We thank the Editor Claudia Timmreck and the three reviewers for their careful evaluation of our manuscript. We found the comments very useful and think that our manuscript will be greatly improved thanks to them. To ensure clarity, the reviewer's comments are written in black and our responses in light blue.

Reviewer #3

Summary

The study by Caillet et al. quantifies the uncertainties in the projected Antarctic contribution to sea-level change by 2100 related to internal climate variability in a subset of CMIP6 models. Three CMIP6 models are selected based on a summary of previous evaluations (Purich and England, 2021; Beadling et al., 2020; Heuzé, 2021; Sect. 2.1). The Antarctic sea-level contribution is projected with the stand-alone ice-sheet model Elmer/Ice; the respective experimental setup is presented in Sect. 2.2 and Sect. 2.3. First, internal climate variability in the selected CMIP6 models is explored (Sect. 3.1 and Sect.3.2). Then, the Antarctic contribution to sea-level change projected by Elmer/Ice based on different ensembles members for the selected CMIP6 models is presented (Sect. 3.3). The authors quantify the effect of internal climate variability on the Antarctic sea-level contribution by the end of this century with 45% to 93%, with a higher impact from atmospheric variability compared to ocean variability, and modulated by the CMIP6 model. Results are discussed in terms of the robust representation of internal climate variability in CMIP models (Sect. 4.1), the internal climate variability as a source of uncertainty in Antarctic sea-level projections (Sect. 4.2), and identifying best ensemble members as an alternative approach to account for internal climate variability in sea-level projections (Sect. 4.3). The authors conclude with general recommendations for future assessments of the Antarctic contribution to sea-level change (Sect. 5).

General comments

By bringing together internal climate variability and the future evolution of the Antarctic Ice Sheet, the paper addresses a relevant and scientific interesting question, that has rarely been explored in previous assessments of the future trajectory of the Antarctic Ice Sheet. The presented results and related discussion may be valuable for future assessments of the Antarctic contribution to sea-level rise. The title clearly reflects the contents of the paper. The abstract provides a concise and complete summary. Overall, the study has a sound methodology and experimental setup. In some cases, the description of the methods could be more precise, and the clarity of language improved. While the results span a wide range from exploring the representation of internal climate variability in selected CMIP6 models to the modelled response of the Antarctic Ice Sheet, the manuscript may benefit from linking both in greater depth (e.g. explaining the climate model - dependence of the impact of climate variability on the projected sea-level contribution, ranging between 45% and 93%, if possible). Here, additional figures, e.g. in a Supplementary Material, may be helpful for the reader.

We will work on section 3.3 to better stress the links between the analysis of internal climate variability in the CMIP6 models and the projected Antarctic sea-level contribution.

In addition, the discussion on a possible selection of best ensemble members from CMIP models could be better integrated in the manuscript.

For a better readability and following suggestions by the other reviewers, the discussion section on the identification of the best member will be revised. Figure 8 and the list of associated metrics will be moved to the appendix and the same analysis will be perform for UKESM-0-LL to get more robust conclusions.

The reasons for choosing the best member will also be better explained. First, ice sheet models need to be initialised and calibrated to match historical observations. Achieving this would in theory be easier with a forcing from the most realistic CMIP ensemble member, which is why we attempted a selection of the best member. Another reason is that ice sheet simulations can be computationally expensive, and running simulations forced by all members of several CMIP models may not be feasible.

Finally, some additional explanations may be needed to directly derive and support some of the recommendations given in the conclusion based on the results presented in this study.

Based on the comments of all the reviewers, we decided to only keep the two recommendations that are the most motivated by our findings, and we will make sure that the link with our findings is explicit.

We will remove the recommendation on coupled ice-sheet/climate models as it is not clearly demonstrated in our study. Instead, the discussion on ice-sheet/climate coupling will be addressed in section 4.1 which deals with the issue of the robustness of internal variability in climate models. The recommendation regarding initialisation will also be removed, as the topic of initialisation is not directly addressed in the paper.

I have included more specific comments, questions and suggestions below.

Specific comments

L12-14: In the abstract, the results of the sea-level projections are summarized before describing the internal climate variability in the CMIP6 models. Maybe it would be more intuitive to follow the same order as in the main text (that is, internal climate variability in CMIP6 models followed by sea-level projections)?

We will rearrange the abstract to match the structure of the paper.

L12-14: Maybe a brief remark on the upper end of the amplitude of oceanic internal variability covered by different climate models could be added, in addition to the mentioned and explained weak mid-depth ocean variability?

We will add the range of the ratio between the amplitude of internal atmospheric variability and internal oceanic variability for the various models. This ratio varies from 1,8 for IPSL-CM6A-LR to 4,8 for MPI-ESM1.2-HR.

We will also temper our statement for West Antarctica, where the amplitude of oceanic and atmospheric variability may be similar depending on the CMIP model.

L15: Please specify 'use of several members in the run and its initialisation'. I think I understand what is meant here after reading the manuscript but this phrase may be unclear for the reader when starting with the abstract.

Based on the general comments described above, we have reformulated the recommendations by focusing only on those that are really motivated by our results, i.e., (i) the use of several members of the same model and several models since internal variability does not have the same amplitude depending on the models and (ii) the use of longer reference period. We suggest the following sentence « *Based on these results, we recommend that ice sheet model projections consider (i) several climate models and several members of a single climate model to include the impact of internal climate variability and (ii) 50-year averages for reference period from which anomalies are estimated to attenuate any unexpected shift triggered by internal variability.* »

L24: Maybe add 'estimates of' or 'projections of', e.g. 'Estimates of the AIS contribution to future sea level rise are currently based...'

