We thank both reviewers for their thorough and thoughtful reviews, which we thoroughly believe will make the manuscript a better one. Below, Please find our responses to the referee comments. Our responses appear in blue text – though as The Cryosphere expects a response prior to a revision, text that mentions proposed changes to the manuscript appears in red to indicate the changes are not yet made.

Gladstone review

General comments.

This study takes the current state of the art of coupled ice sheet - ocean modelling and applies such a model to multi century projections of the Amundsen Sea Embayment in West Antarctica. While the coupling is justified, the impacts of coupling specifically are not assessed in this study.

We agree with this last assessment. Our aim was not to show that coupling gives a better representation of ice-shelf melt, and we do not argue that such a concept is shown by our results, though as the referee states below it is considered to be a favorable option to represent ocean-climate change effects.

Instead, the impact of ice model initialisation is assessed in the context of a coupled model. Two approaches to ice model initialisation are compared, and their impacts on multi century projections are compared. The differences are significant, highlighting an important area of ice sheet model based projection that needs attention in the international community.

Thank you, this was one of the key points we feel is important to get across.

Sea level projections for PIG, Thwaites and the whole ASE are given for a base scenario and a very high climate warming scenario. These can be consiered as plausible point projections which contribute usefully to the sea level projection literature.

Thank you for this, we agree with this careful assessment.

The impact of ice model initialisation is compared to the impact of climate forcing, and an index created to represent their relative importances. It is important to note that this index is very much specific to the design choices of the current experiment, and should not be generalised. The two approaches to model initialisation presented here do not necessarily span the range of possible ice model drifts imposed by initialisation methods in ice models, whereas the two climate forcing scenarios should be considered as end members of viable futures. For this reason, a mroe generic version of the metric presented here would probably give even higher weighting to the uncertainty arising from ice sheet model initalisation, giving a

stronger result than that presented here. But however one views the metric, the results presented here highlight that ice sheet model initialisation is an important and poorly quantified problem in projections of ice sheet contributions to sea level.

We believe the referee refers to Eq 5 and Fig 6. We fully agree that our metric – which is constructed solely to add a concrete quantitative aspect to the patterns observed and mentioned in the text in Fig. 5 – is not representative of ice-sheet models as a whole in terms of emergence of climate change impacts, which requires a large model ensemble, and in fact our estimate potentially underweights the effects of initialisation. We will make this point at the end of the paragraph where Eq 5 appears.

RCP8.5 is a rather extreme forcing scenario. I'm not suggesting that you carry out additional runs at this point, but it would have been interesting to see a comparison between a control and a more intermediate scenario. Persumably this would increase the period over which the initialisation has high relative importance.

Our choice of scenario was primarily based on the study of Naughten et al 2023 which showed relatively small differences in heat increase between an RCP8.5 and a Paris 2C mitigation scenario (and no significant difference between Paris 2C and an intermediate scenario). *Note: the statement of no significant difference between Paris2C and RCP85 was made in error, and will be addressed in the revision.* However, we now will state in the conclusion that comparison across scenarios should be considered in future studies.

The fact that the simulations crashed with numeric instabilities is a bit concerning. I haven't seen evidence that the authors looked into whether these fatal instabilities were manifest in a non-catastrophic way earlier on in their simulations, potentially influencing the interpretation of results. I don't see a description of what the authors tried to do to mitigate these instabilities, perhaps with shortened timesteps or geometric smoothing for example. I don't see an anlysis of where thy occurred - were they all triggered from within the Thwaites catchment? That might increase confidence in the results for other catchments.

We did not explain the details regarding the instability because we thought them overly technical concepts for the intended readers of this study, but are happy to provide more detail here.

One point to make clear is that the instability is not the ice-sheet model blowing up, and hence we have no concerns about the evolution of VAF in the model, which is a key focus of this study. Rather, the ocean model is crashing because the thin ocean layer (~2m) maintained under grounded ice is becoming negative. This layer exists because of the nature of the coupling which allows continuous changes in the ice geometry at the ocean time step, and is essential for a smooth, physical grounding-line retreat. In the case of the model failures we see, the blowup happens because the ocean free-surface solve is evacuating water from the subglacial layer as the grounding line retreats at a very fast rate.

The model provides a report of the location where column thickness first becomes negative, and in all simulations this happens on Thwaites glacier where fast GL retreat rates are seen.

