; and preferably c) discuss how different they are from the present-day climate. Although, supplemental figures S14 and S15 show the mass-balance correction terms for Elemer/Ice and Úa, they do not address point c). These plots do not show similar fields for PISM; the colors in panels showing Úa results are oversaturated suggesting that these corrections are much larger than the colorbar limits of ± 2 m/yr.

With regards to point b), the current description (lines 156-177) is not clear, especially with both RACMO, which is the surface accumulation/ablation, and PICO which is submarine melting denoted by the same variable $\dot{b}$. It would be beneficial for the manuscript to have figures showing $\dot{A}_{ar}$ for each model in the main text and either absolute or relative differences between $\dot{A}_{ar}$ and RACMO and PICO fields (essentially h_t terms). Although the authors point out that the imposed mass-balance corrections have small magnitudes and are a small fraction of the total present-day mass balance (line 458), it is not only their total value, but the spatial distributions of those corrections that matters. This is because, as fig. S14 shows, these corrections are both positive and negative, and when integrated over the whole ice sheet, their contributions cancel each other. So it seems to me that the constructed $\dot{A}_{ar}$ is indeed quite "alternative reality" climate.

The description of how perturbations for PICO fields constructed for the step 3 (lines 257-275) is confusing, especially with inline formulas and too many terms having very similar notation $\dot{b}_{PICO}$. Throughout the text these perturbations are called "small", however the changes of the ocean temperature 5°C or even 1°C are hardly could be described as "small". The extra energy supplied to the ice shelves due to such an increase in the ocean temperature is $\Delta Q = C_p m \Delta T$, where C_p is the sea-water heat capacity. The change in the air temperature that would correspond to this amount of extra heat ΔQ would be by about four times larger (assuming the same unit mass of air and water). This is because the air heat capacity is about four times smaller than the sea-water heat capacity. Thus, the corresponding air-temperature changes would be of the order of 4-20°C. This is well above any high-end projections of the climate warming. I appreciate that the magnitudes of perturbations have to be large enough to cause the grounding lines to move, and that they were also applied for a short time of 20 years, but still, they have to be physically reasonable. Some re-wording or clarifications for these values are needed. Perhaps, it might be better to cast these applied perturbations in terms of enhanced submarine melt-rates expressed in m/yr, rather than the ocean temperature changes. The need for such large changes in the ocean temperature suggests to me either low sensitivity of the models to changes in the submarine melting (if the changes in melt-rates that correspond to these changes in the ocean temperatures are large) or issues with PICO parameterizations. At least something needs to be said about the "smallness" of these perturbations.

It is also not clear how these perturbations have been applied. I suspect that for the ice shelves the mass balance was

$$h_t + \vec{\nabla} \cdot (\vec{v}h) = \dot{A}_{ar} - \Delta\dot{b}_{PICO}, \quad (1)$$

and for the grounded parts it was

$$h_t + \vec{\nabla} \cdot (\vec{v}h) = \dot{A}_{ar}. \quad (2)$$

However, this is not clear from the description.

With regards to the presentation and description of the results (step 4), I am not sure that the detailed regional analysis adds value to the main results of the study which are a) there exist "alternative reality" climate conditions specific to each model, for which there are steady-state modeled geometries that are close to the present-day ice-sheet geometry, and b) these modeled steady states appear to be stable. I leave it up to the authors to decide whether to keep or remove it, but it'd be easier to read the manuscript if these parts of the text would be expressed more succinctly.

