Responses to Reviewer #2

We thank reviewer #2 for their careful and thorough reviews of the manuscript. We first give a general response to the main comments and major concerns. Below we copy the referee comments in black and write our responses in blue.

The manuscript “The Antarctic contribution to 21st century sea-level rise predicted by the UK Earth System Model with an interactive ice sheet” by Siahaan et al. presents perhaps the first results of a climate model coupled to an ice-sheet model of the Antarctic Ice Sheet. The paper presents a protocol for offline data coupling of UKESM and BISICLES and explains the initialization process for this complex configuration. After this, results are presented for small ensembles of coupled simulations following both SSP1-1.9 and SSP5-8.5 greenhouse gas emission scenarios. SSP1-1.9 shows small changes from initial conditions, while SSP5-8.5 leads to ice-shelf basal melt regime change beneath Ross and Filchner-Ronne ice shelves, and associated ice-shelf thinning and acceleration. The manuscript discusses the major terms of the ice-sheet mass balance and considers unique aspects and limitations of the modeling approach presented.

The manuscripts presents a significant achievement of running coupled climate and ice-sheet models for Antarctica. This has been a community goal for many years, and these results are the first to achieve it that I am aware of, even given the “offline” coupling method employed. On the other hand, the methodology for achieving it includes a number of questionable choices with an unclear impact on the resulting simulation. Most significantly, the initialization procedure for both the climate model and ice the ice-sheet model appears a bit ad hoc, with a number of details of the protocol not clearly described. It appears there may be significant artifacts and model drift associated with the ice-sheet model initialization that have not been quantified. The authors do a fair job of noting shortcomings, but more work could be done in quantifying the impacts of those choices.

On the whole, this is an impressive modeling achievement, but the scientific utility of the results is questionable without further substantiation. The authors themselves note these limitations, and I wonder if this paper would not be better suited for a journal like GMD. Below I focus on two major areas that require more work - description of the coupling and description and analysis of the initialization procedure. I then list a number of smaller issues that require addressing. Even after significant additional analysis, it may be that the initialization and coupling procedure leave the scientific intepretation of the results ambiguous. The authors may wish to consider if a model description journal would be a more appropriate venue for this work, where it would be a significant contribution.

We are grateful to the reviewer for their careful and thoughtful review, and we agree with most of the points raised. We are very pleased that the reviewer recognises this work as a significant achievement, and a major ‘first’ in our field. However we also fully accept that this ground-breaking work has uncovered a few areas that will require a lot of work to get right - most notably in the initialisation. The reviewer is absolutely right to raise these areas. In order to accomplish this first study, we made a series of choices in these areas which we would like to examine more fully in future. However, we feel that our experimental design means that the present manuscript is a very valuable scientific (not only technical) contribution which will be of interest to an audience outside of the model development context and with conclusions that stand despite these questions. We detail our main arguments below, responding to the main points raised in the reviewer’s introductory text. These arguments are fully expanded in the responses to the reviewer’s specific points.

Results are sensitive to initialisation and coupling choices: There is certainly much work to do in understanding the best way to couple ice sheet and climate models, and how to initialise such a coupled system. However, our guiding philosophy throughout this work has been that the importance of these choices is strongly mitigated by our experimental design. Throughout the paper we qualitatively compare SSP1 and SSP5 projections. Our conclusions are based on this difference alone, with SSP1 effectively acting as a ‘control’ simulation. Since the impact of initialisation shocks, model drifts, coupling choices, etc. will be expressed in both scenarios, to leading order their differences are independent of these features. On reflection, this overarching philosophy was not expressed clearly enough in the submitted manuscript, and was not repeated in areas where it is relevant, so we will substantially rewrite the paper to clarify this throughout.