This issue, referred to later in the response as the thin column instability, is a known one, discussed e.g in Goldberg et al (2018). The issue could be mitigated with a thicker layer, or with higher drag, but we do not do that here, as the simulations are sufficiently long to support our conclusions – and we do not think it would be of value to rerun them (taking several months each) in order to extend them further.

Importantly, we are absolutely confident that the thinning subglacial layer is not an issue prior to blowup. The subglacial layer does not participate in melting and is just a device to allow the free surface to unground the ice. Sensitivity tests carried out in Goldberg et al (2018) show that layer thickness is immaterial so long as it is below the threshold at which we allow melting.

We acknowledge our description of the instability was vague, however, and will reword this passage to give a better explanation of what is going wrong, without being overly technical.

I didn't find a description of calving in the ice model. I presume that calving is not included in this model [edit: this is first mentioned in 4.4, but I would like to see it mentioned in the methodology], which raises the questions: What happens when a lot of thinning occurs? Is the shelf allowed to thin to zero or is there a minimum thickness imposed? Does this impact on buttressing?

We do not implement calving, as mentioned by the referee. There is a minimum thickness that is implicitly enforced – in that ice-shelf melt does not occur where the ice shelf is less than 5m thick. The reason is that allowing the ice to thin to zero could create tabular icebergs, which cannot simply be “removed” – since in our synchronous model this would result in large tsunamis – and leaving them would require functionality for evolving and advecting large tabular bergs which is beyond the scope of our study. We assume the buttressing by ice this thin to be negligible, but this is not investigated.

We will state this “soft” lower bound on thickness in our methodology and that calving is not currently considered.

Separately, on the ocean side, I presume you force the ocean from CESM2 outputs at the surface (I wasn't sure whether your description of ocean forcing applied only to the vertical boundaries or also the upper surface?), in which case you are affecting future ocean behaviour by keeping the ice fronts fixed and not allowing upper surface forcing to evolve?

We apologise for an oversight in not mentioning this – we do not implement surface forcing. In doing so, we are following other ice-ocean interaction studies which represent only a small portion of the continental shelf (e.g. Dutrieux et al 2014; Goldberg et al 2019; Holland et al, 2023; Bett et al 2024), making the assumption that the majority of the action of the atmosphere on ocean hydrography is encapsulated in the ocean boundary conditions – but have not made this explicit. We will do so, and also mention under Limitations that our estimates of VAF change neglect differing surface mass balances over grounded ice.

Compass directions are discussed in the text, but not shown in the figures. Roughly speaking, north is left, but this is not precise and a reader not familiar with the domain might assume north is upward. I suggest you add a compass graphic in Figure 1 to orient the reader.

We will do so.

Line by line comments.

Line 45. While coupled ice - ocean modelling is likely the best way forward, more advanced parameterisations for sub shelf melt, such as machine learning based parameterisations (e.g. Rosier's work) or 2D ocean models (e.g. LADDIE) may offer viable compromises, especialy given that high fidelity ocean modelling can be prohibitively expensive for large scale coupled projections, leading to resolution compromises. I agree that fully coupled modelling is the current gold standard, but not necessarily the "only" option going forward.

We will revise this statement.

Line 59. Here you talk about SSP but in the abstract you talk about RCP. RCP is mentioned later on too. I suggest you search and ensure consistency.

We apologise as we have in a number of places used "RCP" incorrectly. The boundary conditions from Naughten et al 2023 (for climatology, and for current velocities up to 2100) are downscaling of RCP85, but the ISMIP6-2300 fields we used are SSP-585. We will ensure consistency.

Figure 1. Surely the 85 simulations have more retreat than the base simulations? In which case, why are the base ocean domains expanded at 2200 but not the 85 domains?

This is explained later on, in the results: after retreating from Upper Thwaites Ridge, the retreat in the SSP585 experiments accelerates so strongly that the grounding line ocean column instability occurs; acceleration is far less in the Baseline experiments, postponing instability and allowing more extensive GL retreat. It would be confusing to explain this at this preliminary point so we do not attempt to.

Section 2.1.3. This seems like an important section. I don’t see any discussion of errors in the observed product. I havent read the Gourmelen paper. But it says it is based on remotely sensed data, and seems to cover the whole PIG shelf, so I guess it is mainly satellite data. If this is based on surface elevation change and flux divergence calculations, it's accuracy may be dependent on observational accuracy, displacement from floatation, and density variations. The region near the grounding zone is where the flotation assumption is least accurate. Are you sure that the observed melt rates are correct here? Is there a chance you might be putting effort in tuning your melt to an inaccurate product? It looks like you end up with something that underestimates melt near the GL but overestimates it elsewhere. It is hard to estimate the impact of this error on the uncertainty in your final results.

