Review 1 of Antarctic sensitivity to oceanic melting parameterizations

This paper uses the recently produced extended ISMIP6 Antarctica oceanic and atmospheric forcings which now extend to year 2300 (as opposed to year 2100 previously) and investigate the impact of these forcing on Antarctica using the YELMO ice sheet model. In their study, the authors not only consider the impact of the model when forced with both the oceanic and atmospheric forcings but also when forced by only either of them. They concluded that oceanic forcings greatly affect the West Antarctic Ice Sheet (WAIS) and not so much the East Antarctic Ice Sheet (EAIS) and vice versa for the atmospheric forcing.

This paper gives more insights on the capability of the YELMO ice sheet model in performing future projection experiments. While analyses on the impact on the ice sheet in separating the oceanic and atmospheric forcing from the ISMIP6 is not well published, previous work done on oceanic forcing impact on the AIS that could have been discussed in greater length in this manuscript.

In addition, the discussion of the initialization strategy and its impact on the simulation deserve to be expanded on many levels from its details and result demonstrations to the impact on the forcing interpretations.

Very similar work of this nature has been done before by Lipscomb et al. (2021) and Berdahl et al. (2023) and the authors did not contrast or conform their work with theirs (I will be specific on this matter in my comments afterwards). Also, I believe the authors missed a few opportunities in this manuscript: they could have added more analyzes focusing more on the impact of heat at the grounding line compared to under the ice shelves (which they hinted at in Fig1 but never really pinpointed thereafter); they could have compared their results to model running at higher resolution and making the point that a 16km-resolution model performs relatively well in these types of experiments compared to model using higher resolutions. Regarding the latter point, they could have analyzed the impact of the first 100 year forcing on their model and compare with the results from Seroussi et al. (2020) since they did not participate in this community experiments.

Finally, this manuscript uses forcings that were created for a community effort, the ISMIP6 AIS-2300 which is an extension of the original ISMIP6 that only provided forcing and simulations until year 2100. To the best of my knowledge, the extended dataset is not yet publicly available and should be used with restrain. While the wiki page for the extended experiment is available for now, it only describes the extended protocol but does not provide a link to the data. I do not see reference credited for these forcing. The manuscript of the extended community experiments was not submitted at the time of this article's submission and was, in fact, just submitted recently. In the absence of citation, and as a courtesy and recognition of the work into generating the dataset, the authors should reach out to the core ISMIP6 organizing group on how to best cite or acknowledge the extended dataset. Please, be mindful about these types of details in the future. For now, please refer to Nowicki et al. (2020), Jourdain et al. (2020), Barthel et al. (2020) (since these papers show the forcing to 2100 and the same scripts were

used to prepare the data) and Seroussi et al. (2024) (under review). For further information on ISMIP6 AIS dataset, I would refer the co-authors to the ghub page: https://theghub.org/groups/ismip6/wiki/MainPage/ISMIP6Projections2300Antarctica and the section "how to obtain the 2300 AIS ISMIP6 dataset" for more information.

 I would support the publication of this manuscript after major revisions.

Thank you for your comments and feedback. We will ensure that the new version of the manuscript takes into account all of your comments and suggestions. In particular, we have contrasted our work in larger depth with that of Berdahl et al (2023), we have deepened our analysis of the impact of basal melting at the grounding line and we have reached out to the ISMIP6 organisers to properly manage citation of the community dataset, which was indeed a grave oversight on our side. They have told us that the best way to proceed is citing Nowicki et al., 2020, Jourdain et al. 2020 and Seroussi et al. 2020, together with the new submitted paper for the 2300 forcings and results, Seroussi et al. sub. They also commented that we have to be aware that in the next months the status of this last paper could change and in that case it would be necessary to change the citation if published by that time (Seroussi, pers. comm.). In addition, they have pointed out to acknowledge the projects CliC, CMIP5/CMIP6 and ISMIP, and we have done so.

In the text below, we respond to your specific points and we provide new figures for the comparisons you mention and redo the current figures for the preprint. Note that our comments are in blue.

Specific comments

As mentioned above, a lot of this work feels like an extension of Lipscomb et al. (2021) and Berdahl et al. (2023) who carried out similar experimental setup and parameter sensitivity studies using a very similar initialization procedure. Using the original ISMIP6 oceanic forcing only dataset Lipscomb et al. (2021) not only showed the accelerated retreat behavior before year 2100 also shown in this study, they also show very similar patterns of retreat. Since they are using all the ocean melt calibrations from ISMIP6, please discuss your results with respect to theirs as they also ran to year 2500, like you. These additions can be done in the discussion sections and in the introduction (lines 75-84).

In the new version of the manuscript we will include a detailed comparison with the studies of Lipscomb et al. (2021) and Berdahl et al. (2023) in the introduction and discussion sections focusing on the similarities but also the differences in our experimental setup.

Specifically, Lipscomb et al. (2021) considered a very similar procedure to ours (DIVA solver, basal friction law and optimization for thermal forcing, mainly). The main difference consists in the procedure used to extend the simulations until 2500: they proceed in a similar way as Berdhal et al. (2023), but using the ISMIP6-2100 dataset and with a repeated forcing using the two-decadal range 2081-2100 in order to account for climate variability. Instead, we fixed the forcing at 2300. As a consequence,

their experiments, with the same MeanAnt calibration as ours, contribute just over 400 mm SLE by year 2500, which is a much lower contribution than in our cases (more than 3000 mm SLE in two experiments). Even using the PIGL calibration, which sets a γ_0 value one order of magnitude larger than the MeanAnt, they do not simulate a very high contribution, reaching roughly 1 m SLE by 2500. Therefore, our results indicate that the repeated forcing used in Lipscomb et al. (2021) leads to an underestimation of the AIS response. Table A1 shows the mean thermal forcing under the ice shelves (between 210 and 810 m of depth) for the ISMIP6-2300 dataset in several periods. Clearly the thermal forcing increases with time, so our setup allows for stronger thermal forcings that using the repeated forcing as in Lipscomb et al. (2021). For instance, for the UKESM1-0-LL case, the differences between our chosen period to extend the forcing and theirs can reach more than 4.5ºC. The same happens for CESM2-WACCM and HadGEM2-ES, reaching more than 3.5ºC, and CCSM4 with roughly 2.3ºC.

Although our final amplitudes of the sea-level response differ, both studies agree in showing an initial slow increment (see Figure A7 on a comment below) and produce a significant loss of ice by the end of the 21st century.

For the new version of the article, we will incorporate this comparison in the discussion section, including Table A1.

Reference | Time range/ GCM Model	CCSM4	HadGEM2-ES	CESM2-WACCM	UKESM1-0-LL	UKESM1-0-LL (low)
Lipscomb et al., (2021) | 2081-2100	1.063	1.347	1.309	0.778	0.206
Berdhal et al. (2023) | 2090-2100	1.132	1.441	1.453	0.917	0.183
This study | 2291-2300	3.397	5.048	4.863	5.494	0.253

Table A1. Comparison of the mean thermal forcing (ºC) from the ISMIP6-2300 dataset on the ice shelves between depth layers of 210 and 810 metres depth, for the different time ranges used to extend the simulations (2081-2100, 2090-2100 an 2291-2300) in Lipscomb et al. (2021), Berdhal et al. (2023) and this work, respectively. Values relative to the reference given by Jourdain et al. (2020).

Berdahl et al. (2023) extended Lipscomb et al. (2021) by doing a sensitivity study on thermal forcing (TF) and basal friction parameters. The thermal forcing parameter they varied in their study was \gamma0, just like you and they concluded that not only ocean thermal forcing played a more important role than basal friction law in future forcing simulations, but that increasing gamm0 led to more melting and grounding-line retreat, just like what you find. Please discuss your finding in comparison to theirs.

We have included a comparison of our results with Berdahl et al (2023) in the new version. They made a similar study to ours but not only varying γ_0 but also the parameter p from Leguy's parametrization, which in our case is constant (p=0.5) in all experiments. In addition, except for CESM2, the GCMs they use as forcings are different from our case. For the experiment with CESM2, they found a sea-level rise of approximately 1600 mm SLE by 2500 while in our simulations, it reached more than 3000 mm SLE. This is explained by the same features pointed out above in the comparison against Lipscomb et al. 2021: while we extended our simulations to 2500 by fixing the forcing to 2300, they considered an average of the 2090-2100 period. This could lead to an under-estimation compared to our case. Again, in Table A1, the differences for the ISMIP6-2300 dataset on the ice shelves are shown to compare their mean values in their period (2090-2100) with ours. As a result, as in Lipscomb et al. (2021), their thermal forcing is more than 3 ºC below ours for some experiments.

Regarding the sensitivity to γ_0, we reach similar conclusions. When increasing the value of γ_0, the retreat in the grounding line (especially in the WAIS) is triggered sooner. However, as we will discuss later, our initial ice-sheet configuration produces larger ice shelves compared to observations. The RMSE of the initial state with respect to observations, is about 200 m/yr and 200 m for ice surface velocity and ice thickness, respectively. Berdhal et al. (2023) obtained lower RMSE values, ~129 m/yr and ~58 m respectively (for the case they show with p=0). Nevertheless, our simulations were also performed with a lower resolution (16 km) than in their work with CISM (4 km). We believe the sensitivity results should still be valid in this case, while the specific numbers could be more uncertain.

In the revised text, we will add a comparison with Berdhal et al. (2023) for the discussion part of the text and incorporate Table A1 as the main source for contrasting the differences that could arise between their work and ours.

Your discussion on the initialization deserves additional details:

Thank you for pointing this out. We will add the following details and a more complete description to the revised text:

1. What do you use for calving?

   We have used the von Mises calving law (Lipscomb et al. 2019; Morlighem et al 2016) with the scaling calving parameter k_t=0.0025 m/(yr Pa) and w_2=25 for the calving eigenvalue weighting coefficient, as in the approach by Lipscomb et al. (2019). The calving limit in ice thickness is set to 200 m.

2. What data are you using for Surface Mass Balance (SMB) and air temperature?

   For Surface Mass Balance (SMB) and 2m-air temperature we have used monthly values from the Regional Atmospheric Climate Model (RACMO v2.3) together forced by ERA-Interim reanalysis mean climatology dataset from 1981-2010. We will include this in the updated manuscript version.

3. What do you use for your geothermal forcing?

We use Shapiro and Ritzwoller (2004), just as Berdhal et al. (2023) with CISM. We will include this in the updated manuscript version.

4. What data are you using for comparing your ice velocity results?

For ice thickness observations, we use Morlighem et al. (2020) and for ice surface velocities, Rignot et al. (2011). We will include this in the updated manuscript version.