Yes, the manuscript will be corrected accordingly.

L25: I think CMIP stands for Coupled Model Intercomparison Project. Please check.

Thanks, you are completely right. The manuscript will be corrected accordingly.

L31-35: In this paragraph climate variability is introduced as consisting of two components (1) variability from natural and anthropogenic external forcings and (2) internal variability, and explanations for these components are given, after having referred to internal climate variability in the previous paragraph. Maybe some restructuring is possible to define internal climate variability with its first use?

Internal climate variability is already defined in the abstract.

In the introduction, internal climate variability is used for the first time without having been introduced (L28-30), but its definition is provided in the following sentence (L31). We therefore prefer to keep these things unchanged.

L49/50: Maybe 'the Antarctic Sea Level Contribution'?

Yes, the manuscript will be corrected accordingly.

L50: Why did you chose the SSP2-4.5 emission pathway? Please add a short explanation either here or in Sect. 2.3.

We will clarify the choice of scenario in §2.3 rather than at the end of the introduction as follows « *We use the medium SSP2-4.5 scenario, which corresponds to a global warming of 1.4 to 3.0°C from 1995-2014 to 2081-2100 (90% confidence interval, Lee et al. 2021) and seems the most representative of current effort to tackle climate change (Riahi et al., 2017). As the choice* *of greenhouse gas emission scenario has only a limited impact on the projected Antarctic contribution to sea level rise until 2100 (Seroussi et al., 2020), we have not repeated our calculations for other scenarios. »*

L56: 'drivers' instead of 'driver'?

Yes, the manuscript will be corrected accordingly.

L58-64: Please specify the properties that are used to evaluate the CMIP6 models. Some of them are given in Figure 1 or its caption, but it may be helpful to also include them in the main text.

- What properties of ASBW and CDW are evaluated? Please add this information also in the main text.

- Many dynamical features for the Southern Ocean are listed in the legend of Figure 1b. It might be helpful for the reader to better link the legend and caption of Figure 1 to the description in the main text. This applies to e.g. the ocean properties that are evaluated in terms of their meridional gradients.

- What bottom properties of the Southern Ocean are evaluated? Maybe add this information also in the main text.

Figure 1 will be moved to Supplementary Material and a table summarising the variables and metrics used for the evaluation will be added.

In the main text, we propose to remove the list and to replace it with a sentence including both atmospheric and oceanic properties evaluation « *The selection of models was first based on the number of members available and on the availability of 6-hourly outputs that were needed to run regional climate projections. It was also based on the model ranking for Antarctic atmospheric metrics proposed by Agosta et al. (2024), and on the model ranking for Southern Ocean metrics provided by the review of three studies which evaluate water masses properties in the Southern Ocean and Antarctic seas (Purich et al., 2021), dynamical properties in the Southern Ocean (Beadling et al., 2020) and bottom properties in the Southern Ocean (Heuzé, 2021). »*

L58-64: To facilitate readability, bold or italic fonts for some phrases in this list could be used (e.g. for the evaluated water masses). As an alternative, these properties could be given in a table rather than in a list.

See the response above.

L65: Is the assessment presented in Figure 1 based on one ensemble member of the respective CMIP6 models or an average over all available ensemble members? As different ensemble members are used later in the manuscript, maybe add this information here (or in the figure caption) to avoid confusion.

Thanks for the suggestion. The assessment presented in Figure 1 is only based on the first member of each CMIP6 model. We replace the sentence of L65 by *« A summary of this* *assessment is provided in* Supplementary Material *based on the analysis of the first member of each climate model. »*

Figure 1 will be moved to Supplementary Material.

L66: How is 'best' defined? Does UKESM-1-0-LL have one of the lowest RMSE in all three studies? Maybe state this more explicitly here.

For each study, we calculate the RMSE between the CMIP6 model and the observational dataset with which it is compared, for all the variables analysed. The CMIP6 models are then ranked by increasing RMSE.

We will extend the description of ocean properties analysis a bit further than the response to L58-64 by summarising the metric used, the reference dataset to which it is compared and the period over which they are evaluated.

L68: If I understand Figure 1 correctly, MPI-ESM1.2-HR was evaluated in two of three studies (red triangles in Figure 1a and c). Please check.

Yes, MPI-ESM1.2-HR was evaluated in only two of the three studies.

L70-73: This is a very general sentence, in particular for readers not familiar with the representation of ice-shelf melting in CMIP models. What is meant by 'some kind of prescribed ice-shelf melting at depth'? Does this impact the assessment / ranking of CMIP6 models in Figure 1? I think it may be helpful to briefly discuss the link between the treatment of ice-shelf melting and the CMIP6 model assessment, if this information is mentioned here.

Prescribed meltwater flux at depth has an impact on the properties of the water masses and sea-ice. This inflow affects the stratification of the water column and can be more favourable to convective mixing by reducing the density at depth (Mathiot et al., 2017) and to the intrusion of circumpolar deep waters (Haigh et al., 2024). In contrast, models that prescribe meltwater flux only at the surface tend to increase the stratification of the water column and reduce exchanges between the surface and deeper waters, thereby preventing variability.