This is correct, the Gourmelen data is satellite-based, using Cryosat for surface elevation change; advection for the material thickness derivative; and satellite velocities for divergence. The paper does not give uncertainties for the raster product, which we use here. (Though we note Reed et al (2026) uses a uniform error of 5 m/a in their optimisation.) In-situ estimates of melt are difficult or impossible to obtain in this region, so it is difficult to “ground truth” such estimates, though a much higher-resolution study which integrated airborne data found similar magnitudes (Dutrieux et al, 2013), albeit at different epoch, and more recent estimates based on distinct satellite products found similar magnitudes as well (Shean et al 2019, which we will cite). Furthermore, it is known that coupled models which calculate much smaller melt rates at the PIG grounding line show spurious advance (Bett et al, 2024; Reed et al 2026), despite other measures intended to mitigate this, implying that high melt rates are “demanded” by large-scale continuity considerations.

A full optimisation of the melt transition from velocity to non-velocity dependency is outwith the scope of our study (though as mentioned, a recent study has since carried one out), and so our chosen parameterisation based on a small ensemble does have some errors. We view this as acceptable, as stated: “*Since our priority is accurate melt rates in regions most important to the grounded ice, such as near the grounding line, this is acceptable for the present study*.” Furthermore, our choice is conservative to prevent overestimation of retreat (which was not stated before, but which we will state).

Equation 3. I am missing an explanation of alpha squared? Maybe alpha squared is the Coulomb limit and beta squared is the Weertman coefficient? What value is used for alpha squared?

Alpha squared is 0.5, which is stated in Cornford et al (2020), we will now state this here. It is the parameter relating effective stress to yield stress in the coulomb limit.

Line 198 "timd" typo

Will be fixed

Figure 4. The elevation rate comparison (a to c) is very clear. But the velocities are dominated by the base pattern rather than the difference to observations. How about showing difference to observed in e and f? Do you think that would convey the differences more clearly? Can you add the reference for 4a to the caption? Seems unfair to reference the velocity obs but not the elevation rate obs at this point…

We prefer to keep the panels as they are, as the comparison in the top and bottom row is now consistent. Panels b and c show the modelled thinning rate, i.e. the difference between surface elevation in 2018 and 2013 (for grounded ice). This is directly comparable with panel a. Similarly, panels e and f are directly comparable with panel d. Showing the difference between observed and modelled velocity in panels e and f would limit comparability.

The reference will be added.

Section 2.3. This seems like a good choice of coupling strategy, but it is slightly less tight than I was expecting given the description in the abstract that the ice and ocean were "synchronously coupled". Maybe it is ok to leave it like this, and certainly the strategy is justified. Perhaps it would help the community to define terminology for different levels of synchronicity. This feels to me to be "almost synchronous"...

We are unsure where the definition of “synchronous” falls down? In fact we do not know of a “seminal definition” of the term in the context of ice sheet-ocean coupling in the literature. However, we are clear on our definition of the term: “**the coupled evolution is synchronous in that the ocean evolves continuously with ice-sheet geometry**,” which we feel to be the most important aspect. In this sense, our scheme is synchronous.

Line 240. OK, the ocean domain was expanded when needed. But the ice mesh stayed the same as I understand. Did the GL ever retreat into the coarser resolution region? It looks like it got close for Thwaites... could this impact on retreat rates after this point?

Yes, at this point the max. resolution at the grounding line was approx. 1750m, which is about ⅙ coarser than the highest resolution; we expect impacts to be minor. We will now mention this.

Line 250. typo "oecean"

We will fix.

Line 253. Can you be slightly clearer about this? I think you present your outputs up until the point of simulation failure. Can you state this specifically in this paragraph? Before I looked ahead at the results figures I didnt know how to interpret this paragraph.

We will give a more intuitive explanation of what is happening, and state that we present up until the point of failure.

Figure 5 caption typo. Forgot to include (g)-(i).

We will fix this.

Figure 5. I guess the melt rate smoothing was due to the actual rates being very noisy. Is that noise purely internal variability in the ocean, or is some of it coupling artefact, e.g. jumping as GL retreats, perhaps the instability you mentioned above? The reader naturally wonders whether non-physical artefacts occurred long before catastrophic failure of the simulation.