Initialisation procedure is ad-hoc: Our ambitious coupled modelling work highlights the important and very challenging area of coupled ice–climate model initialisation. There is a fundamental difficulty that climate models are spun up using pre-industrial forcing, while predictive ice sheet models are commonly initialised in the present day using inversion techniques. Any ice–climate coupling will therefore induce some kind of discontinuity into the simulations, and removing the impact of this is a challenging task that will require years of dedicated effort. In the present study we have devised a strategy that works, involving defensible choices, and with acceptable impacts. Since our experimental design mitigates these impacts even further, we believe that this issue should not delay publication of the substantial scientific advances that we have made with this coupled model. To address the reviewer’s comments in this area, we will expand the discussion of the challenges of coupled initialisation, and outline possible future avenues for minimising the resulting impacts.

Initialisation and coupling procedure are not fully described: The coupling procedure is very fully described in Smith et al. (2021) and that is quite briefly described here. We will expand this slightly where recommended by the reviewer. The initialisation procedure is complex and not fully described elsewhere, so we will expand the discussion substantially, including the above philosophical points. However, there are limits to this because this is a scientific paper, not a model description paper, as justified below.

Work may be more suitable to a model description journal: We considered this point at length, both before the original submission and again in response to the reviews. While this manuscript represents a substantial modelling advance, much of the model development is already described in a technical paper (Smith et al., 2021) and the conclusions of the present study are scientific, not technical, in nature. In particular, our conclusions on warming of the Ross and Weddell seas, and the importance of surface mass balance in creating a negative Antarctic contribution to sea-level at 2100, are scientific advances that will be of wide importance and impact. We will revise the paper by more clearly stating that our intentions are to analyse scientific hypotheses, including at the end of the introduction.

MAJOR CONCERNS

1. Better description of coupling.

I recognize that the coupling methodology was previously described in the Smith et al. (2021) JAMES paper, but given the importance of the coupling to the science results, inclusion of more information here is warranted. Some specific suggestions are:

142-6: The description of the coupling protocol is vague. Given this is a significant novelty of this work and coupled model results can be very sensitive to the coupling procedure, it should be described in much more detail. Please provide evidence that the results are not sensitive to the chosen coupling interval.

The coupling protocol is exactly that described at length in Smith et al. (2021), but we will expand and tighten the summary of these details in the present paper.

Early in the development of the coupled model we examined the sensitivity to coupling interval and satisfied ourselves that 1 year was adequate for the flows in the resolutions characteristic of the global models we are using. However this was many years ago and unfortunately the results are no longer available. It would be prohibitively difficult to re-run these tests with the present coupled model as it would require substantial development of the coupling suite. However the 1-year coupling step is not controversial for these kind of experiments with slow centennial evolution of the climate forcing. Favier et al., 2019 (Geosci. Model Dev., 12, 2255–2283, 2019) indicates very little sensitivity to coupling periods between 1 month and 1 year, while Zhao et al., 2022 (Geosci. Model Dev., 12, 2255–2283, 2019) indicates very little sensitivity to the coupling time interval between 0.5 days and 3 months. We will modify the paper to discuss this point and cite the papers.

130: This sentence is unclear - please clarify or expand on this. Also, please add a description of how retreat of the ice-sheet grounding line is handled by the ocean model.

We will modify the paper as suggested. This is a complex area as the depth-integrated ocean transport must be preserved as the ice geometry changes, in order to avoid creating 'tsunamis' in the ocean surface. A retreat of the grounding line is actually the simplest case, since the new ocean column that is created can simply be given zero ocean transport. An advance of the grounding line is more complex, as some ocean transport then 'disappears'. The full details are given by Smith et al. (2021), but we will certainly expand the discussion in the present paper.

126-146: Please acknowledge in this section that the calving front restriction and the bilinear remapping prevent the system from conserving mass and heat, though these errors are not likely to be significant for the experiments being conducted.

We agree with this and will add this acknowledgement.