We will remove the word 'have some kind of' and replace with *« It is also interesting to note that both UKESM1-0-LL and IPSL-CM6A-LR have prescribed vertically distributed ice-shelf melting to ensure the conservation of the ice-sheet mass over the entire simulation, which is known to be important for coastal ocean properties around Antarctica (Mathiot et al., 2017; Donat-Magnin et al., 2021). Most of the CMIP models prescribes meltwater flux only at the surface, which tend to increase the stratification of the water column (Mathiot et al., 2017) and reduce exchanges between the surface and deeper waters, thereby preventing variability. »*

L74-77: Please add more detail on the assessment of atmospheric properties for the CMIP5/6 models in the manuscript, also given that Agosta et al. (2022) refers to a conference abstract. For example, which atmospheric properties have been evaluated and which method is used for the assessment?

For further explanation on the evaluation of atmospheric properties, the reader may now refer to Agosta et al, (2024) which has just been referenced : https://doi.org/10.5281/zenodo.11595213.

The comparison of atmospheric properties with ERA5 has been expanded to rebalance the explanations of atmospheric and oceanic properties in the selection of the CMIP6 model. We will reshape the existing paragraph as follows *«Agosta et al (2024) evaluate 29 CMIP6 models around Antarctica by comparing their performance with the ERA5 reanalysis over the period 1980-2004 for 9 variables. The models are ranked based on two metrics, which are (i) the mean Root Mean Square Error (RMSE) over the 9 variables normalised by the multi-model RMSE and (ii) the second maximum implausible fraction, which corresponds to the fraction of the surface where the difference between CMIP6 models and ERA5 is greater than three times ERA5 standard deviation. »*

Figure 1: Please consider marking the selected CMIP6 models in a different way, e.g. by colouring the model name or adding a box around the model name. The red triangles can be easily confused with the other markers (or appear within the legend, compare Figure 1b).

Yes, the manuscript will be corrected accordingly.

Figure 1: Please briefly introduce the abbreviations used in the legend, e.g. in panel b in the figure caption and / or in the main text (L58-64).

Yes, the manuscript will be corrected accordingly. We will add a table summarising the variables and metrics used for the evaluation. Figure 1 and the corresponding table will be moved to Supplementary Material.

L82: Please add a reference for the friction law.

As this comment appeared several times, we will add the law in the main text for clarity as follows *« The ice dynamics is computed by solving the Shallow Shelf Approximation (SSA) of the Stokes equations (MacAyeal, 1989), assuming an isotropic rheology following Glen's flow law (Glen, 1955) and a linear friction law (i.e.,* $\tau_b = C\,u_b$ *where* τ_b *is the basal shear stress,* C *the friction coefficient and* u_b *the basal ice velocity). »*

L84: What do you mean by 'preferentially refined'? Please specify.

We will remove the word 'preferentially'.

L85: What do you mean by 'high curvatures'? Please clarify.

Curvature is the second derivative of the modelled fields (velocity and ice thickness here) i.e., the Hessian matrix. For more explanation, the reader can refer to §2.2 of Gillet-Chaulet et al. (2012):

Anisotropic mesh adaptation is now widely used in numerical simulations especially with finite elements, as it allows to refine the mesh where needed to capture the flow features within a certain accuracy without increasing the computational cost excessively. The method is generally based on an estimation of the interpolation error used to adjust the mesh size so that the discretisation error is equally distributed over the whole domain. It can be shown that an estimate of the interpolation error induced by the meshing is obtained from the Hessian matrix of the modelled field, allowing to define an anisotropic metric tensor at each node (Frey and Alauzet,

In the main text, we will add a parenthesis « *The mesh is preferentially refined both close to the grounding line and in areas where observed surface velocities and thickness show high curvatures (i.e., high second derivative of the modelled field, Gillet-Chaulet et al.,2012) ...* »

L100-103: Are the ocean temperature corrections to match observed melt rates also based on Reese et al. (2023)? I assume that these may differ from the corrections presented in Reese et al. (2023) given the use of a different ocean climatology here. Please describe how the temperature corrections applied here are derived. It may also be helpful for the reader to briefly mention why temperature corrections are applied (instead of e.g. changing the PICO parameters to match present-day observed melt rates).

We used the set of parameters defined in Reese et al., (2023) which is based on the sensitivity of melt rates to ocean temperature changes obtained from both observations and numerical ocean projections. Ocean temperature corrections are not based on Reese et al., (2023). We have carried out our own temperature correction because (i) the climatology is different (ISMIP6 climatology in our paper instead of climatology from Schmidiko et al. (2014) in Reese's paper) and (ii) the ice-sheet geometry in our Elmer/Ice configuration resulting from an inversion initialisation is quite different from the geometry of Reese's model resulting from a long spin-up initialisation. Our aim is to match the observational estimates from Adusumilli et al., (2020) for all ice shelves over the average period 1995-2014 (see Figure 2) by applying temperature corrections between -2°C and 2°C (step of 0,1°C) in each basin defined in Reese et al., (2018).