This is a good question, but one very difficult to evaluate with the results from this paper alone. Moreover, it is difficult to separate the effect of grounding line retreat on melt, which opens up new pathways for warm ocean water, from any effects of the thin column under grounded ice, which is the source of the instability. The figure below shows the melt rates without any smoothing at all, and from this we can say that not ALL of the interannual variability is associated with GL retreat, as the sectors – primarily thwaites (centre) – exhibit strong retreat with and without SSP585 forcing, but the variability in the Baseline runs is much smaller, indicating that it is the forcing rather than the mode of coupling.

Without similar experiments run with an "asynchronous" approach, it is difficult to say more. But as we describe above, the sub-ice sheet column properties was found in Goldberg et al (2018) to have negligible effect on total melt rates.

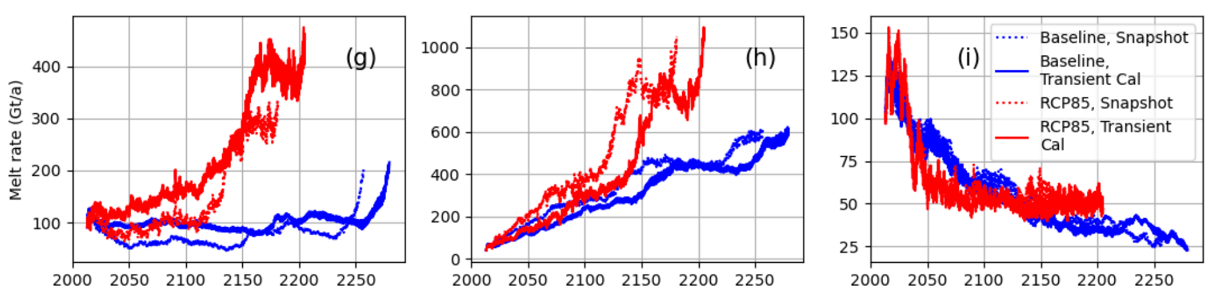


Figure 6b is very noisy. It isn't clear whether the 6 month tiem smoothing was applied to the melt rates here or not. If it wasn't maybe apply it? If it was, maybe try a longer time to smooth it more? And maybe also increase the upper limit of the y axis?

No smoothing was applied to the melt rate fields as we thought this misleading, Our only aim in Fig 6b (made clear in the text) is to show that melt rates do not follow the same emergence pattern as VAF loss rates. Changing the y-limits on the plot would limit this demonstration, so even if noisy, we prefer to keep this panel as it is, but will attempt to overlie a smoothed version, and emphasise its aims in the caption.

Figure 6. I don't know why you have a solid line at y=0.75. I don't see the significance of this number. I suggest to remove it.

We will do so, the other referee suggested this as well

Figure 8 the y axis label is cut in half.

This will be fixed.

Figure 8. Has the PIG/Thwaites catchment boundary migrated? Or was it always slightly out compared to the model?

If you are asking whether the thwaites grounding line has retreated into the nominal PIG catchment, then it has – this is mentioned explicitly at the end of the results chapter. In the initial state, the high thinning and speeds of PIG and THW are separated by the accepted catchment lines.

I infer Figure 9 is also at the point of failure (though the GL speed represents the history). This doesn't seem to be explicitly stated. Add to the Fig 9 caption for clarity?