5. In many places you are referring to Bedmachine. If the climatic and ice velocity dataset you are using are similar than the ones used in Bedmachine, please cite them separately as they were not produced by Bedmachine!

Yes, we forgot to refer to Rignot et al. (2011) for ice surface velocities and will make sure to do so. We will include this in the updated manuscript version.

6. How does varying \gamma0 influence the inverted parameters?

Figures A1 and A2 show the thermal forcing and bed friction coefficient fields that are obtained from the optimization process. All γ_0 values produce extended ice shelves at the start. The higher the value of γ_0 is, the smaller the ice shelves, and therefore the closer to observations in terms of the extended area and RMSE (lower values). The reason for this is that we do not artificially limit the extent of our ice sheets as other groups do. This yields large ice shelves as compared to observations and to other groups.

As a consequence, lower values of γ_0 lead to higher positive values of thermal forcing correction. To illustrate this issue the mean value over each drainage basin is indicated, showing a decrease with increasing γ_0. For instance, for γ_0=9620 m/yr the west of the shelf illustrates the emergence of a positive anomaly, while for γ_0=14500 and γ_0=21000 m/yr, the thermal forcing correction is negative (Fig. A1).

The optimised bed friction coefficient (Fig. A2) does not strongly depend on γ_0 in the EAIS. This is to be expected because the contact between the ice-sheet and ocean is limited in this area. However, the opposite occurs in the WAIS. For the highest γ_0 value, the basal friction coefficient over a wide part of WAIS (particularly around the divide) saturates at a constant value of 1, the imposed internal upper limit in Yelmo (lower limit is 0.001). This increases basal stress in that region, favouring shear-dominated flow and limiting the basal sliding

We will include this discussion in the updated manuscript version.
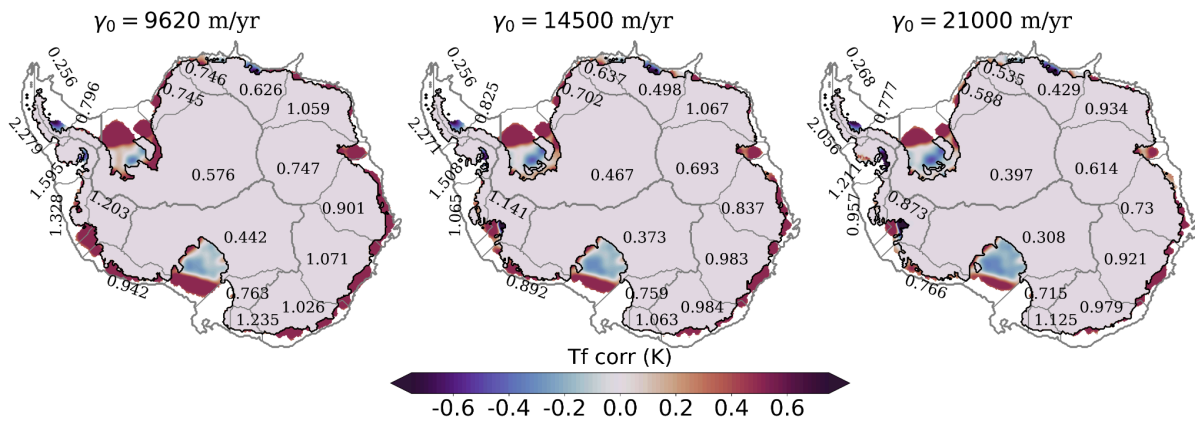
Figure A1. Thermal forcing correction (K) at the beginning of the control simulation for the three values of γ_0. In grey, basins in which Antarctica is divided and numerical values with the average thermal forcing on each basin at start.
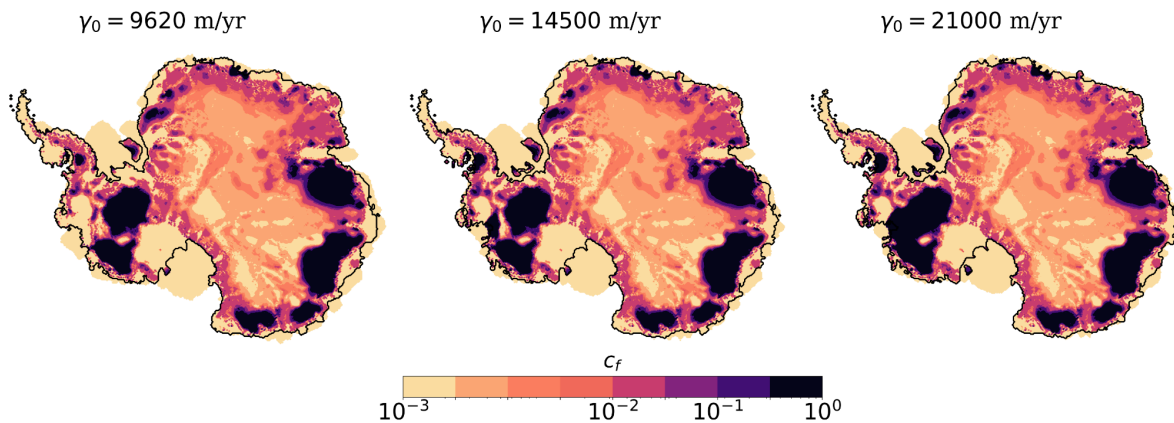

Figure A2. Bed friction coefficient (parameter optimised during the spin-up) at the beginning of the simulations. The minimum value is 0.001 set as a limiting parameter in Yelmo.

7. You mention that your spin-up last 20 kyrs, during which the first 15 kyrs are used to invert for observation. What do you do for the remaining 5kyrs?

During the first 15 kyr, the basal friction and thermal forcing fields are optimized. During the last 5 kyr, the optimised fields are held constant and the spin-up is run with the reference climatology towards steady state. We will make this point clear in the revised text.

Please show a map of \deltaT_sector resulting from your inversion process. This map can contain the values for all 3 \gamma0. Please discuss in more length the implication of this coefficient. In particular, if you need to cool down your basin by several degrees to obtain a stable ice sheet state, it means (at least one

interpretation could be) that the thermal forcing used in the inversion are too warm to obtain a steady state with current observations and the melting of ice shelves is underway.

Figure A1 shows the thermal forcing correction over the shelves with its mean average value over the different sectors of the AIS indicated, just as in Fig. 5 from Jourdain et al. (2020), at the beginning of the simulations (end of the spin-up process), for the three different values of γ_0 used. The Ross, Ronne-Filchner and Amery ice shelves show very low mean values for all γ_0 values. We do not need to cool down our basin to obtain a stable ice sheet. If anything, the thermal forcing correction is positive and can be higher than 1ºC in areas in which ice shelves extend beyond observations. This is due to the fact that we do not artificially limit the extension of our ice shelves. In order to deepen our understanding of this issue, we have done additional simulations limiting the ice-shelf extension, as is done in other studies. When limiting the ice shelves to the present-day observed extension the thermal forcing correction on the basins decreases. We will include this discussion in the new version of the article.
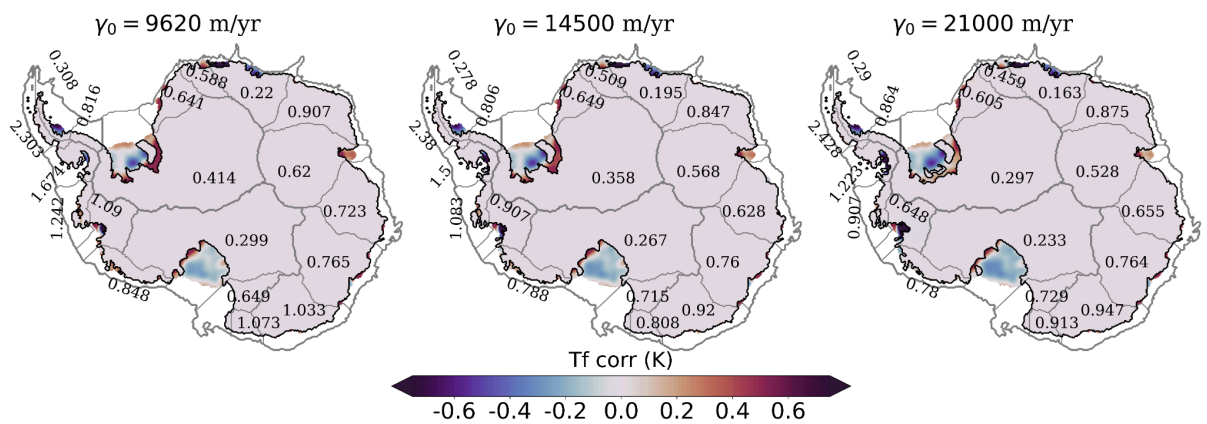

Figure A1*. Same as Figure A1 but with limited-ice-shelves.

8. As you are separating WAIS and EAIS in your analysis, and you are not showing 2-d representation of the forcings, I would suggest you add a figure which separates the averaged time series of your forcing for these 2 regions (or mention that the mean forcing evolutions are very similar in both regions).

We have modified Figure 1 to include this (see Figures section below). Indeed, the difference between the two regions is small, generally well below 1 ºC.

9. It is highly unlikely that the thermal forcing provided by the dataset will always coincide with an ice draft point in a cavity. Please describe your interpolation/extrapolation process in this case.

In order to interpolate the values at different depths, for oceanic fields like salinity and temperature (and therefore thermal forcing), Yelmo has a *marine-shelf interface*. Firstly, it computes the depth of the ice shelf base by distinguishing

between floating $\left(\text{depth}_{\text{shelf}} = H \cdot \dfrac{\rho_i}{\rho_{sw}} \approx 0.89 \cdot H \right)$ and grounded ice $(\text{depth}_{\text{shelf}} = z_{sl} - z_{bed})$ where H represents the ice thickness, ρ_i and ρ_{sw} the densities of ice and sea water respectively, and z_{sl} and z_{bed} the height of sea level and bedrock. Secondly, the weights for the different vertical layers are calculated by taking into account the two nearest layers of the depth of the shelf previously calculated in the first step. Finally, the oceanic fields are calculated in terms of these weights as a weighted sum.

In the updated manuscript we will ensure that this description appears in an Appendix section.

10. In Figure 4, please add the grounding line location on your plot. Also, since you are focusing on WAIS in your results with ocean TF, I would appreciate a closer look at the WAIS which better shows your grounding line locations and your fit to observation. Currently, it is difficult to distinguish.

We have added the grounding-line location and the figure suggested with a close-up into the WAIS (see the Figures section below). We will incorporate the modified figure plot in the new version of the manuscript.

11. Your inversion procedure results in a lot more ice shelves than currently observed. Please discuss the impact of this feature on your results. For example, in the Amundsen Sea Embayment (ASE) your model predicts an ice shelf that could provide more buttressing compared to current observations.