In the main text, we now indicate:

- that the set of parameters are based on observations and ocean models: « *Here, the parameters are those detailed in Reese et al. (2023), i.e.,* $C = 2$ *Sv* $m3\ kg^{-1}$ *and* $\gamma T = 5.5\times10^{-5}\ m\ s^{-1}$*, which are based on the observed and ocean-modelled sensitivity of melt rates to ocean temperature changes.* »
- the reasons that led us to carry out our own temperature correction: « *A correction of temperature, ranging from -1.8°C to 0.6°C with respect to the ocean climatology, is added to match the 1994-2018 melt rates estimates from Adusumilli et al. (2020) (see Fig. 2). This correction differs from Reese et al.,*

(2023) as the current ice-sheet geometry and the oceanic climatology used in this study are different from the one considered in Reese et al. (2023). »

L106-107: Why is a 10 % reduction of the inverted friction coefficients applied? Is this based on testing, a 'best fit' or some other methodology? Does the reduction of the friction coefficients change the modelled velocities (as this quantify has been the target of the inversion)?

We minimise the RMSE between the modelled and the observed ice-sheet mass change for West Antarctica by applying reduction of the friction coefficient, to limit the model drift. A proper inversion is done to obtain the initial basal friction coefficients. Then, these coefficients are adjusted by trial and error to limit the model drift.

We will rephrase as: *« In contrast to (Hill et al., 2023), we do not correct the surface mass balance to maintain a steady state, but we apply a 10% uniform reduction of the inverted friction coefficients to reduce the model drift. For this, we minimise the RMSE between the modelled and the observed ice-sheet mass change (The IMBIE Team, 2018) for West Antarctica. Our configuration overestimates the mass loss trend in the West Antarctica by only 6% but still largely overestimates mass gain in East Antarctica and in the Peninsula (Tab. 1). »*

The friction coefficient correction does not significantly impact the ability of the model to reproduce the observed velocities. The initial RMSE between modelled and observed velocities was around 40 m/yr and increased by around 10 m/yr due to friction coefficient correction.

L107-108: The ice-sheet model configuration slightly overestimates mass loss in West Antarctica (when compared to the uncertainty ranges of the observations) if I understand Table 1 correctly.

We will remove the word 'correct' and articulate the sentence more clearly *« Our configuration overestimates the mass loss trend in the West Antarctica by only 6% but still largely overestimates mass gain in East Antarctica and in the Peninsula (Tab.2). »*

L108-109: Can you maybe add a brief remark (or a figure) on how large the trend bias in the ice-sheet model setup is?

We will add *« In general, our configuration is currently gaining a little mass (+36 Gt/yr, Tab. 2), instead of losing mass (-109±56 Gt/yr, Tab. 1). »*

L109-110: Please specify the reference that your results (in terms of the Antarctic sea-level contribution) are compared to. Are the projections in response to the CMIP6 climate models analysed relative to each other? Do you substract from a control experiment to remove the drift? After reading the discussion, I think the trend is not removed.

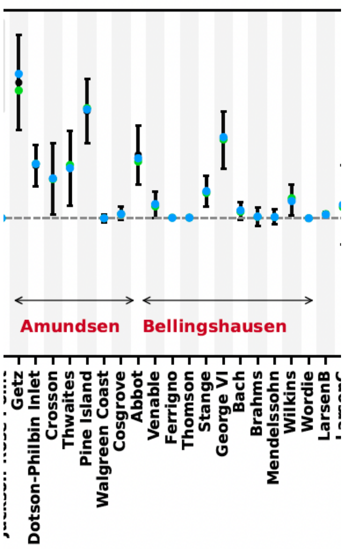
The projections in response to the CMIP6 climate models are analysed relative to each other. We will replace *« This is why our results are primarily analysed in relative terms. »* with *« However, this bias should not impact most of the analyses presented here, as the projections in response to the CMIP6 climate models are analysed relative to each other. »*

L112-114: This formulation might be confusing for some readers.

We will rephrase as « *Antarctic future mass change results from combined effects of surface mass balance and dynamical changes. In standalone ice-sheet simulations, variations in surface mass balance can be attributed to atmospheric-forced changes and dynamical mass loss to ocean-forced changes as SMB changes have little impact on the Antarctic sea-level dynamical contribution over a century (Seroussi et al., 2014; Seroussi et al., 2023). Thus, the effect of atmospheric and oceanic variations on Antarctic sea-level contribution can be analysed separately and then summed to reconstruct the combined effect (Bindschadler et al., 2013).* »

Figure 2: It may be helpful to indicate the most relevant ice shelves in a map (as already done for the Antarctic basins in Figure 7).

Instead of a map, we suggest adding an indication of the main regions (Amundsen, Aurora and Bellingshausen) in red on the Figure 2, like this:



L115-116: I got confused by the focus on the ocean here. Do you also run projections with ocean forcing only?

Given that SMB change has a limited impact on the dynamical sea-level contribution, the Antarctic global sea-level contribution can be calculated as the sum of the dynamical contribution (modulated by oceanic variability) and the SMB contribution (modulated by the atmosphere variability).

On the one hand, the SMB seal-level contribution is directly deduced from the emulated SMB (cumulative sum of the SMB - initial SMB in 2015). On the other hand, to investigate the dynamical sea-level contribution, we run Elmer/Ice simulations driven by the SMB of the first member of selected CMIP6 models and the ocean of several members of the selected CMIP models from 2015 to 2100 and we then remove the SMB contribution of the first member.