2. Model initialization and its impacts.

Initialization of coupled climate and ice-sheet models is a very challenging problem. Still, the procedure described here appears ad hoc, missing key information, and does not include an assessment of key initialization choices on the results. The specific point below detail these concerns:

This is a very complex area and we agree its explanation could be clearer. In response to the comments below we will substantially re-write section 2.3. After some consideration we think we can improve the clarity of this section by clearly labelling the different stages of the simulations, as follows:

A. standalone ice model inversion (Cornford et al, 2015)
B. standalone ice model relaxation (Cornford et al., 2015)
C. generate ocean cavity properties (1 simulation, 45 years)
D. add ocean cavity properties to global UKESM ocean and relax (4 simulations, 15 years)
E. relax ice model under UKESM ocean cavity and surface mass balance forcing (4 simulations, 17-30 years)
F. coupled projections (8 simulations, of which first 5 years is regarded as spinup)

We will rewrite the section to describe each of these stages, and will add a schematic diagram illustrating them.

179: This paragraph is confusing and is missing key information.

The issue under consideration here is that we have the CMIP6 UKESM simulations for historical periods but those have a static ice sheet, and no cavities beneath floating ice. They simply have a coastline at the ice front. Therefore we need to generate initial ocean properties and ice shelf melt rates within the sub-ice cavities from a short run of the coupled model. We will expand and clarify this paragraph.

182: 45 years seems like a short ocean spin-up time. Can you provide justification that the most important transient behavior had reduced to small levels prior to creating the branch runs?

In retrospect, the original manuscript was not sufficiently clear on the purpose of this initial 45-year simulation (stage C above). Its only goal here is to produce some preliminary ocean properties in the ice shelf cavities, and 45 years is more than enough to accomplish this. In stage D, these cavity properties are then joined on to global ocean states from UKESM historical runs, and that is run forward for 15 years in order to flush the cavities with ocean properties from the global UKESM. Thus the convergence of stage D is important to the simulations, but the convergence of stage C is not. This will be fully described in the paper, and the convergence of stage D described (see below).

179-194: After reading the full paragraph, I am more confused. Replace the opening paragraph by clearly stating the initialization process is based on a hybrid state of the ice cavities from UKESM1.0-ice stitched onto the global ocean state of UKESM1.0. Also, a schematic of the various runs and processing, would help communicate this process.

This is a complex area and we will certainly replace the opening paragraph as requested and add the schematic.

186: Emphasize for the reader that UKESM1.0 does not include ice shelves, e.g. add a parenthetical "(without ice shelves)" after "UKESM1.0".

We will add this emphasis, reminding the reader that the suffix 'ice' in UKESM1.0-ice is added to refer to models with ice shelf cavities and an active ice sheet.

187: Which years were chosen? Can you elaborate on what "a range of variability" means? How many ensemble members were in the UKESM1.0 CMIP6 historical ensemble? Also, do you have a reference for that ensemble, or a reference to the dataset on ESGF? What does "end of the 20th century" mean? What specific years were used?

The UKESM1.0 CMIP6 historical ensemble contains 19 members, and is most usefully cited via Yool et al, "Evaluating the physical and biogeochemical state of the global ocean component of UKESM1 in CMIP6 historical simulations" GMD 14, (2021). Three of the initial states were taken directly from this ensemble, all from the year 2000 in their respective members. They were chosen to maximise the range of ACC strength and Southern Ocean annual average SST across the available UKESM ensemble members in that year. An error was made in configuration meaning that the fourth was not, in fact, taken from the UKESM historical ensemble, but from the preliminary UKESM1.0-ice simulation mentioned in the paper. However, since this had not been run long enough to drift far from the 1995-2014 average EN4 climatology it was initialised with, it provides a state more representative of the observed modern ocean than the spun-up UKESM historical ensemble that contains systematic biases characteristic of UKESM, so may show if those biases have a significant impact on our projections. We will add clarification of the source of our initial states to the revision.

189: What does “short” mean? And what does “balance” mean on line 190? Please show evidence of balance or behavior that is close to steady state, either in the form of a plot or some statistics.

We will replace ‘short’ with ‘15-year’, as described in the following paragraph. We will add some statistics to this section showing that the melting is steady.

196: Please explain why the ocean-sea ice simulations are regarded as beginning in 2000 if they were branched from specific times of UKESM and included a cavity state from perpetual 1970.