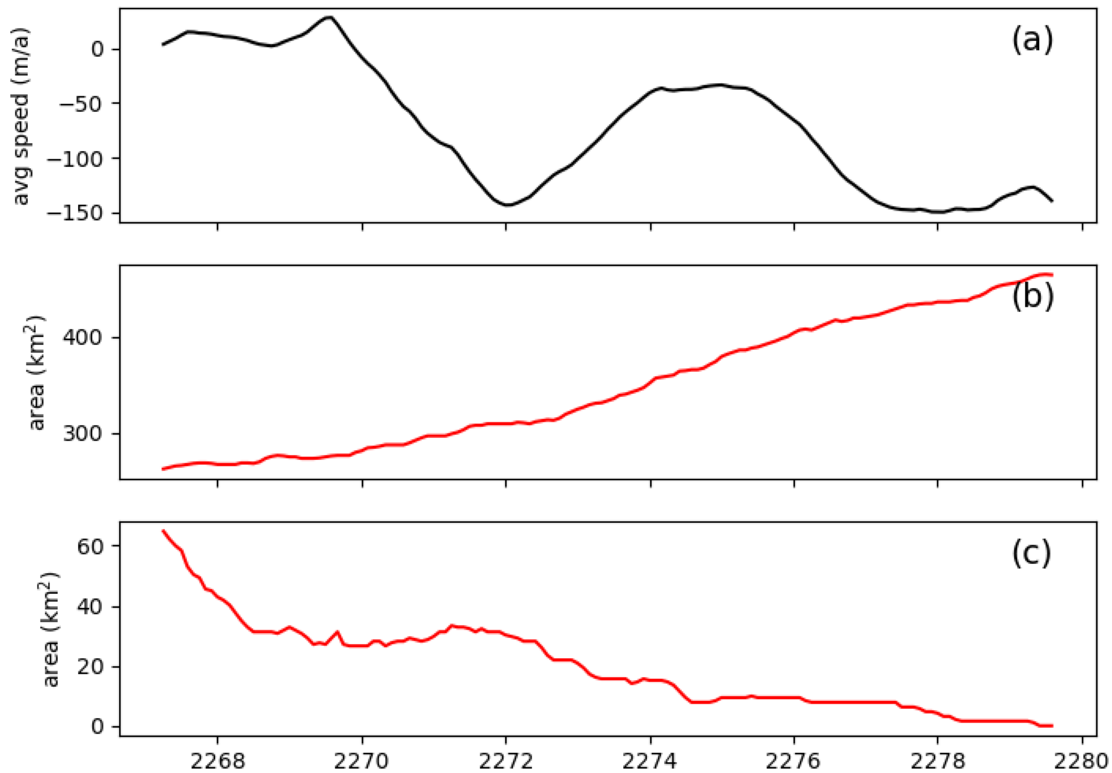
This will be added

Line 324. Well, it is hardly going to occur after model failure…

Will remove "at least"

Figure 11b and c. Leaves me wondering how much this is changing relative to the total grounded area, which is presumably pretty small since we're just looking at a couple of pinning points. From this perspective I think it would be more informative to show the absolute (rather than relative to 2267) grounded area. You don't have to include y=zero on the axis, so the plot would look the same, but the axis would be slightly more informative. Especially c. You can't tell in Fig 10 how much of the pinning point has gone. Has it halved? More Less?

We will show this (see below). The eastern pinning point(s) go completely; the western pinning points roughly double in area which is roughly what can be seen in FIg 11.



Section 4.1. Dynamic regrounding (downstream advection of thicker ice in response to accelerated flow; see also https://tc.copernicus.org/articles/12/2425/2018/) may be more likely in the base than RCP85 experiments due to the differences in melt rates. Might be relevant here.

Thank you we will cite. We may have misunderstood but what we are describing is in a sense dynamic regrounding?

Figure 12. Since the buttressing number is calculated in the ice flow direction, a gentle smattering of flowlines might aid the reader in interpreting this plot.

We will test this out, but it may make the patterns more difficult to see; if so, we will assume the general direction of flow (from land to sea) is known.

Figure 12. Idle thought: I wonder if you can combine your melt rates with the buttressing number somehow to come up with a "melt impact" number…

Thank you, this would be interesting. But it might be misleading given the (acknowledged in the manuscript) heuristic nature of the buttressing score. A more quantitative measure of this impact (e.g. Reese et al, 2018; Goldberg et al, 2019) would be beyond the scope of our paper.

Lines 385-386. While I like the overall context and use of the buttressing number as a proxy for sensitivity to ocean induced melting, the wording here underplays the potential of the grounding zone to control ice dynamics. Technically "upstream of the GL" could include the elements/grid cells immdeiately connected to the GL, but that's probably not the implication most readers will take from this. I suspect that the parameterisation of drag in the grounding zone, along with the way in which tuning determines the spatial pattern of these parameters, can have a very strong impact on the model drift once transient simulations start. See also this paper, in which different ways of implementing effective pressure in regularised coulomb type sliding laws has a massive impact on projections of Thwaites catchment (and we found the differences in basal drag resulting from these choices were highest in the grounding zones): https://www.nature.com/articles/s41467-025-58375-4

We see your point – and will make sure that the importance of the grounding zone (and the parameterisation of basal drag) is conveyed.

Line 420. If I were you I would probably be tempted to go one step further here, and suggest that any theshold for instability must be defined in terms of both climate forcing and GL position/geometry (the "traditional" thrshold perspective for MISI).

Unfortunately the definition of stability is a very thorny issue, one which we prefer to leave alone here, and simply state what we see from our results.