Moving on the presentational aspects, it appears that there is still a confusion between the effects of time-variant and steady-state climates on the grounding lines. The abstract starts with the sentence "Theoretical and numerical work has firmly established that grounding lines of marine-type ice sheets can enter phases of irreversible advance and retreat driven by the marine ice sheet instability (MISI). " However, theoretical and numerical work has firmly established that grounding line can equally not enter phases of irreversible advance and retreat (e.i., be stable to small perturbations). Without this second sentence, the first sentence gives an impression that the irreversibility is the only option for the grounding lines. The last two sentences "...his suggests that if the currently observed mass imbalance (external climate forcing) were to be removed, the grounding-line retreat would likely stop. However, under present-day climate forcing, further grounding-line retreat is expected, and our accompanying paper (Part B, Reese et al., 2022) shows that this could eventually lead to a collapse of some marine regions of West Antarctica." indicate that the authors view only the mass imbalance as the climate forcing, and not the temporal variability of the surface accumulation/ablation and submarine melting. Were the authors to use their constructed "alternative reality" climate conditions $\dot{A}_{ar}$ and apply temporal variations to them (even without long-term trends), they might find the grounding line behavior significantly different from the one they obtained in this study. It is unclear what is the basis of the statement that "under present-day climate forcing, further grounding-line retreat is expected". What is the reason for such an expectation?

Despite changes made to the introduction section, it continues to rely on the bed slope as an indicator of stability. The authors added a statement that "the retrograde sloping bed is a necessary conditions". This statement is incorrect. None of the stability conditions derived by Haseloff and Sergienko (2022) and Sergienko and Wingham (2019, 2022) has a necessary condition for an unstable steady state configuration to have a retrograde sloping bed. If the authors disagree, they need to support their statement with mathematical derivations demostrating that these stability conditions indeed have such a necessary condition. The authors' statement about the retrograde slopes is a widely held misconception that stems from the stability condition for a configuration with very specific conditions derived by Schoof (2012). These conditions are the absence of the lateral confinement, very smooth beds with negligible bed slopes and the smallness of the accumulation/ablation rate at the grounding lines. Only if these conditions are simultaneously satisfied than indeed the stability condition is reduced to a condition on the sign of the bed slope (Haseloff and Sergienko, 2022; Sergienko and Wingham, 2022). However, there are no locations in Antarctica or Greenland for which all these conditions are satisfied. In addition to that, in the presence of feedbacks between the ice sheet and climate conditions there are no general stability conditions that can be related to steady-state properties (Sergienko, 2022). There is no need to promote this misconception that the retrograde beds are the necessary condition for instability; so the rest of the introduction needs to be modified accordingly.

The provided reason why the present-day Antarctic Ice Sheet is not in a steady state (lines 72-73) is incorrect as well. The ice sheet is not in a steady state not because of its response to varying climate forcing is long-term, but because the climate forcing itself varies in time, and because this climate forcing is never in steady state. In their experiments, the authors keep their constructed "alternative reality" climate conditions constant to maintain steady-state configurations. This never happens with the real climate conditions, they always vary on a wide range of temporal scales. It appears that the authors confuse steady states with equilibrium states. In the latter ones, the climate can vary with time, and if ice sheets vary in such a way that their mass gains balance their mass losses (both controlled by these time-varying climate conditions), then the ice sheets are in equilibrium with their climate. However, such an equilibrium is not a steady state, because the climate conditions vary with time. Weertman's (1974) analysis, Schoof's (2012), Haseloff and Sergienko (2022) and Sergienko and Wingham (2022) stability conditions apply to steady states only, and are not valid to equilibrium states. Currently, there is no theoretical analysis of the ice sheets which are in equilibrium with their time-varying climate conditions.

In the discussion section, comparisons with the results of other studies appear as attempts to reconcile the conclusion of this study that the grounding line is stable with conclusions of those studies that it is unstable. Because the climate conditions used in this study are vastly different from climate conditions in other studies comparison of the results is similar to comparing apples and oranges.

In summary: the manuscript needs a very clear description what has been done in the study; description of the constructed climate conditions and discussion how different they are from the present-day ones, and not only in the integral magnitude expressed in Gt/yr, but the spatial patterns as well; clarifications of the concepts of steady states and their stability that reflect the present level of knowledge; and description of the results in the context of the design of the study.

I would like to reiterate that the study has produced interesting results and it would be a pity if the manuscript describing them would misinterpret and misrepresent them.

References:

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