Responses to Reviewer #1

We thank reviewer #1 for their careful and thorough review of the manuscript. Below we copy the referee comments in black and write our responses in blue.

General comments

This paper is a major step forward for coupled ice sheet–climate modeling. It presents results from the first simulations using a complex Earth system model with full two-way coupling of ice sheets to the atmosphere and ocean, for both the Greenland and Antarctic ice sheets (though Antarctica is the focus here). Many earlier studies have argued for the importance of coupling and speculated on what might happen when feedbacks are included. Here, these speculations are put to the test, in ensemble simulations to 2100 for both low-emission and high-emission scenarios. The authors explain why their study is novel, while giving due credit to previous work. The paper is well structured and clearly written, with figures and tables that effectively illustrate the main findings.

The results are both plausible and interesting. For me, the most important findings are (1) the intrusion of warm water into the Ross and Filchner Ice Shelf cavities by the end of this century under a high-emission scenario, with consistent timing across ensemble members; (2) the absence of a strong response in the Amundsen Sea region, where warm water is already present in cavities; (3) the fact that increased snowfall (a near-term response to warming) adds more mass above flotation than the ice sheet dynamic response can remove by 2100, but with the likelihood that the dynamic response would accelerate in the 22nd century. There are many uncertainties – related, for example, to the coarse ocean resolution and the challenges of ice-sheet spin-up. The authors acknowledge these uncertainties and are careful (except for a few minor cases noted below) not to draw conclusions that go beyond the data.

We are grateful to reviewer #1 for recognising that while our study has limitations, it is the first of its kind, and offers several new scientific advances that have not previously been possible. We are grateful to the reviewer for pointing out a few areas where we can better caveat our results, given the acknowledged limitations of this ambitious work.

I have some suggestions to sharpen the text and to guide readers who may be unfamiliar with some of the details, but no major criticisms.

Specific comments

p. 5, l. 127: Here or below, it would be useful to say more about how NEMO computes basal melt rates. It would be good to know, for instance, whether the melt rate is a strictly linear function of the thermal forcing, or if it also depends on the speed of the sub-shelf ocean current.

NEMO computes the melt rates based on the approach described in paper cited (Mathiot et al., 2017) but we understand that readers may not be aware of this, so we will be pleased to expand the associated discussion.

p. 6, l. 162: “ice sheet model projections are typically initialized without any spinup”. I would say “often” instead of “typically”, because many ice sheet models (roughly half the models in the ISMIP6 projections) are spun up in some way.

We agree and will change this

p. 7, l. 211: Were basal melt rates assumed to be zero in the Cornford (2016) initialization? If so, please state this, since the tuned ice-shelf viscosity could be compensating for missing basal melt.

In the Cornford et al. (2016) initialisation, for a given ice sheet geometry (informed by observations) the model flow equations are instantaneously inverted to find fields of basal friction and stiffening coefficients that minimise the difference between model velocities and those derived from satellite observations. A short relaxation phase follows, where the model is run forward with mass balance boundary conditions provided by surface mass balance and basal mass balance diagnosed to balance the SMB and flow divergence at all points on the ice shelves, and thus maintain their initially prescribed geometry. We will expand the paragraph to describe this.

p. 8, l. 221: What is the magnitude of the steady value where the rms thickness rate settles? In what regions is the remaining drift largest? Did you do multi-century standalone ice sheet runs, continuing with the same forcing? Such runs would increase confidence that the drift is small enough to maintain stable grounding lines.

The approximately steady ice sheet-average RMS dh/dt for the 4 simulations are 0.4, 0.35, 0.38, and 0.34, after 17, 28, 29, and 30 years respectively, and the remaining drift is largest in isolated cells near the coast, and not in regions of dynamically evolving glaciers feeding ice shelves. We will add a discussion of these points to the paper. If we continued these standalone simulations, the grounding lines would continue to retreat because ocean melting is out of balance with the ice sheet in the present day. In any case, our experimental design strongly mitigates any concerns over model drift. Throughout the paper we compare SSP1 to SSP5 projections. Therefore SSP1 serves as a 'control'. To leading order, the effects of model drifts will cancel between SSP1 and SSP5, thus revealing the influences of anthropogenic forcing, which is our main interest.

p. 9, Fig. 1: In general, the figures in the paper are informative and easy to interpret. However, many figures (including this one) use a rainbow colour scale that could be problematic for colour-blind readers. Please consider a different scale.

We will revise the figure colour scales where possible.

p. 11, Fig. 3: In Figs. 3a and 3b, there is good agreement with observations in the thinning of Pine Island and Thwaites Glaciers and thickening of the Kamb Ice Stream. My understanding is that this is largely the result of tuning basal coefficients to match observed ice speeds in the BISICLES spin-up. We would expect the tuned velocities to change the thickness in places where ice flow has recently accelerated (for PIG and Thwaites) or decelerated (for Kamb). This can be inferred from the text, but could be made more explicit for readers who are unfamiliar with tuning strategies.