We will add *« On the one hand, the SMB seal-level contribution is directly deduced from the emulated SMB (i.e., cumulative SMB – initial SMB). On the other hand, the dynamical sea-level contribution is investigated through Elmer/Ice simulations driven by the SMB of the first member and the ocean of several members of the selected CMIP models. We then remove the SMB contribution of the first member to deduced the dynamical contribution. »*

L116: I am not sure if I understand what is meant by 'constrained'. Maybe consider replacing by e.g. 'driven' or 'forced', if applicable.

'driven' will be used in the revised manuscript.

L118-122: Please add more details on the selection of the CMIP6 ensembles members. I am not sure which section you are referring to for additional information on the selection, based on covering a wide spread in (1) possible ocean temperatures in the Amundsen Sea Embayment and (2) surface mass balance. Is the focus on the Amundsen Sea Embayment motivated by observed present-day mass loss in this region? Why is a different number of ensemble members chosen for each CMIP6 model? I appreciate the assessment of CMIP6 models in Figure 1, but, if I understand correctly, this evaluation justifies the choice of the CMIP6 model rather than the individual ensemble members for driving Elmer/Ice. It might be helpful for the reader to stress the link between the CMIP6 model evaluation, the assessment of the internal climate variability for these CMIP6 models and the selection of a subset of ensemble members for driving Elmer/Ice.

We will replace the paragraph with *« Because of the numerical cost of our simulations, we select a limited number of members. The selection is made over the current period (1995-2014 means) to cover the widest range of values for the ocean temperature on the continental shelf in the Amundsen Sea. We focus on this region as (i) observed present-day ocean-induced mass loss and (ii) the amplitude of the across member standard deviation of the 1995-2014 mean potential temperature are among the highest around Antarctica (see Fig. 3d-f, j-l).*

We selected 5 members for each CMIP6 model, i.e., the two members with the coldest temperatures and the two members with the warmest temperatures in the Amundsen Sea continental shelf (Fig. 3j-l), for which the SMB is available, as well as member 1, used by default in most of the studies. In total, we run 11 simulations, five with the IPSL-CM6A-LR model (r1i1p1f1, r3i1p1f1, r6i1p1f1, r11i1p1f1, r25i1p1f1, see https://goo.gl/v1drZl for CMIP6 convention name of ensemble members), four with the UKESM1-0-LL model as member 1 is already included in the temperature criteria (r1i1p1f2, r2i1p1f2, r4i1p1f2, r8i1p1f2) and two with the MPI-ESM1.2-HR model (r1i1p1f2, r2i1p1f2). As the oceanic variability is very low in MPI-ESM1.2-HR (Fig. 3f), we retained only member 1 and another member to verify the low impact of oceanic variability on the dynamic contribution. »

We took into account all the members available for the SMB contribution.

L120-122: I am not sure how familiar readers are with the CMIP variant labelling. While it may not be necessary to explain it in full detail, it may be helpful to briefly state that these lists describe different ensemble members for each of the CMIP6 models.

These abbreviations are a CMIP convention and are a brief description of the experiment. rX corresponds to realization index (i.e., the member number), iX to initialisation index, pX to physics index and fX to the forcing index.

We will add, in the main text, a link to a URL (https://goo.gl/v1drZl) that describes the CMIP6 writing conventions for the attributes.

L129-135: This paragraph could be shortened. Maybe detailed information on the SMB in ISMIP6-Antarctic (L129-130) is not needed here.

We think that this is an important explanation for the ice-sheet community and we would prefer to keep this explanation.

L135: Maybe 'constrain' could be replaced by 'drive' or something similar, if applicable.

'drive' will be used in the revised manuscript.

L136-144: I would like to mention that my comments are limited to this manuscript, and I have not assessed the approach for emulating MAR and thus for obtaining the estimates of SMB used in this study. From my point of view, no detailed evaluation is needed here and it is fine to refer to the approach described in Jourdain et al. (2024, in discussion) as done. Please make sure that respective inputs and outputs of this approach become clear (see some of the following comments / questions).

We would like to keep this summary of the methodology so that readers can get the essence of this method without digging into Jourdain et al. (2024).

L137: 'surface melting' instead of 'melting'?

Yes, the manuscript will be corrected accordingly.

L140-141: I am not sure if I understand correctly how the SMB for a given member is estimated. What is meant by 'perturbed as a function of the annual temperature difference'?

Yes, the exponential dependencies are based on temperature differences between the members.

L150: Maybe replace 'a subset' by 'the subset'?

Yes, the manuscript will be corrected accordingly.

L150: 'two first subsections' could be replaced by directly stating the subsections that you would like to refer to here to improve readability.

Yes, the manuscript will be corrected accordingly.

L153-155: It might be helpful for the reader to explicitly state the ocean properties that reflect the oceanic internal climate variability in the beginning of this section (that is, salinity and temperature, as shown in Fig. 3, and as eventually described in the beginning of the following paragraph starting in L159).

At the beginning of the paragraph, we will add *« Ocean internal climate variability is investigated through salinity and temperature variability. »*

L155: What is meant by 'typical' standard deviation across model members? Does this mean that in most regions values are around 0.017 g kg-1 and 0.07°C for MPI-ESM1.1-HR? Or are these typical values for CMIP6 models?

We will remove the word 'typical' which has indeed no relevance here.

L159: 'continental shelf' instead of 'shelf'?

Yes, the manuscript will be corrected accordingly.