Simulating the correct ice extent of ice shelves is a challenging task. Some ice models impose fixed ice-shelf extensions or prevent ice-shelves to grow beyond the observed ice front (Seroussi et al., 2019). In addition, tuning parameters to simulate realistic ice-shelf extensions may differ between different Antarctic embayments (Wilner et al., 2023). In effect, we allow ice shelves to extend further than observed ice-shelf fronts, which is especially noticeable for the Ronne-Filchner and the Ross ice shelves.

For the ASE, indeed the ice shelf covers all the area, advancing various kilometres into the ocean and that results in a deceleration of some ice streams flowing towards that sector at the beginning of the simulations (see Fig. 4 for a comparison with observations). As mentioned before, higher values of γ_0 results in a more realistic set up at the beginning (which can be observed in terms of RMSE, Fig. 3), despite having a greater rate of melting.

We have done additional simulations considering limited ice shelves with the mask from observations in the BedMachine dataset. In terms of sea-level contribution, results and spreads are very similar for higher-emission scenarios (Fig. A3) with differences ranging between 0.4 and 23.5% in absolute value, but generally being less than 10%. In the case of the low-emission simulation this

difference is much greater. In general we can say that our extended simulations have a considerable effect on sea level, but less than 141 mm SLE at 2500.

	CCSM4	HadGEM2-ES	CESM2-WACCM	UKESM1-0-LL	UKESM1-0-LL (low)
γ_0=9620 m/yr	23.5	-10	0.4	1.2	38
γ_0=14500 m/yr	13.5	4.3	1.9	4.7	-48
γ_0=21000 m/yr	7.9	3.1	0.9	2.7	-4.9

Table A2. Differences (in percentage) in sea level contribution at 2500 for the experiments carried out, between the runs with extended ice shelves and the ones with limited ice shelves.
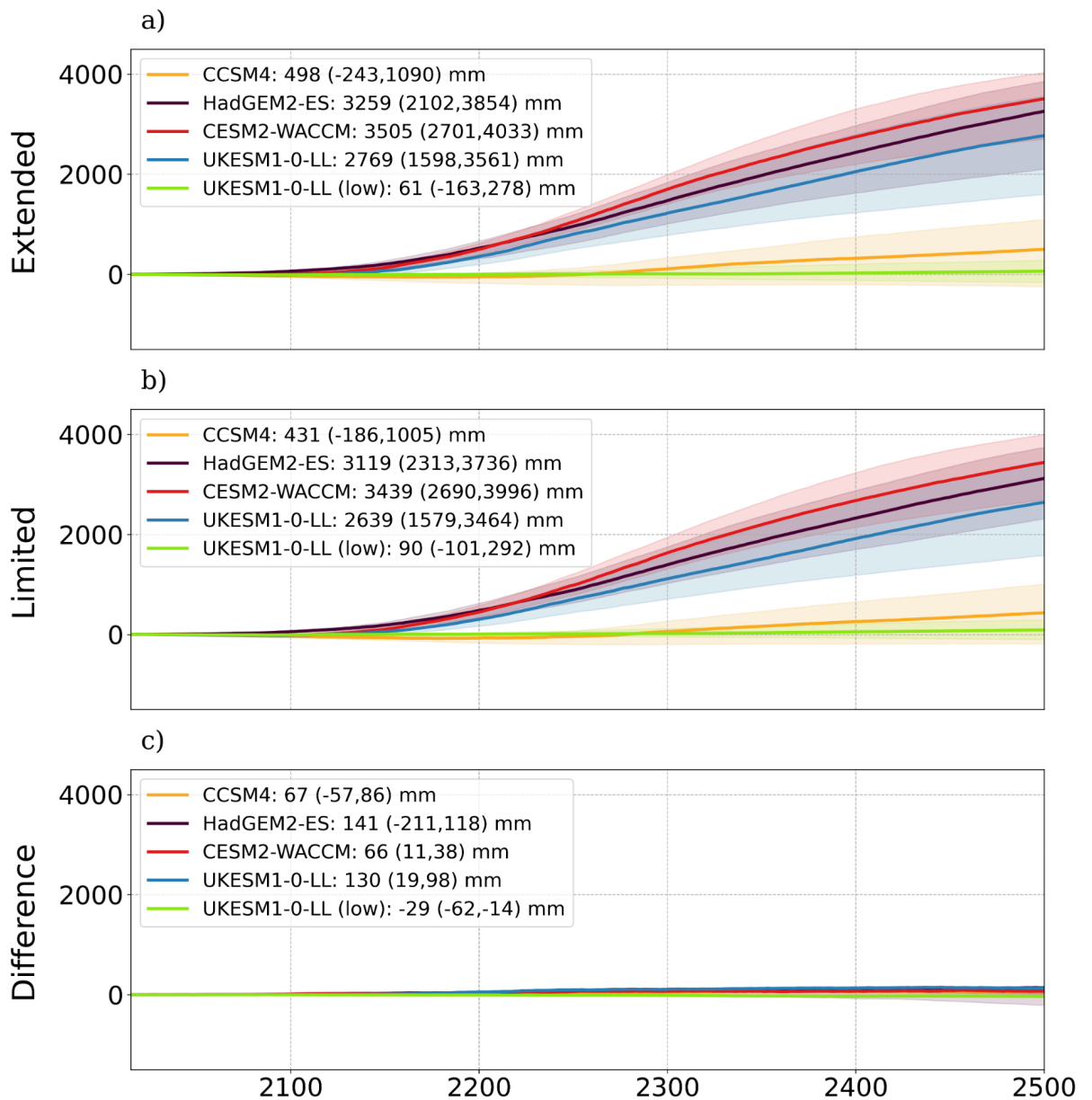
Figure A3. Evolution of the sea level contribution until 2500 for the different experiments carried out, together with the spread produced by the different values of γ_0. In a) with the extended ice shelves, in b) with the ice shelves limited by a mask from observations in the BedMachine dataset and in c) difference between a) and b).

We plan to include a more profound discussion of this matter in the Discussion section, and show this figure on the Appendix.

12. In your text, you refer to specific location with TF warming (e.g., ASE and Bellingshausen line 226) but point to an averaged time series as "proof". For this reason, I would suggest adding a figure showing this TF in 2015 and 2300. For

practicality, you could show the depth averaged TF between depth raging 200 m to 800 m, say (where the groundling line is located mainly).

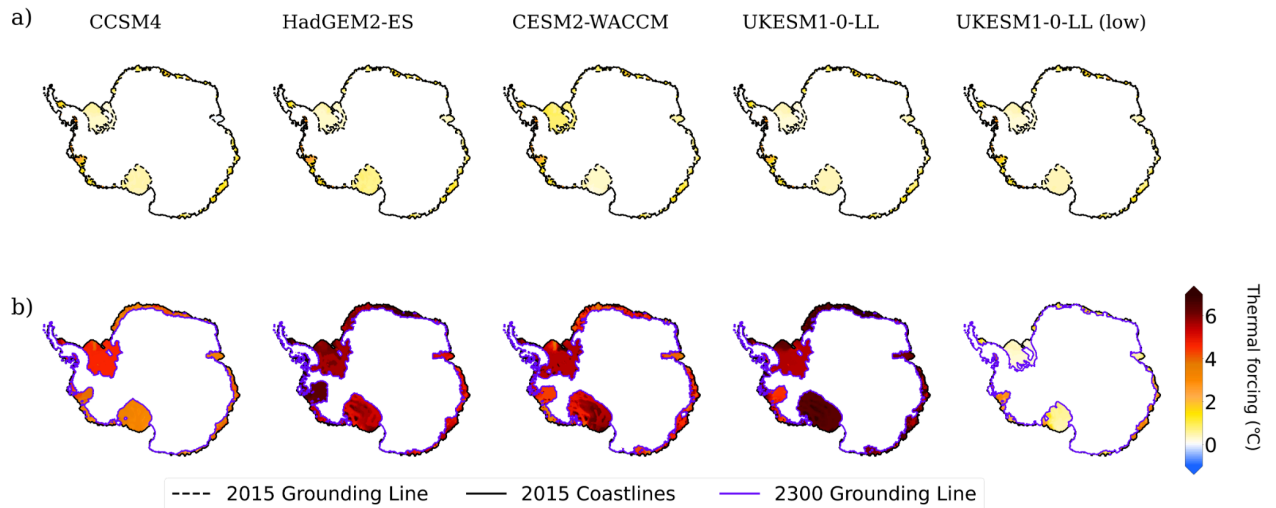In the revised manuscript , we will add the following new figure in the Appendix (Fig. A4).

a) CCSM4   HadGEM2-ES   CESM2-WACCM   UKESM1-0-LL   UKESM1-0-LL (low)

b)

---- 2015 Grounding Line   —— 2015 Coastlines   —— 2300 Grounding Line

Figure A4. Depth-averaged thermal forcing under the ice shelves (ºC) between 210 and 810 metres in (a) 2015 and in (b) 2300.

13. Please show in one of your figure or mention in your text the behavior of your control run over the length of your experiment. You mention that you are running one, but do not discuss its evolution (even one sentence could suffice).

The control runs with the different values of γ_0 are all stable during the time of simulation, with variations rounding 5 mm of SLE between 2015 and 2500 (Fig. A5) in terms of volume above floatation (m SLE).

With increasing values of γ_0, the ice sheet has less ice volume at its initial state, but nevertheless variations are negligible ranging between 10-15 cm SLE (Fig A5).
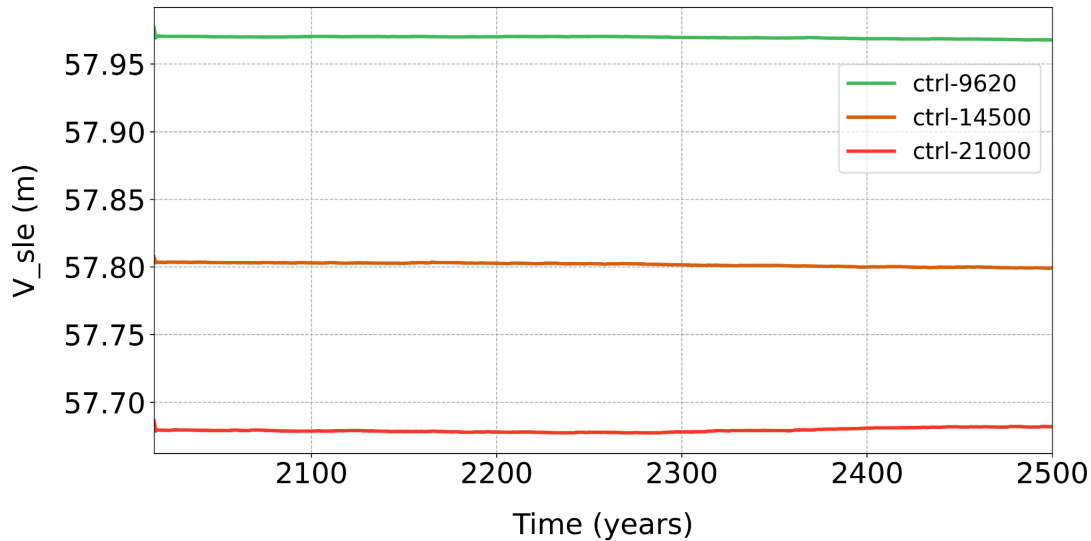
Figure A5. Evolution of the volume of sea level equivalent (m SLE) in the control runs for the AIS with the different values of γ_0.