Line 423. This confuses me. Both "algorithmic uncertainty" and "model uncertainty" sound like "structural uncertainty" to me and neither sounds like "parametric uncertainty". Structural and paramteric uncertainties are the two modes of uncertainty I am familiar with in the context of model based experiments designed to quantify uncertainty.

Here, we attempted to distinguish between uncertainty associated with not knowing the correct equations (commonly called “structural” but “model” is sometimes used instead), and that arising from multiple independent codes that differ in numerical aspects such as discretization choice, stopping criteria of iterative solves, and even machine architecture, while solving nominally the same equations with the same parameters. The two can sometimes be grouped together such as in the “ice model uncertainty” term of Seroussi et al (2023), but we wish to flag them as distinct. There is not a precedent in ice-sheet modelling literature for making this distinction, and so we will better define the term “algorithmic uncertainty”.

Parametric uncertainty is left off our list of things that we did not consider because (with a sample of two) we did, using two different inversions which give different parameter fields.

Line 429. Typo biase

We will fix.

Line 449. Missing "ice"? i.e. "advection of less damaged ice"?

Yes, that is correct, we will fix.

Line 467. Typo? full stop in the brackets should be comma or semi colon?

Yes, full stop is a typo to be removed.

Lines 468-469. This is a very Thwaites-centric comment, and to my mind not quite strong enough based on what you've shown. Surely the more complete statement is that the benefits of climate mitigation are indicated by this study to be very strong on a multi century timescale, but that these benefits become apparent at different rates for different catchments. The implication being that if you want to assess the benfits over many generations you need to be careful in your choice of which catchments to look at to see the benefits.

We prefer to leave this statement as it is, because from a policy perspective it is more likely to be ice-sheet wide assessments that are looked at, and Thwaites is a large contributor to the region- and, possibly in future decades, continent-wide mass balance. Of course, we can only speculate about other catchments as to our knowledge similar assessments to ours have not been done, but we do not want to undersell the importance of persistence of initial state to perceptions of effectiveness of mitigation.

As for your final line, yes, sure, but the more specific point here is that ice model initialisation practices tend to tune a model drift, and that this drift can impact sea level projections. This study highlights how important the ice model intialisation is, whether you have a coupled system or not, and while coupled ice ocean models surely do need to be improved, that general comment isn't really about what you've specifically shown here.

We will adapt our statement so as not to downplay the initialisation aspect.

Anonymous Reviewer 1

Our responses appear in blue text – though as The Cryosphere expects a response prior to a revision, text that mentions proposed changes to the manuscript appears in red to indicate the changes are not yet made.

Review of Century-scale impacts of ice-sheet model initalization on Amundsen Sea Embayment, West Antarctica, by Goldberg, Holland and Naughten.

General Comments

Realistic regional configurations coupled ocean – ice sheet models have emerged very recently, and this manuscript presents the first (as far as I know) multi-centennial projection of the Amundsen Sea Embayment with this kind of model and at a relatively high resolution. The authors demonstrate that, even after grounding line retreats past a bedrock obstacle into the deepest and widest parts of its catchment, the ice sheet evolution is not independent of forcing. This is a very interesting result despite the acknowledged caveat that the evolution of ice damage and calving is not represented. There are many very minor aspects to improve, but the overall manuscript is clear and scientfically robust. I therefore recommend this manuscript for publication in The Cryosphere.

We thank the reviewer for their encouraging words.

Specific comments

Title: It is up to the authors, but “multi-centennial” would seem more in line with the projections from 2000 to ~2300 than “century-scale”.

Thank you, “Decadal scale” generally refers to more than 1 decade so “Century Scale” seemed apt but we will consider this change.

The abstract really focusses on Thwaites while the manuscript is more balanced between Pine Island, Thwaites and Smith. Please also summarize the results regarding Pine Island and Smith.

We agree, and will change the abstract to be more generalised.

L. 22: you could add “and Pine Island ice shelf (Shean et al., 2019)” to make clear that this is not a specificity of Thwaites.

Thank you for the suggestion, we will do so.

L. 29-32: I am not sure it is correct that "Naughten et al. (2023a) […] saw no significant difference in the level of warming between the RCP 8.5 scenario and one in which a 2°C warming target is met", at least not after 2045, as far as I can tell from their Fig. 3.

Thank you for pointing this out, this statement was made in error by the lead author. The actual statement, "*no significant difference between mid-range emissions scenarios and the most ambitious targets of the Paris Agreement*", was misread, and refers to RCP85, not RCP45; this will be addressed

L. 40: a pure depth-dependent parameterization is not suitable for representing the melt dependency on the ice slope.

We agree this is not sufficient to represent melting, and this is why we use the qualifier "somewhat". Note also that we only refer here to representing thermal stratification of the column, and not the melt physics.