L161: Maybe replace 'largest variability' by 'large variability' to avoid confusion? If I understand correctly, for example, the highest variability in mid-depth salinity for UKESM1-0-LL is found around Prydz Bay.

Yes, the manuscript will be corrected accordingly.

L171: Is 'deep ocean' considered as same ocean depth as 'mid-depth'?

'deep ocean' is used here to characterise offshore waters, outside the region of the continental shelf, but Figure 3 shows the mean salinity and temperature standard deviation between 200 and 700 m over the entire area represented. We can replace 'deep ocean' by 'beyond the continental shelf'.

L173: I would like to suggest to replace 'ice-sheet mass loss' by 'present-day ice-sheet mass loss' or something similar.

Yes, the manuscript will be corrected accordingly.

L184-190: I think it may be helpful to add figures on the assessment of oceanic internal climate variability based on 60-year averages, at least in form of a Supplementary Material, given that the discussion and recommendations reflect on the time period of averaging.

We agree and we will add the figure for 60 years in the supplementary material to support our arguments.

L197: Please specify that the increased water vapour saturation in warmer air then results in enhanced precipitation.

We will add 'resulting in more precipitation' in the sentence. *« By 2100 and for the SSP2-4.5 medium scenario, run-off is supposed to remain limited (Kittel et al., 2021), so the SMB is projected to increase largely due to the increased water vapour saturation in warmer air, resulting in more precipitation. »*

L200: Is the SMB that you refer to here emulated or directly derived from the CMIP6 models? According to the caption of Figure 5 it is based on the MAR emulation. Maybe this could be specified again also in the main text.

The SMB is an emulation of MAR simulations. We will specify 'emulated SMB' in the main text.

L202: 'consistent' instead of 'consistently'?

Yes, the manuscript will be corrected accordingly.

L202 - 205: This section also refers to the absolute SMB. Since the atmosphere is suggested as an important factor for the (spread in the) projected Antarctic sea-level contribution in the following section, and the choice of the CMIP6 model at the same time also modulates the projected sea-level change, I would like to suggest to add a related figure of SMB and atmospheric temperatures (e.g. in the Supplementary Material) for interested readers.

Thanks for the suggestion. We will add a figure of absolute SMB, surface temperature and precipitation in the Supplementary Material.

L204: 'which is both due to' instead of 'which is due both'?

We will replace 'which is both due to' with 'which results from'.

L205: MPI-ESM1.2-HR also shows a relatively high standard deviation in atmospheric temperature in the Siple Coast region, compared to the other selected CMIP6 models. Is this relevant for the projected future evolution of the Antarctic Ice Sheet? In Figure 7, the ice-sheet response in basin 9 (Siple Coast) to atmospheric changes in MPI-ESM1.2-HR (showing mass loss) differs from the other CMIP6 models (showing mass gain).

The temperature variability appears to be linked to the variability in the ASL position between members (see Figure 5g), which could explain why large anomalies are found in all the basins influenced by the ASL (basins 9,10,11 in Fig. 7).

L205-208: What are the typical characteristics of the two Pacific-South American modes? Maybe add a short summary here for readers that are not familiar with Wang et al. (2022) and Marshall and Thompson (2016).

We consider that this would be going too far away from the main focus and our sentence already gives a lot of information « *As previously reported by Marshall and Thompson(2016), the internal climate variability of sea level pressure and air temperature have the typical characteristics of the two Pacific-South American modes (usually referred to as PSA1 and PSA2), which are associated with wave trains originating in the tropical Pacific and possibly modulated by feedbacks with clouds and sea-ice (Wang et al., 2022)* ».

L210-227: Maybe the link between the analysis of internal climate variability in CMIP6 models and the projected Antarctic sea-level contribution could be stressed here, in particular, for explaining some of the key results related to the uncertainties in the projected contribution of the Antarctic Ice Sheet to sea-level change. This includes, for example, the results that (1) atmospheric internal climate variability has a larger effect on the spread in the projected sea-level change with Elmer/Ice than oceanic internal climate variability, (2) the impact of the choice of the CMIP6 model on the sea-level contribution from Antarctica, and (3) the similarity of atmospheric internal climate variability for the selected CMIP6 models.

For (1), this was already specified in L223-227.

For the other two points, we will rephrase this section to stress the link between the analysis of internal climate variability in the CMIP6 models and the projected Antarctic Sea level contribution.

L213: For MPI-ESM1.2-HR there is a mean (?) mass loss related to the atmosphere in West Antarctica (Fig. 6i).

We used only 2 ensemble members for MPI-ESM1.2-HR. One of them projects a positive SMB sea-level contribution and the other one a negative contribution (Fig. 6i). Indeed, the average of the two ensemble members indicates a slightly positive contribution to sea levels (Fig. 6i). We will add « *except in West Antarctica for MPI-ESM1.2-HR (Fig. 6i).* »

L215-217: Do you meant to refer to 'Pine Island and Thwaites ice shelves' / 'Getz ice shelf' here or rather the respective basins?

We refer to 'Pine Island and Thwaites ice shelves' / 'Getz ice shelf'. For clarity, we add « *The West Antarctic positive SLC is mostly explained by the grounding line migration and the dynamical response of Pine Island and Thwaites ice shelves (~3 cm in Fig. 5c, basin 11) as well as Getz ice shelf (~1 cm, basin 10).* »

L217-218: Is this drift of the unforced Elmer/Ice experiment removed or is the absolute Antarctic sea-level contribution given in the respective figures? I think I got confused by the statement in L109-110 (please also see my related previous comment). And can the influence of the drift on the trends in East Antarctica be quantified?