In Fig. 1, you go through the trouble of differentiating the TF at the grounding line from the one in cavities. While for most model the TF at the grounding line is always warmer compared to in the cavity by a fraction of a degree, the TF from CESM2-WACCM is warmer by a couple of degrees in the cavities. Could you infer any importance on this behavioural difference from your results?

There may be some confusion here. We are not differentiating between TF at the grounding line from TF in the cavities. We are making a comparison at the grounding line for panels b and c in Figure 1. The difference comes when calculating the curves. For Fig. 1b, we take the data from ISMIP6 masking on the grounding line of 2015 (which is fixed). For Figure 1c, we consider Yelmo outputs of TF on the ice shelves and let the mask evolve while evolving the grounding line (retreating more precisely). So that is what leads to a difference, while for Fig. 1b, the grounding line is fixed for all models, in Fig. 1c, it evolves differently in each case. For CESM2-WACCM, it is especially noticeable because it retreats more towards the interior.

In my (limited) experience about the use of YELMO in publication, it is more frequently used in paleoclimate studies as opposed to century forecasting ones. In this study YELMO is run at a resolution of 16km which is a resolution many people would argue is too coarse to help reduce uncertainties in future projections. If you can, I would suggest you try to make the point in arguing that your results are encouraging at that resolution. I think you started hinting at this with your table 4. I would also suggest adding the results from Lipscomb et al. (2023) to this table and add the resolution of each model for their study. Also, as hinted above, you could succinctly (or not) write a section about your results regarding the ISMIP6 experiment until 2100 and compare YELMO to the output from Seroussi et al. (2020). This would be of limited extra work since these would be a subset of your current runs.

Indeed, Yelmo has often been used for paleo-simulations with timescales of several thousand years (Blasco et al. 2021, Moreno-Parada et al. 2022). Concerning horizontal resolution, for this work we first ran all simulations with a 32 km resolution. This was very informative as to what to expect from the model, and then we moved forward with 16 km resolution simulations, which helped in better simulating the present-day ice sheet with Yelmo.

At 32 km, results for sea level evolution are very similar, varying 7-10% for high-emission scenarios (Fig. A6). Generally, they yield an increase in the contribution but a decrease in the spread between the low and high values of γ_0 (Table A3). Lipscomb et al. (2021) made the point with 2 and 4 km resolution with sea level variations of ~15% and with 4 and 8 km with ~20%, but they find that the higher the resolution, the higher the sea-level contribution. In our case, it is the contrary, at least at the resolutions we tested. We will add results from Lipscomb et al. (2021) to Table 4 in our manuscript (see section Tables below).

	CCSM4	HadGEM2-ES	CESM2-WACCM	UKESM1-0-LL	UKESM1-0-LL (low)
16 km	1333	1752	1332	1963	441
32 km	1199	1458	1357	1810	356

Table A3. Comparison with different resolutions of the spread (mm SLE) at 2500, between the low and high value of γ_0 for the experiments carried out.
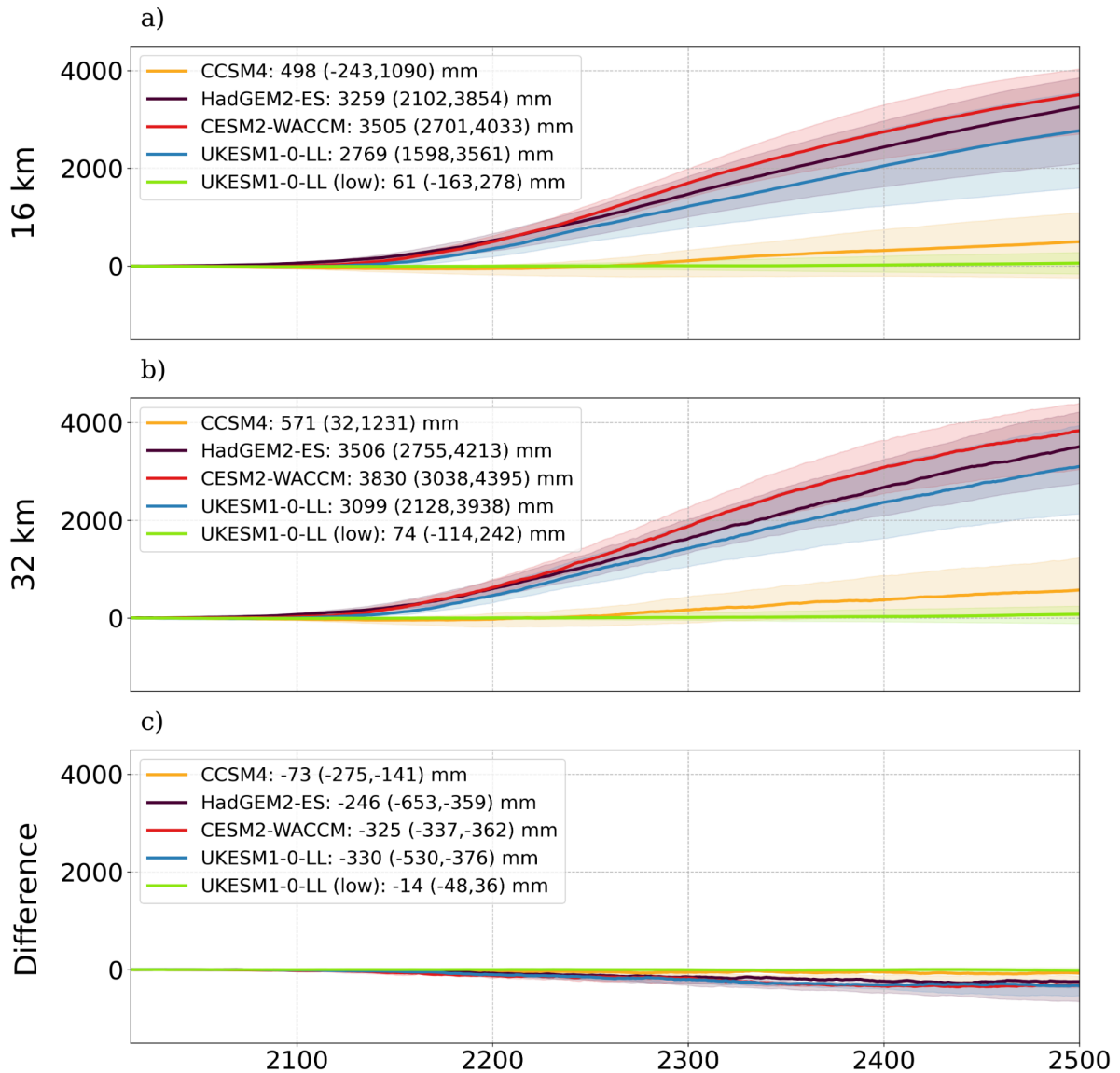
Sea Level Contribution (mm SLE)

a)

CCSM4: 498 (-243,1090) mm	
HadGEM2-ES: 3259 (2102,3854) mm	
CESM2-WACCM: 3505 (2701,4033) mm	
UKESM1-0-LL: 2769 (1598,3561) mm	
UKESM1-0-LL (low): 61 (-163,278) mm	

b)

CCSM4: 571 (32,1231) mm	
HadGEM2-ES: 3506 (2755,4213) mm	
CESM2-WACCM: 3830 (3038,4395) mm	
UKESM1-0-LL: 3099 (2128,3938) mm	
UKESM1-0-LL (low): 74 (-114,242) mm	

c)

CCSM4: -73 (-275,-141) mm	
HadGEM2-ES: -246 (-653,-359) mm	
CESM2-WACCM: -325 (-337,-362) mm	
UKESM1-0-LL: -330 (-530,-376) mm	
UKESM1-0-LL (low): -14 (-48,36) mm	

Figure A6. Sea level contribution for the different experiments carried out and spread of the values of γ_0 using resolutions of 16 km (a) and 32 km (b). In c, differences between a and b.

Sutter et al. (2023) argued using PISM with 16, 8 and 4 km resolution grids that their 8 km ice-sheet configuration is close to their 16 km runs, nonetheless the rate of retreating at the grounding line is different, starting sooner for 8 and 4 km compared with 16 km, especially observed for Thwaites Glacier. This suggests the MISI instability is triggered earlier when using a higher resolution. Yet, running simulations at higher resolution (8 km or finer) is currently technically infeasible for Yelmo, as we do not have a parallelized procedure and it would be computationally very expensive.

Concerning the comparison with Seroussi et al (2020) until 2100, as shown in Figure 12 from that paper, the spread and mean values produced by the different ice-sheet models ranges between -23 and 35 mm SLE for the medium value of γ_0 (Figure A7). Our results for 2100, are similar but only three GCM models fit within the former spread: CESM2-WACCM and the high and low emission cases with UKESM1-0-LL, with a negative contribution in 2100. HadGEM2-ES exceeds in approximately 25 mm the upper limit from Seroussi et al. (2020) and CCSM4 does not reach the lower limit being 12 mm below it. Regarding the low value, only HadGEM2-ES fits in the range of the shading while all the other forcings are below the lower limit during the whole period (Figure A7a). Finally, with respect to the high value, only HadGEM2-ES diverges from the shading from 2070 and on, while the others are enclosed in most years of the period. A possible explanation for this can be based on the fact that the forcings evolution (Fig. 1) is very small during the first 100 years, expanding from that year and on. Also, we are using two CMIP6 models while in Seroussi et al. (2020), all the forcings come from CMIP5 models.