We will expand the text on lines 289-290 to make this much clearer to the readers.

Another notable feature of Fig. 3 is the general slowing and thinning of ice shelves. The text (p. 12, l. 298) attributes the thinning to the SMB and basal melt forcing from the climate model. Is basal melt primarily responsible for shelf thinning, or does SMB also play an important role?

The thinning is caused by discrepancies in basal and surface forcing between the Cornford et al. (2016) and Martin et al. (2019) initialisation and our relaxation process. Such discrepancies are inevitable without a formal coupled ice and climate initialisation procedure (which is an important but very challenging avenue for future work). There are regions where SMB discrepancy is larger than the basal melt discrepancy, but the basal melt difference is larger around the grounding lines. We will expand the text around line 298 to describe this.

p. 12, l. 297: Why would this slowing mostly occur during the first year of the standalone ice sheet initialization stage? I would expect it to occur more gradually as the shelf thins.

The slowing is caused by the change of basal melt forcing around the grounding lines when we first use melt rates from an ocean model rather than the diagnosed melt rates in the initialisation as described above. The ocean model cannot accurately represent the very thinnest cavities near the grounding line, and so no melting occurs there in the modelled fields. Therefore, the ice generally thickens near the grounding line upon coupling, leading to small grounding line advances. This is one of the reasons why this ice relaxation period is important. The increase in drag from these re-grounded areas is instantaneously transmitted through the ice shelves, so this causes the ice shelves to rapidly decelerate. We will expand the text to fully explain this point.

p. 12, l. 302: You refer to “the SMB and basal melting implicit in the inverted reference velocities”. I am not sure what this means. My understanding is that the SMB and basal melting in the BISICLES spin-up are part of the input forcing, with SMB based on reanalysis and basal melting (possibly?) set to zero. In that case, the shock could be attributed to the fact that the SMB and basal melt rates in the adjustment process (derived from the UKESM historical run and standalone ocean spin-up) are different from the SMB and basal melt rates in the spin-up. It would be helpful to describe or plot the differences.

As described in the above response to the comment on line 211, the SMB is imposed in the initialisation, and dh/dt = 0 is imposed on the ice shelves, and so this means that the implicit basal melting varies as part of the Cornford et al. (2016) initialisation. It is correct however that the shock occurs because the SMB and implicit basal melt rates in this initialisation differ from those derived from the UKESM historical run and standalone ocean spin-up. We will describe these differences in the revised paper.

p. 15, Sect 3.2.1: The abrupt transition to warm water for the Filchner Ice Shelf is a very interesting result in an ESM, consistent with the recent regional studies.

We agree that this is fascinating. This has been previously found in ocean-only and ocean/ice models, but our study is the first to show this in a coupled climate model.

p. 16, Fig. 6: Since the transition occurs near 2100, I suggest extending the x-axis to 2115, if possible.

We will extend the axis.

p. 15, Sect 3.2.2: The transition to a warm Ross Ice Shelf cavity is another very interesting result, notwithstanding the fresh bias in the Ross Sea. Some other studies also show freshening in this region in the beginning or near mid the 21st century.

We feel that this is a key result of our study and we believe the Ross Sea shelf warming is a new result. We are aware of a paper suggesting a freshening in this region - Timmermann and Hellmer (2013); Jacobs et al. (2022), so we will add a discussion of those papers. The region has been freshening for the last 60 years, as noted in one of those papers, but that has not yet led to a transition to a warm state.

p. 29, l. 629: “These results may indicate the bigger potential that Ross/Weddell sectors have in becoming major sea level contributors in future warming scenarios.” Here, “bigger” seems to mean “bigger than PIG and Thwaites”. It’s true that the Ross and Weddell sectors have the potential to become major sea level contributors, but it’s also true (based on present-day observations and published simulations) that PIG and especially Thwaites could be major contributors. The Amundsen Sea contribution might not be captured by the model, for the reasons discussed in Section 4.3. So I suggest rewording this claim.

We are grateful to the reviewer for bringing this to our attention and will certainly re-word the claim, to say that the Ross and Weddell sectors have a ‘large’ potential, rather than ‘bigger’.

p. 29, ll. 642ff: This paragraph is a good summary of the novelty and importance of the study.

We thank the reviewer for their supportive comments.

p. 30, l. 666. The SROCC is cited several times, but I couldn’t find a citation of AR6. Please add AR6 citations where appropriate. For context, I suggest including the projected GMSL from Antarctica under low and high forcing scenarios, according to AR6.

We will update this text to cite AR6.

p. 30, l. 667: It's plausible that the AIS would have a positive mass balance in this century, but it's misleading to call this a "rapid sea level fall". The snowfall contribution is better described as a modest offset (~2 cm) to a robust global trend of rising sea level (28 to 55 cm by 2100 under SSP1-19, and 63 to 102 cm by 2100 under SSP5-85, according to AR6).