We will expand the discussion. As noted above in the comment about line 187, the global ocean-sea ice states were taken either from the year 2000 in one of the members in the UKESM1 historical ensemble, or represent the modern observed ocean (averaged 1995-2014). The ice-shelf cavity information does come from perpetual 1970 forcing UKESM1.0-ice simulations (stage C), but is merged with these global ocean states and run for 15 years with UKESM surface forcing from 2000 to 2015 (stage D). This flushes the cavities and bring them in line with the global ocean, and produces an ocean state representative of year 2015.

197: Are the atmospheric fluxes from the same runs that each was branched from? Or one common set of atmospheric fluxes?

We will add a note that the atmospheric fluxes were taken from the same ensemble member as the initial global ocean state.

195-202: So the final initialization step is 1) not fully coupled to the atmosphere and 2) has temperature and salinity restoring applied. Given that, it is unlikely that you can branch into a fully coupled projection without some shock and drift. Please justify this choice.

We will expand the discussion at this point to reflect two philosophical points and one direct answer to the reviewer’s comment.

Firstly, the topic of coupled ice-climate initialisation is a challenging research question that will take much future research to fully solve. Secondly, our experimental design strongly mitigates the influence of any coupling shock. Thirdly, we believe that our strategy for this particular issue is broadly defensible and is hard to improve upon.

Stage D of the initialisation procedure is intended to produce ice-shelf cavity ocean properties that are consistent with the UKESM1.0 ocean state in the wider ocean. There is no perfect way to do this, but we believe our strategy is the best available. We are constraining ocean surface fluxes to match UKESM1.0, and restoring ocean properties outside the ice-shelf cavities to match UKESM1.0. Thus, we believe that in 2015, after 15 years of this procedure, we have ice-shelf cavity ocean properties that are the best match possible to the wider UKESM1.0 global ocean states. Thus, when we subsequently run this forward as a coupled model, we do not expect large ocean-state shocks to occur. The ‘perfect’ solution to this problem would be to have a historical climate model simulation with ice-shelf cavities, but these are not currently available. This is our strategy for the next generation of the UKESM.

195-202: Throughout this section, please clarify if every time you refer to UKESM1.0-ice, prognostic ice-shelf basal melt fluxes are on and what is happening with iceberg fluxes (if anything).

Both ice shelf basal rate and iceberg calving, drift, and melting are always prognostic in UKESM1.0-ice. We will state this as requested.

214: What is the thickness output of the inversion procedure? In the previous sentence it is only said that basal drag coefficients and viscosity are adjusted.

The thickness output is described fully in Cornford et al. (2016). Over the ice shelves it is equal to the observed ice thickness from Bedmap2. For grounded ice, it is this ice thickness after the inversion (stage A) but subject to relaxation (stage B). We will add a discussion to the paper, and a citation to Cornford et al. (2016).

216: Ice-shelf melting from what year(s) of each standalone run?

In order to test the influence of interannual variability, two members use 2014 melting whereas two others use the 2010-2015 average, in common with the SMB forcing noted in line 218. This will be noted.

221: Referring to this 'spike' is vague, as is "about 20 years". Please report statistics or, better, show a time series of this RMS metric.

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. These statistics will be added to the paper. Ice simulations in stage E were motivated by a wish to remove any immediate coupling shock on introducing UKESM surface and basal mass balance fields to the ice sheet model and ensure numerical stability of the initial steps of the interactive ice-climate system in stage F. Stage E simulations were 30 years long. The ice states taken forward to phase F were chosen to have a continental average RMS dh/dt of less than 0.5m/yr and a maximum magnitude of dh/dt in any location less than 75 m/yr. The state taken at year 17 was taken earlier than for the others to avoid the effect of a significant transient spike in both measures of dh/dt that occurs near the end of that simulation.

204-223: It is entirely clear what year the ice sheet initial condition represents. In line 212, it is stated the ice-sheet state starting the procedure represents "early 21st century", but then it is relaxed for "about 20 years". How does the final state of each ensemble member compare to recent observed thinning rates, which have been highly variable in time?