For the reviewed manuscript, we will include a paragraph to justify our choice of horizontal resolution and include Fig. A7 as Supplementary material to have a comparison with Seroussi et al. (2020) results, showing the ranges where sea level contributions by Yelmo are.
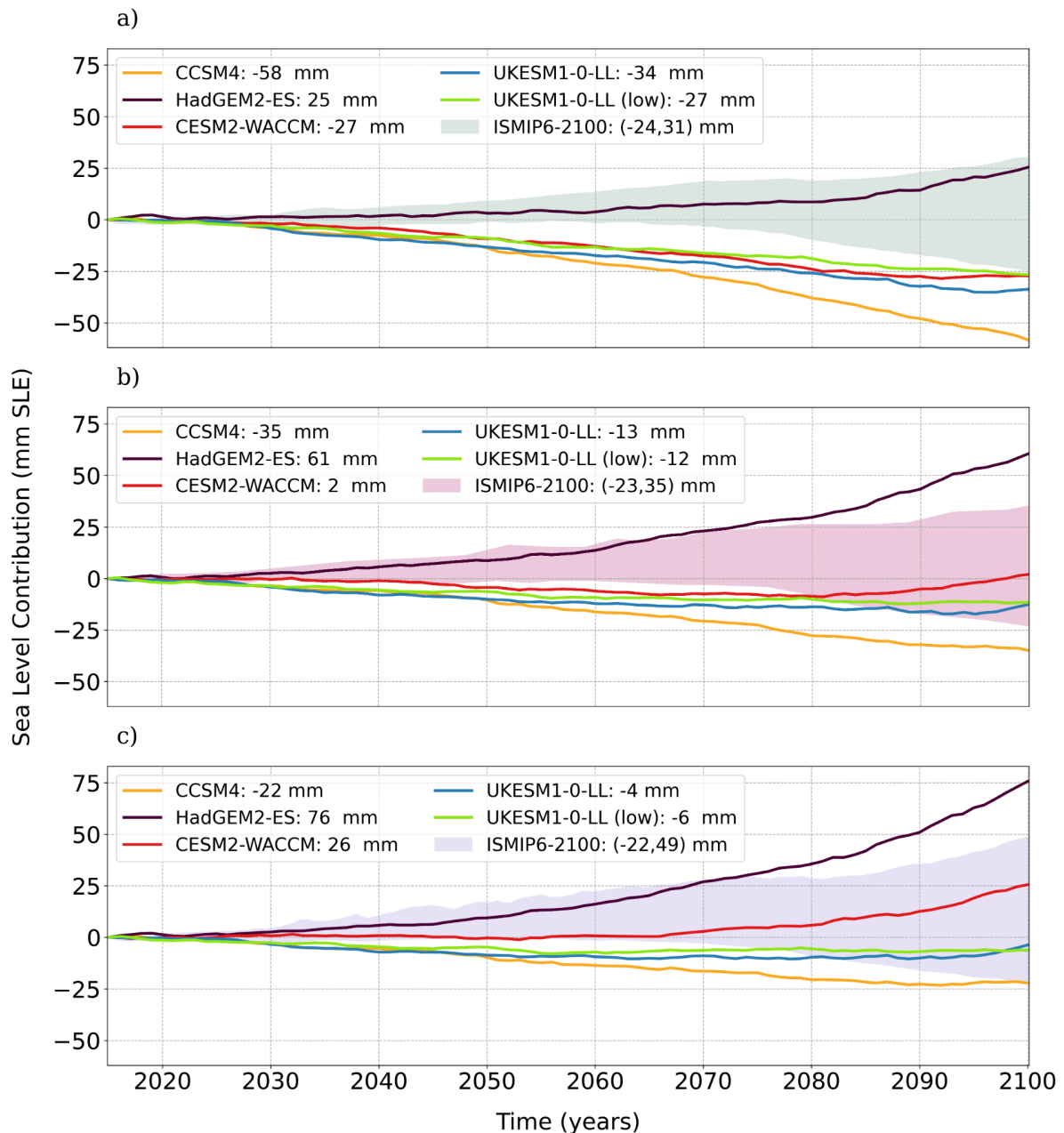
Figure A7. Sea level contribution (mm SLE) for the experiments carried out with five GCM forcings under high-emission scenarios, until the year 2100. In a) the low value of γ_0=9620 m/yr, in b) the medium value γ_0=14500 m/yr and in c) the high value γ_0=21000 m/yr. Solid lines represent the curve for the values of γ_0 and the shading the overall results from Seroussi et al. (2020) with different ice-sheet models and the corresponding value for γ_0.

I am reiterating the significance of \deltaT_sector in your results interpretation. Particularly, when interpreting the results for CCSM4 (e.g., Sect. 4, lines 924-296). (Since I have not seen the values for \deltaT_sector, this comment is somewhat speculative but with a point to be addressed nonetheless). If your inversion leads to a \deltaT_sector=-3 degC in the ASE and the TF for CCSM4 is about 4 degC, your model will actually feel only 1degC there. This means that the slow response of say Thwaites,

is due to your model calibration as opposed to the forcing itself. This is a particular and important point to make to not undermine the importance of a 4 degC TF suggested by CCSM4. Please clarify this point in your discussion. As a parallel, I would refer to Lipscomb et al. (2021) and their 2 degC synthetic experiment in the ASE for that exact reason.

As mentioned above and shown in Fig. A1, this is not the case: thermal forcing corrections are generally below 1.5ºC in most basins of the WAIS and EAIS. With the limited-ice-shelves simulations and repeating the analogue for Figure A1, we can infer a reduction of this warming correction.

The beginning of section 3.1 refers to the initialization procedure's results. I would move it at the end of section 2.3 or create a new section 2.5. In any event it is out of place in a section that is supposed to introduce sensitivity experiments.

In order to make it more clear, we will put the initialization results in a section 2.5 addressing the discussion on this.

Throughout your manuscript you refer to sea level equivalent in your figures. I did not see anything indicating otherwise, therefore I presume that the values were converted solely from the ice mass above floatation. I would like to bring to your attention that this conversion is not as simple (Goelzer et al. (2020)) and I would encourage you to document your conversion to avoid future readers' ambiguity. Along these lines, please add "s.l.e" (sea level equivalent) to "m" when you refer to m of sea level in your units in the text.

Thank you for this important comment. Regarding the conversion, yes, Yelmo returns the variable "V_sle" which calculates the volume of ice above flotation for grounded ice in metres. The conversion is done based on the ice above floatation because as Goelzer et al. (2020) pointed out, on short timescales, this conversion can assume a constant area for the ocean when calculating the SLC. Therefore, in a five-century period the bedrock is not expected to change considerably. In this study Yelmo uses ELRA as GIA model. Seroussi et al. (under review) have demonstrated (pers. comm.) that an ELRA model correction in the bedrock elevation could result in differences of around +11-17% per ~1.3 m SLE calculated with the volume above floatation method. Our results are conservative, but nonetheless we are mainly focused on sensitivities in our work rather than absolute contributions. To make the final computation shown in the evolution sea level curves in the text, we take into account the control run as reference. We will also add SLE to m or mm in the revised text as we have put in our figures.

In the near future, Yelmo will incorporate a more realistic GIA model (FastIsostasy, https://github.com/JanJereczek/FastIsostasy.jl, Swierczek-Jereczek et al., in revision) that could be interesting to test with these ISMIP6-2300 simulations. Anyhow, we will make sure to document the conversion in the new version.

Regarding the figures, I found the spatial figures really hard to read and would highly suggest enhancing them by making them bigger for a start.

We have remade them bigger and with grosser linewidths for coastlines and grounding lines.

Data availability: while the YELMO code is open source, please provide a link to your output and configuration files for reproducibility purposes.

We have created a Zenodo repository with a DOI identification (10.5281/zenodo.10657938) containing a zip file with different folders with the NetCDF Yelmo output files for 1D and 2D variables for each experiment and value of γ_0. We will publish it upon acceptance.

Technical comments

For the technical comments below, we will take special care to address each one for the next version.

Line 52: please cite Edwards et al. (2021) regarding ice-ocean interaction uncertainty. We will do so.

Line 68: remove the http link and web address from the text. We will remove it.

Line 75: please include results from Lipscomb et al. (2021) in your discussion here. We will do so.

Line 89: please cite Nowicki et al. (2020), Jourdain et al. (2020), and Seroussi et al. (2024) for the extended ISMIP6 AIS 2300 dataset. (See main comments above.). Remove the http citation. We have contacted the ISMIP6-2300 team leaders to inform them about our publication and to ask for the proper citation.

Line 109: please cite Goldberg et al. (2011) as a reference to DIVA. We will cite it.

Line 110: The description of the AIS grid is in a weird location in the text as it precedes the reference to idealized experiments. I suggest moving this detail at a less ambiguous place (maybe end of paragraph (see next comment)). We will follow your suggestion and move the description of the grid to the end of the paragraph, also reorganising with your following comment.

Line 114: I would start a new paragraph after "Last Glacial Maximum". We will reorganise these lines.

Line 119: I believe you mean Leguy et al. (2014) (their 2013 manuscript was in the TCD and the 2014 version is the final published version). Yes, we will revise that.

Line 123: here and elsewhere I would suggest using a more active voice instead of passive. I would replace "has been" by "is". Thank you, we will check not only this line but also the rest of the text.

Line 143: remove the reference to the wiki page. We will do it and add a correct citation.

Line 152: "bellow the rest" please be more precise. Thank you for pointing this out, we will rephrase it in the next version of the manuscript.

Line 152-153: "Therefore […]" I find this sentence odd, out of place, erroneous, and not adding any useful information to the manuscript. I suggest you remove it, or you clarify what you are trying to get at. You are trying to compare the output of global climate models (GCMs) that are (commonly) tuned for the pre-industrial time period and then react to a forcing scenario after the historical period. The atmospheric and oceanic forcing used here are a response of the GCM to SSPs and RCPs forcings. When I read your sentence, it almost sound to me that you are trying to say that the atmospheric and oceanic forcings co-evolve independently which is simply not true.

We will change the sentence for another which makes this point more clear.

Line 159: replace "On the shelves" by "in ice shelf cavities" (or "In cavities)". Thank you for the correction.

Line 160: what are the "main" cavities of WAIS and EAIS? Please be more precise. We will be more clear in the new manuscript.

 Line 161: see comment for line 159.

Line 161: "The grounding line […]" I don't understand the point you are trying to make here. Please remove or rephrase. We will rephrase it in the reviewed text version.

Line 164: here and elsewhere, I find it clearer to refer to equations as "(Eq. 3)" compared to "(3)". Thank you, we will change it along the reviewed version.

Line 169: where is the climatology from? Note that Bedmachine is a dataset for bed topography, ice thickness and surface elevation, not for ice velocity, SMB, or air temperature.

For the atmospheric fields, Surface Mass Balance (SMB) and 2m-air temperature, we have used monthly values from the Regional Atmospheric Climate Model (RACMO v2.3) together forced by ERA-Interim reanalysis mean climatology dataset from 1981-2010. In the submitted version there is a mistake on our side when saying that we use a reference climatology average in the period 1995-2014. Regarding the oceanic fields, the reference is Jourdain et al. (2020). We will clarify this in the final version.