We will expand the text to say that while the AIS contribution to sea level will be significantly negative, this would comprise a modest offset to the overall expected increase. This remains a very important conclusion, however, since the current paradigm is that the AIS could contribute a rapid rise in sea level to 2100.

p. 31, l. 701: "… do not retreat." Doesn't Fig. 15 show some GL retreat for PIG and Thwaites?

We will reword the sentence to clarify that we meant that the GL in SSP5 does not retreat far relative to that in SSP1.

p. 31, l. 705: "Nevertheless, the impact of a future strong climate change in Amundsen Sea cavities is unlikely to be larger than our modelled changes in the Ross/Weddell cavities. This is because the Amundsen continental shelf and ice shelf cavities are already filled with the warm Circumpolar Deep Water and hence there is less potential for further warming and strong ice response." I think this is a bit too strong. It may be true that the ASE cavities have less potential for further warming, but this does not imply less potential for strong ice response. Because of its bed geometry, Thwaites might already be retreating unstably, or might be near a threshold such that it could be tipped into unstable retreat with a small amount of additional warming.

We agree that the sentiment expressed in this sentence is highly uncertain. We will rewrite this section to acknowledge that bed geometry may play an important role in modulating the role of changes in forcing, and that in fact the Amundsen Sea could experience warming, as warmer Circumpolar Deep Water at 2C is present offshore of the shelf break in this region.

p. 32, l. 715: "do not appear to simulate". I suggest "do not simulate".

We will change this.

p. 33, Section 5: Many conclusions already appear in the Discussion section. Since some readers will look at only the Abstract and Conclusions, I suggest adding some content in Section 5. For example:

You say here that these are the first AOGCM runs with full two-way ice-climate coupling; you could add a sentence or two (as in Section 4.1) about why this is important.

You could point out that the Filchner warming is consistent with previous modeling studies, whereas the Ross warming is something new.

You could mention the ASE non-response, with appropriate caveats about uncertainty.

I would not end the paper with a sentence that refers to the Ross Ice Shelf alone.

We will briefly add the above points to the Conclusions, while also trying to keep it short and focussed as far as is possible.

Technical corrections

We thank the reviewer for their careful reading and will address all of the suggested changes below.

p. 1, l. 21: "of the 21st century"

p. 5, l. 137: The phrasing is awkward, with two uses of "along with"

p. 7, l. 189: "integration" without the "s"

p. 7, l. 210: "caving" -> "calving"

p. 13, Fig. 4 caption: “The white boxes”

p. 14, l. 330: “where the SSP1-EM melt rates become”?

p. 14, l. 334: “large cold” -> “large, cold”

p. 16, l. 358: “on the ice front” -> “at the ice front”

p. 16, l. 360: “The shelf” -> “Water on the continental shelf” or something similar. In general, please be specific where the two meanings of “shelf” could be confused.

p. 16, l. 361: “becomes” -> “is” (since the deep water is not becoming denser in an absolute sense, but only relative to the shelf)

p. 16, Fig. 6i: In this panel the x-axis is different from the other panels.

p. 18, Fig. 8a: The dashed lines in this panel are hard to see in the ice shelf and continental shelf regions.

p. 18, l. 415: Add comma after “simulation”

p. 20, l. 441: Delete “is” before “already”

p. 23, l. 501, Fig. 11 caption: Typo in “SSP5-EM”

p. 24, l. 516: “on Queen Maud Land” -> “in Queen Maud Land”

p. 25, l. 539, Fig. 13 caption: It’s not accurate to describe the right-hand panels as “changes” like the left and middle panels. Please reword, e.g. using “differences”.

p. 26, l. 576: “end of the 2060s”

p. 27, Fig. 14 caption: The descriptions of the middle and bottom rows are reversed. In the fourth line, delete “the” before “West Antarctica”. In the last line, “column” -> “columns”.

p. 27, l. 588: “area-integrated” with a hyphen; delete “area” after “grounded ice sheet”

p. 27, l. 591: “from the 2040s”

p. 28, l. 598: Change “retreat up to 40 km takes place under southern Thwaites Glacier” to something like “the grounding line of Thwaites Glacier retreats southward by up to 40 km” (since the southern part of Thwaites Glacier lies far in the interior). Similarly for PIG.

p. 28, Fig. 15: It took me a few moments to get my bearings for the left and middle panels, which are rotated with respect to the standard view in a polar stereographic projection (e.g., Figs. 11-13). Maybe rotate back to the standard view. On the left panel, perhaps add labels pointing to the Thwaites and PIG ice shelves.

p. 29, l. 639: Instead of “a while”, maybe “longer.

p. 29, l. 647: “modern-day” with a hyphen, or just “modern”. Similarly, “present-day” at l. 652.

p. 31, l. 696: “end of the 21st century”

p. 32, l. 726: I can’t tell if there is a paragraph break after “century”. If not, please add one.

p. 33, l. 757: “brings” -> “bring”

p. 33, l. 759: Add a period.

References: Please check for consistent capitalization in paper titles.

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