The ice sheet inversion (stage A) represents the ice thickness from the Bedmap2 dataset and the ice velocity from the Rignot dataset. These data are from a range of dates and so the inverted ice state does not have a clear uniform time-stamp other than 'early 21st century'. In stage E, the ice sheet is then evolved for a further 17-30 years so that this ice sheet state can adjust to the four different realisations of 2014 or 2010-2015 basal melt forcing produced by the UKESM. So the ice model state upon coupling does not have a clear time stamp, other than it is 'early 21st century'. This will be fully described in the revised paper.

Figure 3 and associated ext (286-292): The very large amount of noise (presumably transient flux divergence) in the ice-sheet elevation/thickness change deserves a few sentences of explanation. While this is a well known and common challenge for ice-sheet models, the amount of spurious thickness change after 15 years of integration and 20 years of relaxation (if I followed the protocol correctly - it was confusing - see above) seems unacceptably large. Also, what is the purpose of panel d? It shows nearly the same information as panel b.

We will clarify this in the text in the revised manuscript. We do not feel that the noise in Figure 3b is unacceptably large - it is of order 0.1 m/y. Crucially, as shown in figure 13 and discussed further below, this 'noise' exists in both the SSP1 and SSP5 projections, and is cancelled in their difference. Therefore, despite the 'noise', we are able to determine the influence of climatic forcing. The purpose of figure 3d is to show the total ice thickness change over the ice model relaxation (stage E), while the purpose of figure 3b is to show the initial trend during the projection period.

297: It is not obvious to me why ice shelves would slow so significantly in the first year just due to one year of thickness change coming from surface and basal mass balance, especially if further adjustment after one year is small. Is it possible grounding line positions have shifted or the ice temperature field changed or something else is going on? A more thorough explanation is warranted.

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 'implicit' melt rates in the ice initialisation (stages A and B). 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 realise that this was not well explained in the original manuscript and will expand the text to fully explain this point.

Section 3.4: It would be easier to interpret the changes presented if there was also a standalone BISICLES control run that had constant 2015 forcing. Presumably the speckly pattern of thinning and thickening in Fig. 13 panels a and b is due to unrealistic transient behavior in the initial condition. That is a common problem in ice-sheet models, so it does not necessarily invalidate the results, but it should be clearly identified. I would prefer to see additional results for an unforced control run. Without it, it is difficult to assess what aspect of these results are an effect of the ice-sheet model initialization procedure and what is due to the climate forcing coming from UKESM.

We thank the reviewer for pointing this out. The 'noise' in figure 13 is the same between SSP1 and SSP5 scenarios, so it must be associated with the initial state. However we feel that this 'noise' is not large relative to the dynamic thinning signals of interest, and most crucially of all, it does not appear when we difference SSP1 and SSP5 in figure 13. This means that the climatic signals that we are focussing on are not influenced by the 'noise. This will be fully described in the revised paper. We considered the possibility of conducting a standalone ice control run but this is problematic for two reasons: i) the ice would evolve because ocean melting is out of balance with the ice sheet in the present day and ii) it is not clear what melt rates we would use when the grounding lines retreated.

Other Comments:

Abstract: Mentioning Greenland in the abstract is slightly misleading, because the Greenland results are not part of this paper.

We will clarify that while Greenland is coupled into the model, we only analyse Antarctica here.

47: Also cite the only paper that demonstrates this for the observational period that this sentence discusses:

Gudmundsson, G.H., Paolo, F.S., Adusumilli, S., Fricker, H.A., 2019. Instantaneous Antarctic ice sheet mass loss driven by thinning ice shelves. Geophys. Res. Lett. 46, 13903–13909. doi:10.1029/2019GL085027

We will update the references

48-55: There are a lot of other important studies that would be appropriate to reference here, e.g.:

Spence, P., Holmes, R.M., Hogg, A.M., Griffies, S.M., Stewart, K.D., England, M.H., 2017. Localized rapid warming of West Antarctic subsurface waters by remote winds. Nat. Clim. Chang. 7, 595–603. doi:10.1038/nclimate3335

We will update the references

65: CMIP5 and CMIP6

We will change this.