Line 170: what happens during the last 5kyrs of the initialization's procedure?

During the first 15 kyr, the basal friction and thermal forcing fields are optimized.   During the last 5 kyr, the optimised fields are held constant and the spin-up is run with the reference climatology towards steady state. We will make this point clear in the revised text. This will be clarified in the revised text.

Line 179: please provide a map for \DeltaT_sector for the 3 \gamma0 parameters.

It is now provided as Figure A1; see the discussion above.

Line 188: I would suggest rewriting this sentence with an active voice: "conducted: atmosphere-only runs in which […] thermal forcing are imposed […] simulation and ocean-only simulations with the surface mass balance and surface temperature […]"

Thank you for the suggestion, we will change to active voice along the text.

Line 189: (regardless of my previous comment) remove "temperature and salinity" since the thermal forcing was derived using these 2 variables. Yes, this will be deleted for the new version.

Line 190: why do you feel the need in mentioning both precipitation and SMB? We will change it in the text.

Beginning of Section 3.1: see main comments about the discussion of initialization.

In order to make it more clear, we will put the initialization results in a section 2.5 addressing the discussion on this.

Line 193: what dataset are you using for ice velocity comparison?

The dataset is Rignot et al. (2011), as mentioned above. This will be properly stated in the revised text.

Line 194: there is no need to keep referring to Bedmachine and its reference. Once is enough. We will correct for this.

Line 198: similarly to above, I would suggest a more active voice and rewrite as: "For the medium value of \gamma0, the model overestimates ice thickness at the ice-sheet margins." Thank you, we will proceed as suggested.

Line 202: please rephrase. We will do it in the next version.

Line 203: replace "the values of" by "varying". We will do it in the next version.

Line 205: 3.5 m of sea level (or s.l.e after defining the acronym somewhere). Yes, we will use an acronym previously defined.

Line 206: replace "but" by "and". We will do it in the next version.

Line 209: replace "over" by "under". We will do it in the next version.

Line 210: I believe you meant to refer to Fig 1.c. We will revise this in the text and clarify it.

Line 211: say more about the difference in temperature and the rate of change in the first century. We will do it and expose it clearly in the new manuscript.

Line 212: remove "Furthermore". We will do it in the next version.

Line 220: the contribution of WAIS and EAIS adds up to 95%. Where are the 5% missing, from the control?

No, it was just approximated numbers from two GCM models (CESM2-WACCM and UKESM1-0-LL) on Table 3. In other words, we just try to summarise Table 3 with some numbers.

Line 220: replace "1 m" by "\approx 1 m s.l.e". Yes, we will use an acronym previously defined.

Line 221: replace "thus […] even" by "being the major contributor". We will do it in the next version.

Line 227: Fig 1 is not a spatial figure and for this reason it does not support your claim for the warmer waters of the ASE and Bellingshausen. Yes, we will consider another figure, like you have proposed before and rephrase this.

Line 236-237: you have not defined which are the main ice shelves of the west (this is ambiguous). Remove "of the main ice shelves of the west". Certainly, we have not. Therefore, we will remove that sentence.

Line 284: replace "not allow" by "not lead". Also, you have not shown spatial patterns of SMB. Maybe it might be worth doing so. We will replace the words you say and consider the showing of SMB.

Line 291: the relationship of \gamma0 on sea level contribution was also shown in Berdahl et al. (2023). Please add this to your discussion. We will ensure to do it in the Discussion section as previously mentioned at the beginning of the reply.

Line 336: update citation and remove the web address. Yes, we will do it.

Tables

Table3: in caption, replace "at the year" by "at year" and "the heat exchange velocity" by "\gamma_0". We will do it in the new revised text.

GCM Model	Chambers et al. (2021) / 8 km		Greve et al. (2022) / 8 km	Lipscomb et al. (2021) / 4 km	Yelmo / 16 km	
	2300	2500	2300	2500	2300	2500
CCSM4	0.5	2.1	1.25	0.28	0.1	0.5
HadGEM2-ES	1.6	3.6	3.3	0.35	1.5	3.2
CESM2	0.45	1.75	0.95	0.26	1.7	3.5
UKESM1-0-LL	1	2.6	1.95	0.38	1.2	2.8

Table 4. Sea-level contribution (m SLE) at 2300 and 2500 among the studies made by Chambers et al. (2021), Greve et al. (2022), Lipscomb et al. (2021) with different resolutions and the results from Yelmo.

Figures

We here provide the new versions of the figures following the referee's suggestions. We will replace them in the text and add some others that appear before on this reply.

Figure 1: In your text, you mention that the EAIS and WAIS will be affected differently by the forcing. It might be worth it to add the mean forcing for EAIS and WAIS separately in this figure.

Also:

1. Panel b: add "at the grounding line" to the y-axis label.
2. Panel c: how did you obtain your thermal forcing, did you filter them out to only account for year 2015 cavities or are these averages over each full basins in your Antarctic grid? Please add some details here.

Yelmo outputs the evolution in time of the thermal forcing over the shelves. To produce Fig. 1c, we simply mask on the grounding lines points which are evolving, opposite to Fig. 1b where the grounding line is fixed in 2015. We will provide a clarification on this issue in the new text.

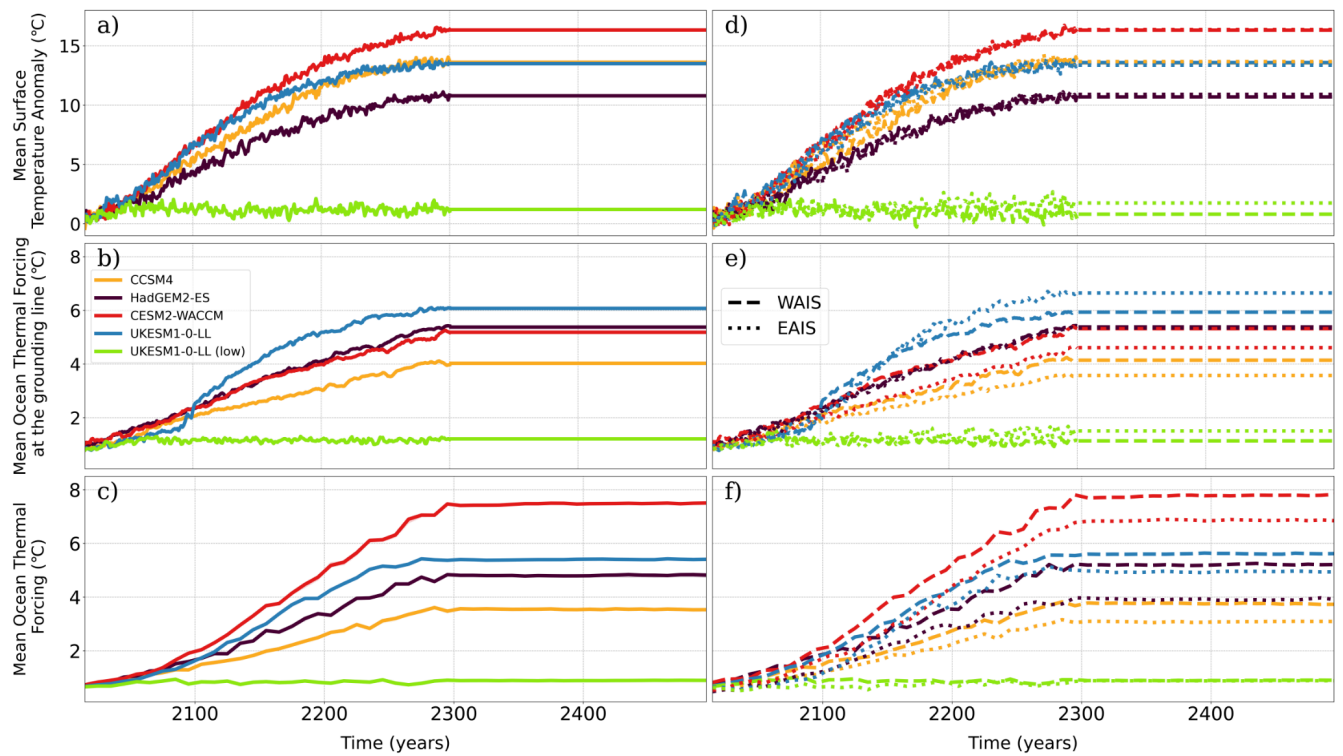3. For each panel and the figure caption, add that these are mean evolutions.

Figure 1. Mean evolution of the atmospheric (a), oceanic thermal forcing at the grounding line depth fixed in 2015 (b) and thermal forcing at the evolving grounding lines in the AIS (c) for the five experiments carried out: CCSM4 (RCP8.5), HadGEM2-ES (RCP8.5), CESM2-WACCM (SSP5-8.5), UKESM1-0-LL (SSP5-8.5) and UKESM1-0-LL (SSP1-2.6). For (d), (e) and (f), same as in (a), (b) and (c) respectively but for the WAIS and EAIS. From 2300 to 2500 a constant climate is imposed through the average climatic conditions in the period 2291-2300.

Figure 2: Please make the figure bigger. Also:

1. Add labels to the colorbars.
2. Add a legend to the figure describing the grounding lines.
3. In figure caption, I believe (b) shows thermal forcing differences.
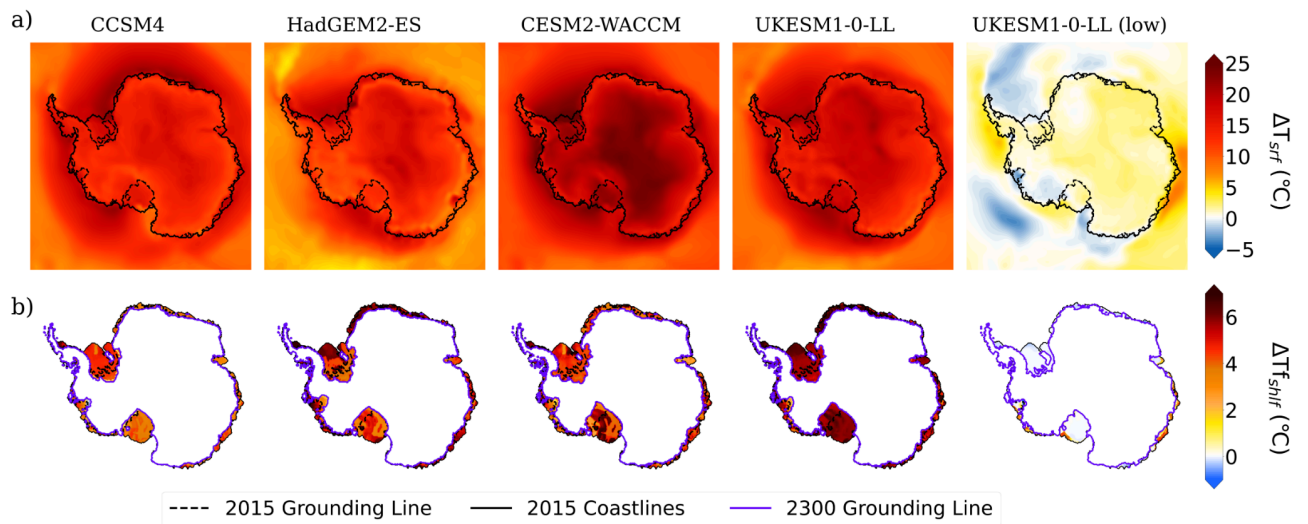
Figure 2. Surface temperature anomaly (a, in ◦ C) between 2300 and the present day as simulated by each of the GCMs selected from ISMIP6-2300 in Table 1, using four high-emission scenarios and a low emission one. Thermal forcing differences over the ice shelves (b, in ◦ C) between 2300 and 2015, together with the grounding lines and coastlines at the initial time and the 2300 grounding line.