The drift of the control Elmer/ice experiment (i.e., Elmer/ice configuration driven by current atmospheric and oceanic forcing) is not removed, which is why we had to correct the friction coefficient to reduce the drift.

All the figures display the absolute Antarctic sea-level contribution.

Based on Table 1, the mass change rate simulated in East Antarctica is equal to +107 Gt/yr whereas the observed mass change is +5±46 Gt/yr, meaning an overestimation between +56 Gt/yr and +148 Gt/yr.

L217: I would like to suggest to replace 'contaminated' by 'influenced' (or something similar).

Yes, the manuscript will be corrected accordingly.

L218: What can be learned on the sensitivity of East Antarctica and the Antarctic Peninsula to internal climate variability based on the simulations presented here? Maybe you can make use of Figure 7 and add some details in this section.

We can complete the analysis for the three regions (East Antarctica and Antarctic Peninsula in addition to West Antarctica) on the same model as the analysis carried out for West Antarctica.

L220-222: I would like to suggest to give the full name of the CMIP6 models throughout the whole manuscript (consistent with e.g. Sect. 3.1).

For sure, the manuscript will be corrected accordingly.

L223: Basin 5 (including Totten glacier) shows a relatively large spread in the dynamical sea-level contribution (Fig. 7b). Can this be related to the assessment of oceanic internal climate variability in Sect. 3.2?

Yes, the ocean variability in Fig. 3e-f is particularly strong near Totten, which explains why ocean has a larger effect on internal variability there. This will be mentioned in the revised manuscript.

L226-227: Please add more information on this finding. How is the number determined? Can it be seen in a figure (likely Figure 6)?

For each of the three CMIP6 models, we compare the amplitude of sea-level contribution (SLC) variability induced by ice flow dynamics, which is largely modulated by ocean-induced basal melting (Figure 6b), with the amplitude of the SLC variability due to the surface mass balance (Figure 6c), which is largely driven by the atmosphere.

When we talk about amplitude, we mean the difference between the SLC value in 2100 of the member giving the smallest contribution and the member giving the largest contribution.

We will add a reference to Figure 6 in the main text for a clearer explanation « *On average, by the end of the century, the amplitude of SLC variability due to the atmosphere (Fig.6c) is 3.4 times higher than the variability due to the ocean (Fig.6b).* »

Figure 6: I assume that the number in brackets in the legend refers to the number of ensemble members for each CMIP6 model. Why do the numbers differ between panel a/b and panel c? Please check.

You are completely right, the number in brackets in the legend refers to the number of ensemble members for each CMIP6 model. We will add it to the legend.

For panel b): *« Because of the numerical cost of our simulations, we select a limited number of members. The selection is made over the current period (1995-2014 means) to cover the widest range of values for the ocean temperature on the continental shelf in the Amundsen Sea. We focus on this region as (i) observed present-day ocean-induced mass loss and (ii) the amplitude of the across member standard deviation of the 1995-2014 mean potential temperature are among the highest around Antarctica (see Fig. 3d-f, j-l).*

We selected 5 members for each CMIP6 model, i.e., the two members with the coldest temperatures and the two members with the warmest temperatures in the Amundsen Sea continental shelf (Fig. 3j-l), for which the SMB is available, as well as member 1, used by *default in most of the studies. In total, we run 11 simulations, five with the IPSL-CM6A-LR model (r1i1p1f1, r3i1p1f1, r6i1p1f1, r11i1p1f1, r25i1p1f1, see https://goo.gl/v1drZl for CMIP6 convention name of ensemble members), four with the UKESM1-0-LL model as member 1 is already included in the temperature criteria (r1i1p1f2, r2i1p1f2, r4i1p1f2, r8i1p1f2) and two with the MPI-ESM1.2-HR model (r1i1p1f2, r2i1p1f2). As the oceanic variability is very low in MPI-ESM1.2-HR (Fig. 3f), we retained only member 1 and another member to verify the low impact of oceanic variability on the dynamic contribution. »*

For panel c): we take into account all the members available.

For panel a): the total contribution is a combination of dynamical contribution and SMB contribution, so the number of ensemble member for the total contribution depends on the limited ensemble members of the dynamical contribution and thus have the same number of ensemble members.

Figure 6: Does the solid line indicate the mean? Maybe I have missed this.

We forgot to include this information in the legend of Figure 6. The manuscript will be corrected accordingly.

L232-233: Maybe specify which paleoclimate proxies are used in Parsons et al. (2020) (similar to stating that Casado et al. 2023 base their analysis on ice core reconstructions in the following paragraph), for readers that are not familiar with this study?

Yes, the manuscript will be corrected accordingly.

L232: ‘global mean surface air temperature’ or its variability?

Thank you for the careful reading, the word ‘standard deviation’ is missing.

We will rephrase as « Parsons et al. (2020) compared the distribution of standard deviation of global mean surface air temperature of CMIP piControl simulations to paleoclimate proxies representative of the 1450-1849 period. »

L233: Maybe you could add the observational plausible range for the temperature variability for comparison with the values for the CMIP6 models?