68: One CMIP-class ESM recently published (since this manuscript was submitted) Antarctic subglacial melt rates (but those simulations were not part of CMIP6):

Comeau, D., Asay-Davis, X. S., Begeman, C., Hoffman, M. J., Lin, W., Petersen, M. R., et al. (2022). The DOE E3SM v1.2 Cryosphere Configuration: Description and Simulated Antarctic Ice-Shelf Basal Melting. Journal of Advances in Modeling Earth Systems, 14, e2021MS002468. https://doi.org/10.1029/2021MS002468

We will update the references

69-74: There also is recently published (since this manuscript was submitted) regional model that includes all physical climate components (atmosphere, ocean, sea ice, land, ice sheet):

Pelletier, C., Fichefet, T., Goosse, H., Haubner, K., Helsen, S., Huot, P.-V., Kittel, C., Klein, F., Le clec'h, S., van Lipzig, N.P.M., Marchi, S., Massonnet, F., Mathiot, P., Moravveji, E., Moreno-Chamarro, E., Ortega, P., Pattyn, F., Souverijns, N., Van Achter, G., Vanden Broucke, S., Vanhulle, A., Verfaillie, D., Zipf, L., 2022. PARASO, a circum-Antarctic fully coupled ice-sheet–ocean–sea-ice–atmosphere–land model involving f.ETISh1.7, NEMO3.6, LIM3.6, COSMO5.0 and CLM4.5. Geosci. Model Dev. 15, 553–594. doi:10.5194/gmd-15-553-2022

We will update the references

58-81: Somewhere in here you should also acknowledge the fully coupled configuration of CESM with the Greenland Ice Sheet.

We will update the references

104: Can you report the approximate horizontal resolution of the 1 degree ocean grid at the typical latitude of Antarctic ice shelves?

About 17-22 km.

111: It is worth pointing out that this adaptivity is dynamic in time.

We will write ‘time-evolving adaptive’ the first time we mention this.

117: Can you briefly summarize the impact of choosing 2 km as your finest resolution instead of 1 km or 0.5 km as is sometimes used for BISICLES?

We will update the text in the discussion section (the 2 km highest level of mesh refinement we allowed for BISICLES in these simulations may not be sufficient to accurately model the grounding line dynamics in this region (Cornford et al., 2016), although testing suggests that increasing the allowed refinement of the BISICLES to 500 m would not significantly alter our model evolution of the next few decades.)

162: I would say most ice-sheet models follow this practice, but it is not ‘typically’ the case - there are a number of ice-sheet models that do a paleo or steady state spinup.

We will use ‘often’ instead.

169-171: These comments make me wonder if this manuscript would be more appropriate for GMD or JAMES.

Though the paper describes some technical detail, the main conclusions are all scientific in nature, and so we believe this work is much better suited to The Cryosphere.

Figure 1a,b and associated text in 3.1: It is rather awkward to compare these plots to referenced observational data without showing those observations or model biases relative to them. As presented, these comparisons are not useful.

There are no complete observational data sets of the stream function or mixed-layer depth, so we just show the model results. We will mention this in the text. For temperature and salinity, we compare to observations.

264: Would not surface restoring bring properties closer to observations?

This is correct so we will remove the reference to surface restoring and then refer to the inadequacy of ocean-atmosphere surface flux in the stand alone ocean model.

249-271: This discussion of water mass properties, especially at depth, based on mixed layer depth is obtuse. It would be much better to show T&S diagrams for the regions of interest compared to observations. Many global ocean models struggle with the formation of Dense Shelf Water, even with realistic mixed layer depths, so that in and of itself is not a guarantee of good water mass properties. It would also be quite important to see maps of ocean bottom temperature and salinity, as those matter more for ice-shelf basal melting than surface properties.