Figure 3: I would suggest creating 2 numbers, one with your extended ice shelves, and one that uses the same ice extend as Bedmachine.
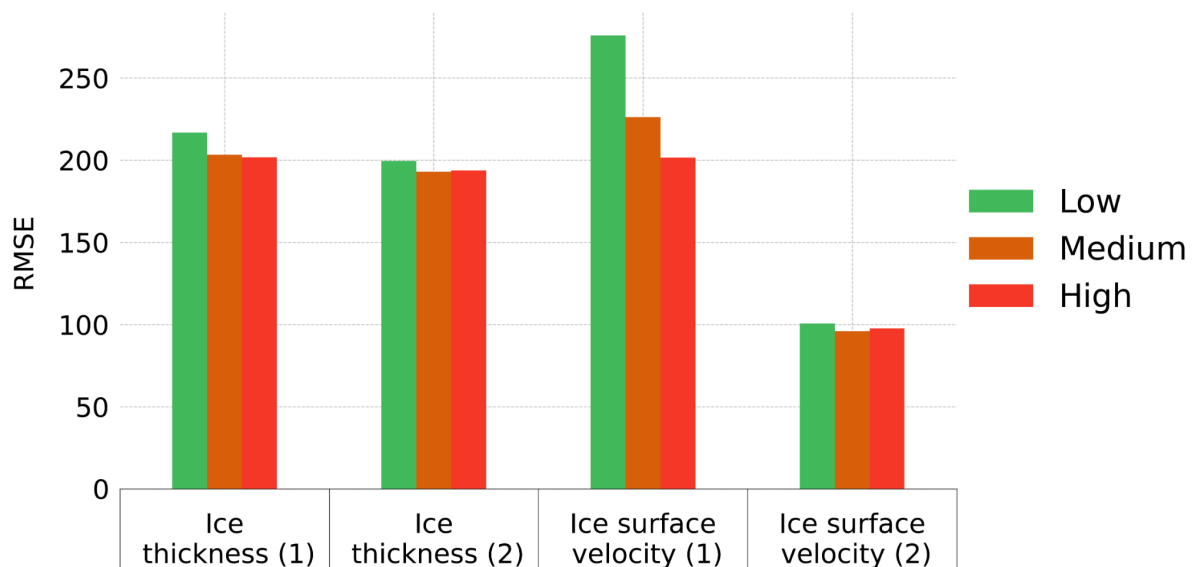
Figure 3. Root mean square error (RMSE) for ice thickness (in m) and ice surface velocity (in m/yr) between the start of the simulations in 2015 and observations. The different colorbars represent the values of γ_0. To mask over the cells containing ice, the fields at the start of the simulation (extended ice shelves) have been used for the columns labelled with (1) and the observations for the ones labelled with (2).

Figure 4:

1. Add titles to the figures.
2. Create a subplot zooming in on the WAIS.
3. Add the grounding line locations for both model and obs.
4. What data did you use for ice velocity? (Note that Bedmachine used a dataset and if you are using the same, you still need to refer to it separately.)
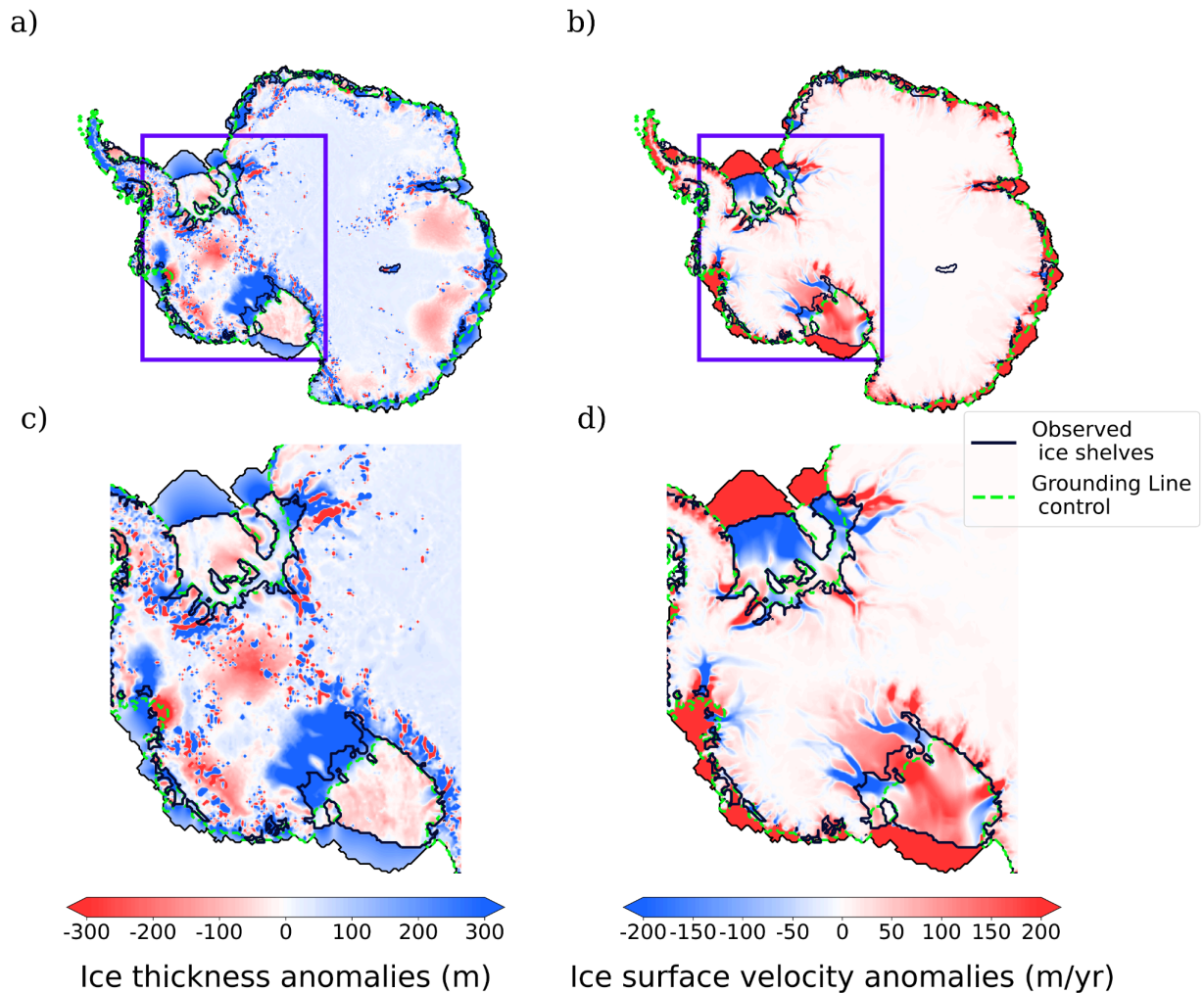

Figure 4. Ice thickness (a, in m) and surface velocity anomalies (b, in m/yr) at 2015 for the medium value of γ_0, with respect to observations from the BedMachine Antarctica V2 dataset and Rignot et al. (2011) dataset respectively. Zoom over the WAIS region (c and d) to better see details on the grounding line.

Figure 5:

1. In the caption, line 2, replace "The values on" by "The values in".
2. In the caption, line 3, add "at year 2500" after "contribution".

a)

b)
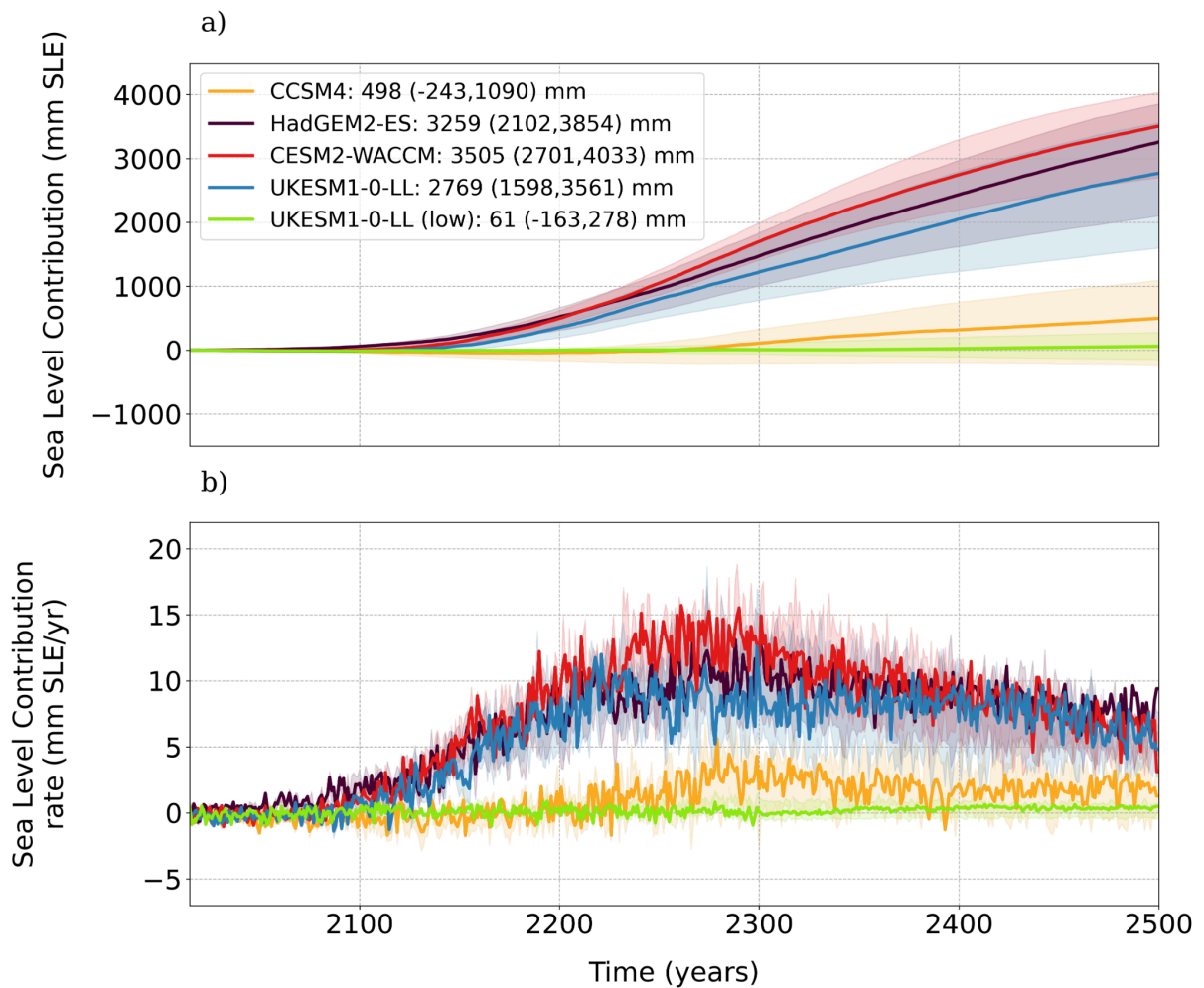
Figure 5. a) Projections of the AIS sea-level contribution (mm SLE) for the five scenarios chosen (Table 1), relative to the control run. In shaded colours the spread resulting from the range of γ_0 values is represented; the solid line corresponds to the medium value. The values in the legend are indicative of the sea-level contribution given by the medium value followed inside the parentheses by the corresponding low and high value. b) Same as a) but for the rate of sea level contribution (mm SLE/yr).