When you introduce equation (1) specify that it is used to process CESM2-WACCM. And equation (1) could be wricen with "(.)" instead of T, as in eq. (5), to avoid having another T of different meaning in section 2.3.

Thank you we will implement these changes to improve clarity.

Please note also that in many places (though not in the introduction) we mistakenly refer to the CESM2 outputs as an RCP85 scenario – though it was in fact SSP5-85 (there was no CESM2 RCP85 output included in the ISMIP6-2300 protocol). Following referee #1 we will address this throughout.

The coupled model is limited to the Amundsen Sea. Is it fair to assume that the evolution of the ice sheet in the Ross, Bellingshausen and Weddell sectors until 2300 won't impact the evolution of the ice sheet in the Amundsen Sea?

The ISMIP6-2300 ensemble (Seroussi et al, 2024) shows ungrounding of the divide between the Siple Coast and the ASE, but the percentage of the ensemble in which this happens is close to zero. We will now raise this point under Limitations but express that it is unlikely.

Throughout the manuscript, please refer to the model as CESM2-WACCM rather than CESM2. The two models exist and they differ at least in the vertical extent of the atmosphere and in the representation of atmospheric chemistry.

We will do so.

L. 118-119: please indicate here that the transition is a function of the water column thickness.

We will do so.

L. 124: please indicate the units for the exchange velocity γ!".

We will do so.

L. 135: “where is” -> where there is (?)

Thank you, we will fix.

L. 146-147: I don’t understand why sea level rises when grounded ice is lost. What does “lost” mean here, and why does it increase the domain mean sea level?

By “lost” we mean that the Volume Above Floatation within the domain has decreased.

The confusion may arise because loss of mass from floating ice should not cause sea level rise. And indeed, if grounding line movement is not considered, and loss of ice-shelf mass in the coupled model is due to melting alone, then the lowered overburden of the ice shelf should be balanced by the input of fresh water due to melt.

But we also must consider dynamic changes. We describe a simplified scenario: imagine firstly that the grounding line is not yet migrating but that grounded ice volume is being drawn down dynamically – this means that ice mass is flowing into the ice shelf, and so either the ice shelf grows in volume, or loses the accumulated mass through a combination of melting and calving. If we discount calving, then either the ice shelf becomes thicker (and not due to freezing), displacing water. If an increase in melt completely balances the influx of mass, then effectively there is an influx of water (from melt), leading to a volume increase not offset by a thinning shelf.

We have not mentioned calving, and in fact as Referee 1 requested we will mention earlier that this is not represented, one reason being that we would potentially need to track tabular icebergs through the domain to prevent tsunami generation. As such, iceberg flux is not captured in our water mass budget. Still, in our experience sea level in the domain rises unless the mentioned adjustment is applied.

L. 152: what is “DIVA”?

The Depth Integrated Viscosity Approximation, which represents vertical shearing along with longitudinal stresses at a similar computational cost to the Shallow-Shelf Equation.

L. 155: “all of Smith” should be clarified here or somewhere else. As far as I can see from the plots, this seems to refer to Smith and Kohler glaciers and the corresponding ice shelves: Crosson and Dotson.

We state that in the ‘flow catchment’ subsection that our ``Smith'' catchment is the combined Smith, Pope, and Kohler catchments (together with Crosson and Dotson ice shelves). We will now add a “see Sec 2.4” here so that we do not need to restate it

Section 2.2.1: please indicate which period this snapshot is representative of?
We will give the period of MEaSUREs. The product is a mosaic spanning 1996-2016, which means it uses imagery from multiple satellite eras, and the product does not provide a way to trace pixels to a certain year or satellite, or provide representative time periods for various catchments. The user guide shows no coverage prior to ALOS-PALSAR in 2006, so we will give this as a lower bound for the period.

Fig. 4: in the caption, indicate which period is used to produce these plots.

We will do this.

L. 199: “van den Akker et al. (2026)” -> (van den Akker et al., 2026)

To be fixed

L. 250: “oecean”

To be fixed

L. 268-269: I am struggling to see how the text corresponds to Fig. 5f.

5f shows the rate of change of Vaf for the smith catchment for the 4 experiments, and this sentence describes how the forcing rather than initialisation dictates Vaf loss rate after a certain time.

L. 271: please add a statement for Pine Island as well.

We will do so.