In Figure 1 we show the water properties over the depth range important to ice-shelf melting, which is the focus of this study. This shows how well the water masses on the shelf are represented. We refer to mixed layer depths merely to describe why the shelf water masses appear as they are (in the real world and in the model) - it is the presence or absence of cold, salty waters produced by sea ice formation that determines this. We will clarify this in the revised paper.

271-2: Please provide evidence for this statement (e.g. the bias value for UKESM and other CMIP models). Are you basing this statement off of the version of UKESM in Heuze (2020) or the simulations presented here?

We realise that the citation to Heuze was misleading. We meant that the temperatures were broadly accurate (Figure 1) and this was superior to many other models (Heuze). We will change the text accordingly.

278: Do you mean *near-shore* fresh bias here? Over most of the Southern Ocean, Fig. 1 shows a saline bias.

We are referring to the fresh bias in the Ross continental shelf and will change the text to reflect this..

Figure 4: Similar to figure 1, this figure should include the observational references fields (or show an anomaly). Simply saying "shows a similar spatial pattern to present day observations (Rignot et al., 2013; Adusumilli et al., 2020)" and expecting a reader to pull those up and make comparisons across different colourbars is not sufficient. Also the linear colourbar is inadequate for showing the high melt rates in the Amundsen Sea - presumably the ice shelves in that entire sector are well above 5 m/yr. Similarly, the colourbar in panel e and f saturates in areas of interest (e.g., Ross and Larsen ice shelves).

We will use a nonlinear colour bar and add the satellite-derived melting.

Table 1: Presumably in your model analysis you can separate Ross and FRIS into the two halves that the observational data uses.

We can add this.

309: While the modeled melt might be within the range, I suspect a t-test would indicate a significant difference. That is not necessarily unacceptable, but please report a more careful comparison.

The errors in the observational datasets are very large and the disagreement between datasets is large. For this reason we are unsure that detailed statistics are appropriate. We will re-phrase the sentence to say that the model is loosely in agreement.

Figure 5: Please also show present-day observational estimates for reference.

We will add these to the plot

Section 3.2.1: This section demonstrates the melt regime change at FRIS very clearly, but the causal mechanism is left only hinted at. There is a plot and mention of declining sea ice volume and its possible relation to declining density. There also is a mention of increasing freshwater flux. This is already a long, dense paper, but if it were possible to tease out the mechanism(s) leading to WDW increase, that would be a valuable contribution. Have you looked at changes in wind stress? The previous papers you cite also discuss that as a potential mechanism for the WDW intrusion at FRIS.

Our results share many aspects with the cited papers, so we choose to rely on those for FRIS and provide a fuller explanation of the changes in the Ross Sea. This will be described more fully.

424-6: From Figure 9, it looks like a missing piece of this explanation is that Dense Shelf Water (cold and saline) on the continental shelf is present at the start of the simulation, but becomes significantly fresher by 2060 (Fig. 9e). This is consistent with the sea-ice decline mentioned and shown in Fig. 8b to become more substantial around that time. Similar to the

FRIS case, the reduction in the continental shelf density barrier facilitates the intrusion of mCDW. This series of events is alluded to in this paragraph, but the sentences at 426-7 implies that the driving mechanism is warming of the mCDW, which is not apparent in Figure 9. Maybe this just requires some rewording.

We agree with this narrative and will change the wording.

432: As you say, I think the relative model fresh bias in each of these regions is critical. To further illustrate that point, could you follow up with a quantitative metric of the salinity bias in each region at the start of the simulation? (e.g. averaged over the region or at the shelf break analysis point used in each region.

We will add a metric

440: You haven't shown that the regional climate is warming during this period. Maybe reword to "While the changing climate".

We will reword this

441: Remove "is".

We will reword this

Fig. 10: What year and simulation are these draft values from?

Year 2080 from one of the SSP5 members. We will clarify the caption to this figure.

Section 3.2.3: Initially I was skeptical of even discussing results from an ice shelf represented by 11 grid cells, but I appreciate the honest assessment of what is happening in the model here, given the importance of this region. Better to acknowledge the limitations of interpreting these results than ignore it and risk readers reading their own interpretation into it.