Yes, the manuscript will be corrected accordingly.

L243: If possible, maybe a conclusion on the representation of atmospheric variability in CMIP models could be added, bringing together the results of this study (Sect. 3.1) with the previous literature?

We will generalise the assessment of internal oceanic and atmospheric variability carried out in §3.1 to 15 CMIP6 models:

- for ocean: calculation of across member standard deviation of the 1995-2014 mean potential temperature over the whole continental shelf for the 200-700m depth,

- for atmosphere: calculation of across member standard deviation of the 1995-2014 mean SMB over the whole Antarctica.

This will allow us to see how the three selected models stand in comparison to a larger set of CMIP6 models. The new figure will be discussed in §4.1 and added in supplementary material.

L245: Maybe add a reference for these observations?

We will add a reference to CTD profiles measured in the Amundsen Sea and described in Dutrieux et al., 2014 and Jenkins et al., 2018.

L258-259: As the choice of the CMIP6 model is suggested to have a similar impact on the Antarctic sea-level contribution as the internal climate variability, it may be helpful to add a short paragraph on this finding also in Sect. 3.3 (in addition to this statement in the discussion).

Yes, the manuscript will be corrected accordingly.

L265-266: This sentence can maybe be reformulated. As already indicated, given the limited impact of the emission pathway on the Antarctic sea-level contribution to 2100, SSP2.4.5 may not be the main explanation for the Elmer/Ice projections presented here being at the lower end of previous projections.

We will remove the argument of GHG scenario in the sentence and focus mainly on the present-day drift and we will mention the sources of uncertainty described in Seroussi et al., (2023), i.e., uncertainties in the physics of the ice-sheet model, the choice of climate model and uncertainties associated with ice-climate interaction (melt parameterisation and calibration).

L269: I would like to suggest 'ocean-induced melting' (or something similar).

Yes, the manuscript will be corrected accordingly.

L270: I am not sure if I understand the meaning of 'high variability of 20-year means' correctly. Maybe it is possible to rephrase?

We will replace with « *wide confidence interval on a 20-year mean* ».

L278-283: This paragraph seems to contain much information that is also given in the beginning of the following Sect. 4.3. I would like to suggest to move L278-283 to Sect. 4.3 and merge with the first part of this section.

Yes, the manuscript will be corrected accordingly.

L284-354: This is an interesting analysis and discussion. If I understand correctly, it supports to include multiple CMIP ensemble members in Antarctic sea-level projections as done in the work presented here. At the same time, it seems slightly detached from the previous parts of the manuscript. I would like to suggest two options that may help to add focus to this section:

A) This section may be shortened, summarizing the main analysis and the conclusion. The major part of the analysis may be moved to the Supplementary Material.

B) Parts of this section (e.g., the metrics and justification for these metrics) may be included in the Methods, and the outcomes could be highlighted and discussed with the main results. If applicable, the Antarctic sea-level contribution for the 'best' ensemble members could be added separately to e.g. Figure 6.

Thanks for the suggestion. We choose option A).

For a better readability and following suggestions by the other reviewers, the discussion section on the identification of the best member will be revised. Figure 8 and the list of associated metrics will be moved to the appendix and the same analysis will be perform for UKESM-0-LL to get more robust conclusions.

L290: I am not sure if I understand this phrase. Maybe replace 'assess' by e.g. 'demonstrate'?

In the revised paragraph, we will specify that the metrics considered were chosen to:

– ensure a good representation of the mean atmospheric and oceanic states. We selected variables directly used to drive the ice sheet model, i.e., the SMB for the atmosphere and temperature for the ocean. We focused on the ocean temperature in the Amundsen sector as the region experiences the current main mass loss and CTD profile data are available for a relatively long period from 1994 to 2018 in this area.

– ensure a good representation of the amplitude of oceanic variability using the same observational data described in the previous paragraph. We did not evaluate the variability of SMB since it has been relatively stable in recent years and there is no observational data.

– ensure the best representation of important modes of variability known to affect the ocean and atmosphere in/around Antarctica. We focus our analyses on the SAM and TPI index.

– ensure a phasing of internal variability with observations, which could be important for future detection/attribution studies and for projected Antarctic sea-level contribution. We chose two variables, sea-ice concentration and the presence of warm periods on the continental shelf of the Amundsen Sea to provide insights on the phasing of internal variability.

It should be noted that this part of our study remains exploratory and the choice of variables and metrics is indeed very subjective, which is part of the caveats that we discuss.

L368-384: This paragraph contains many valid and helpful recommendations for future assessments of the Antarctic contribution to sea-level change. However, some of these recommendations do not seem to be directly justified by the presented results, or a better link to and additional information in Sect. 3 may be needed. For example, a fully-coupled assessment would be ideal to include feedbacks of the ice sheet with the ocean and the atmosphere, but some additional discussion how this would e.g. improve the representation of or remove biases in the internal climate variability in the selected CMIP6 models presented in Sect. 3.1 and Sect. 3.2 may be needed to directly relate to this study.

Following the comments of all the reviewers, we have only kept the two recommendations that are directly motivated by our findings, and we will make the links with our findings more explicit.

L379-380 / L382: Do the ‘various members’ / ‘multiple members’ refer to the CMIP6 model ensemble members or to ice-sheet initial states?

This sentence will be removed from the revised manuscript.