Fig. 6 does not have Y-axis labels or units. The threshold at y=0.75 should be either removed or described in the caption (if kept, then indicating y=0.25 would also make sense w.r.t. the text). And why not extending the Y-axis limits for panel (b), it does not macer is the range is not the same as in panel (a), and it is important to see how high this goes (e.g., a value of 10 means that climate forcing is an order of magnitude more important).

The metric is dimensionless but we will add a label. We will remove the arbitrary 0.75 threshold, which referee 1 asked for as well, but we will also add a line at 1.0, simply to provide reference. We prefer not to expand the range of panel b, as the important aspect of this is how early the metric becomes O(1) or larger, and this may be lost otherwise, but we will better emphasize this in text.

L. 283-284: “For Smith, the ratio rises above 1 by 2050”, but falls again below 1 after 2085. Please also comment this behaviour. How can it be that there is a period over which the initial sate becomes unimportant followed by a period over which the initial state is important again? Is the conclusion that “the impacts of climate emerge relatively quickly (i.e. over 2-4 decades) for Pine Island and Smith” (L. 356-357) really valid for Smith given that they decrease afterwards?

The peak and descent is misleading when looking at Fig 6 alone. In fact, the descent occurs because of crossings of TC85 and TCBase (and of Snap85 and SnapBase) as the as they diverge from each other, making the numerator of Eq 5 become small. When taken together with Fig 5f, it is more clear that the descending branch is not meaningful; we show it regardless as we want to represent the metric into the 22nd century.

More generally, Fig 6 is simply a means of quantifying patterns we would otherwise point out from Fig. 5. Fig 6 helps with objectivity but does not provide new information, and the figures should be examined alongside one another. We will make this clear in the text.

L. 284-285: “aside from a small spike in 2160, the ratio for Thwaites remains < 25% until 2090”. I think the authors refer to the peak in 2060, not 2160. It could also be mentioned that the ratio remains below 1 until ~2140.

Yes, this is correct, it will be fixed.

L. 285-286: I also find interesting that the Pine Island ratio becomes so high after 2120.

In our opinion this is also clear from fig 5d due to the clear divergence of initialisation-determined paths based on forcing (though this is subjective, which is the reason we include Eq 5 and Fig 6). It could be because forcing seems to have a large impact on thinning in the catchment, but thinning of thwaites also creeps into the PIG catchment, which could affect the PIG signal in the longer term (as we point out at the end of the results section), and so we wish to avoid bringing too much attention to this later-stage behaviour.

L. 286: “the ratio becomes significant for Thwaites (and other catchments)” -> the ratio is significantly greater than 1 over most of the simulated period for the three catchments.

We will change

L. 288-293: why not mentioning Smith in this paragraph?

Smith somewhat opposes the trend we are describing, as there seems to be a weaker forcing differential but also a weaker influence of initialisation. We will add a sentence for completeness.

Fig. 7: why showing ocean temperatures only in year 2100? This year is quite specific with a high emergence of the climate forcing dependence in the melt rates of all ice shelves (Fig. 6b), and there is natural variability, both of which would encourage averaging over a longer period, and even showing several distinct periods.

We choose this time period as opposed to a much later date when there will be larger differences in grounding line retreat of thwaites between forcing scenarios, making comparison less meaningful. Thus we see this as an epoch late enough to see a strong difference in forcing, but not so that that comparison is hindered.

L. 329: "Reed et al. (2024)" -> (Reed et al., 2024)

We will fix

Fig. 10 caption: indicate that this is Thwaites. And I don't see thin and thick black lines, I see grey and black.

Yes, 'thin black line' should be 'thin grey line'. We will fix this and say it is thwaites.

L. 450: "Marine Cliff Instability" -> Marine Ice Cliff Instability

We will change this

L. 466: "To the extent that ocean warming of the continental shelf is reversible": It seems to be reversible, to the extent that global warming is reversible (Caillet et al., 2023).

Thanks very much for pointing out this paper as it seems relevant, we will cite it in this context.

In the Discussion or Conclusion, it may be worth adding a comment about the fact that you regionally obtain much longer predictability than what McCormack et al. (2026) obtained at the scale of Antarctica.

Thank you for bringing this to our attention as well. The statements of this study are subtly different – they look at the time scale on which nonlinear feedbacks related to ice-sheet dynamics dominate loss, rather than the time scale on which forcing dominates it. Thus, results are not directly comparable. However, given the overlap, we will mention in the conclusion.

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