We agree

491: Similar to previous comments, simply stating your results look similar to your observational reference is insufficient. Please include a panel in the figure showing the reference dataset (or the difference from it).

We will add the data

Table 2: Please also include a present-day estimate (e.g. from RACMO).

We will add this

525: Another relevant reference here: Trusel, L.D., Frey, K.E., Das, S.B., Karnauskas, K.B., Munneke, P.K., Meijgaard, E. Van, Broeke, M.R. Van Den, 2015. Divergent trajectories of Antarctic surface melt under two twenty-first-century climate scenarios. doi:10.1038/NGEO2563

We will update the references

541: The Thwaites and Pine Island inland thinning goes away when you difference the two scenarios, and there is in fact less thinning in the SSP5 scenario. Please discuss this. Having a control run for context (previous comment) would likely help here.

This is consistent with the results of the paper. The dynamic thinning in the Amundsen region is the same in both projections. We don't place much faith in this, because the ice shelves are not well resolved. SSP5 has a greater SMB than SSP1, hence 'less thinning'. We will clarify this in the text. SSP1 is effectively the control run.

Figure 14: Typo in 'cumulative' in title above panel b.

We will reword this

574: Consider rewording this sentence to avoid the possible interpretation that the thinning of Ross Ice Shelf has a direct impact on MAF.

We will reword this

580: This goes back to my earlier comment about what Thwaites and Pine Island are doing in the control run.

We will reword this

Figure 15: Consider using the same colour scheme for the two scenarios here as in the previous figure.

We will change this

642. 655-8: Maybe GMD/JAMES is a better fit?

The scientific conclusions of our paper are a good fit to The Cryosphere. We are primarily interested in differences between SSP1 and SSP5.

Section 4.2: A short comparison of the results to those of ISMIP6-AIS is warranted, as that set of experiments is perhaps the closest point of reference to this work. In addition to considering the overall behavior of each region, it would be interesting to look at the threshold for surface hydrofracture employed by ISMIP6 and if/when that is passed in your simulations. Similarly, comparing your simulated basal melt rates to the parameterization they employ might help explain differences in response.

ISMIP6: Seroussi, H., Nowicki, S., Payne, A.J., Goelzer, H., Lipscomb, W.H., Abe-Ouchi, A., Agosta, C., Albrecht, T., Asay-Davis, X., Barthel, A., Calov, R., Cullather, R., Dumas, C., Galton-Fenzi, B.K., Gladstone, R., Golledge, N.R., Gregory, J.M., Greve, R., Hattermann, T., Hoffman, M.J., Humbert, A., Huybrechts, P., Jourdain, N.C., Kleiner, T., Larour, E., Leguy, G.R., Lowry, D.P., Little, C.M., Morlighem, M., Pattyn, F., Pelle, T., Price, S.F., Quiquet, A., Reese, R., Schlegel, N.-J., Shepherd, A., Simon, E., Smith, R.S., Straneo, F., Sun, S., Trusel, L.D., Van Breedam, J., van de Wal, R.S.W., Winkelmann, R., Zhao, C., Zhang, T., Zwinger, T., 2020. ISMIP6 Antarctica: a multi-model ensemble of the Antarctic ice sheet evolution over the 21st century. Cryosph. 14, 3033–3070. doi:10.5194/tc-14-3033-2020

We compare our results to ISMIP6 on lines 681-682 and 742-746 and will expand this text.

705-708: This is a very speculative statement. The water in warm cavities can certainly get warmer, as it is modified CDW and not unadulterated CDW. Please remove or rephrase this statement with supporting information.

We will rephrase this

Section 4.3: A major limitation not mentioned is the lack of iceberg calving and dynamic calving front position. Other missing physical processes that might be important are subglacial hydrology/basal physics and the impact on ice rheology of fractures and damage.

We will describe these limitations and our plan to study them in future work.

Section 5: The conclusion would benefit from an additional couple sentences about the technical achievements and limitations of the model.

We will edit the conclusion sections to cover the suggestions of both reviewers.