Figure 6:

1. Place the title from the y-axis to the top of the figure.
2. Add a y-axis title "WAIS" for panel (a) and "EAIS" for panel (b).
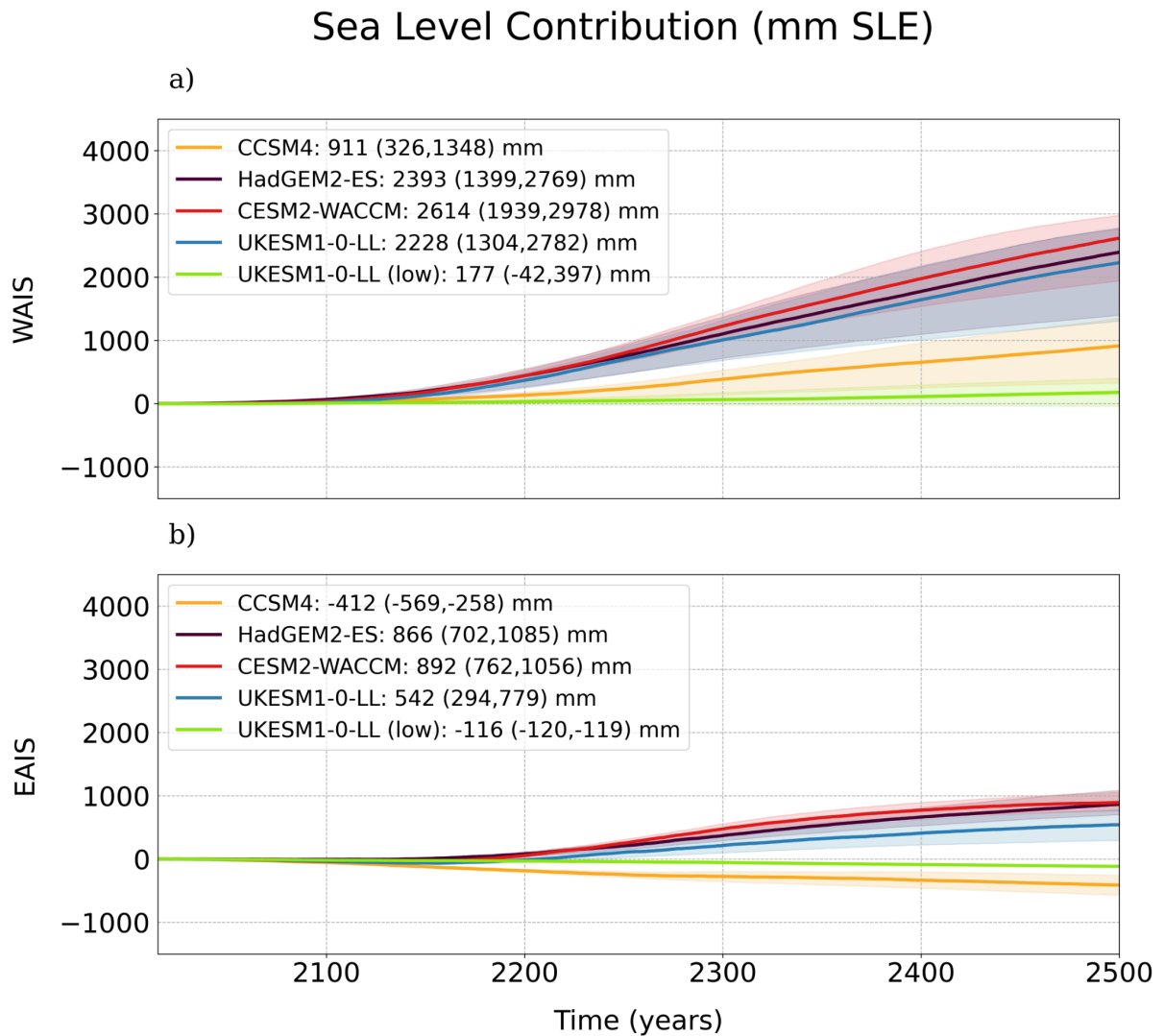
Figure 6. Evolution of the sea-level contribution (mm SLE) from the two main regions of Antarctica, the WAIS (including the Antarctic Peninsula, a) and the EAIS (b), relative to the control run, since the start of the simulations in 2015 to year 2500.

Figure 7:

1. Make this figure bigger if you can.
2. It is very difficult to see the grounding lines. It looks like the stronger differences are for WAIS so maybe create a figure that focuses on WAIS.
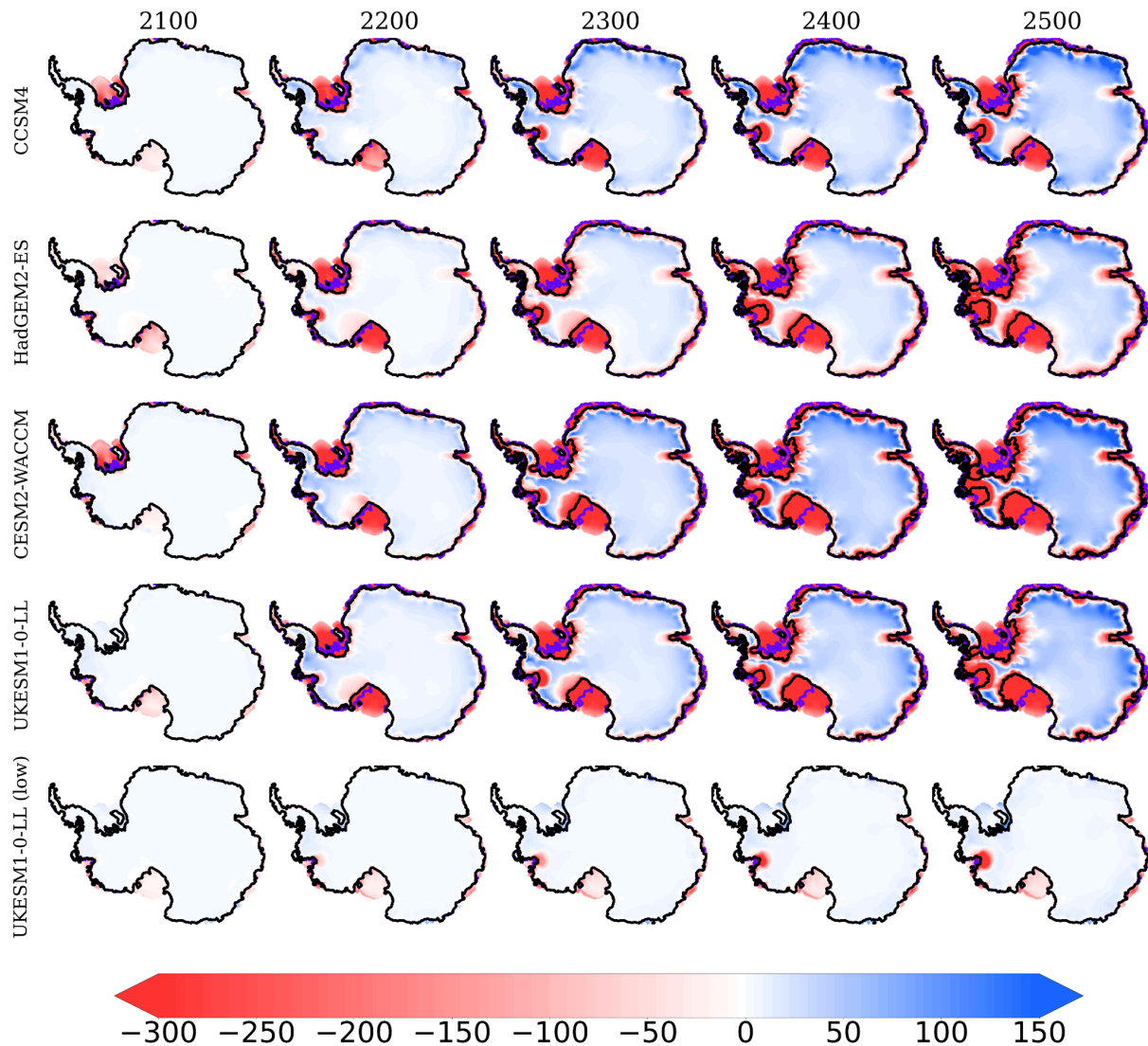
Figure 7. Ice thickness anomalies (in m) for the years 2100, 2200, 2300, 2400 and 2500 with respect to the start of the simulations for the set of five experiments carried out with the median value of γ_0. The grounding line is represented by black and violet colours for 2500 and 2015 respectively. To show negative anomalies in the WAIS, no masking has been implemented as time evolves, keeping the original coastlines.
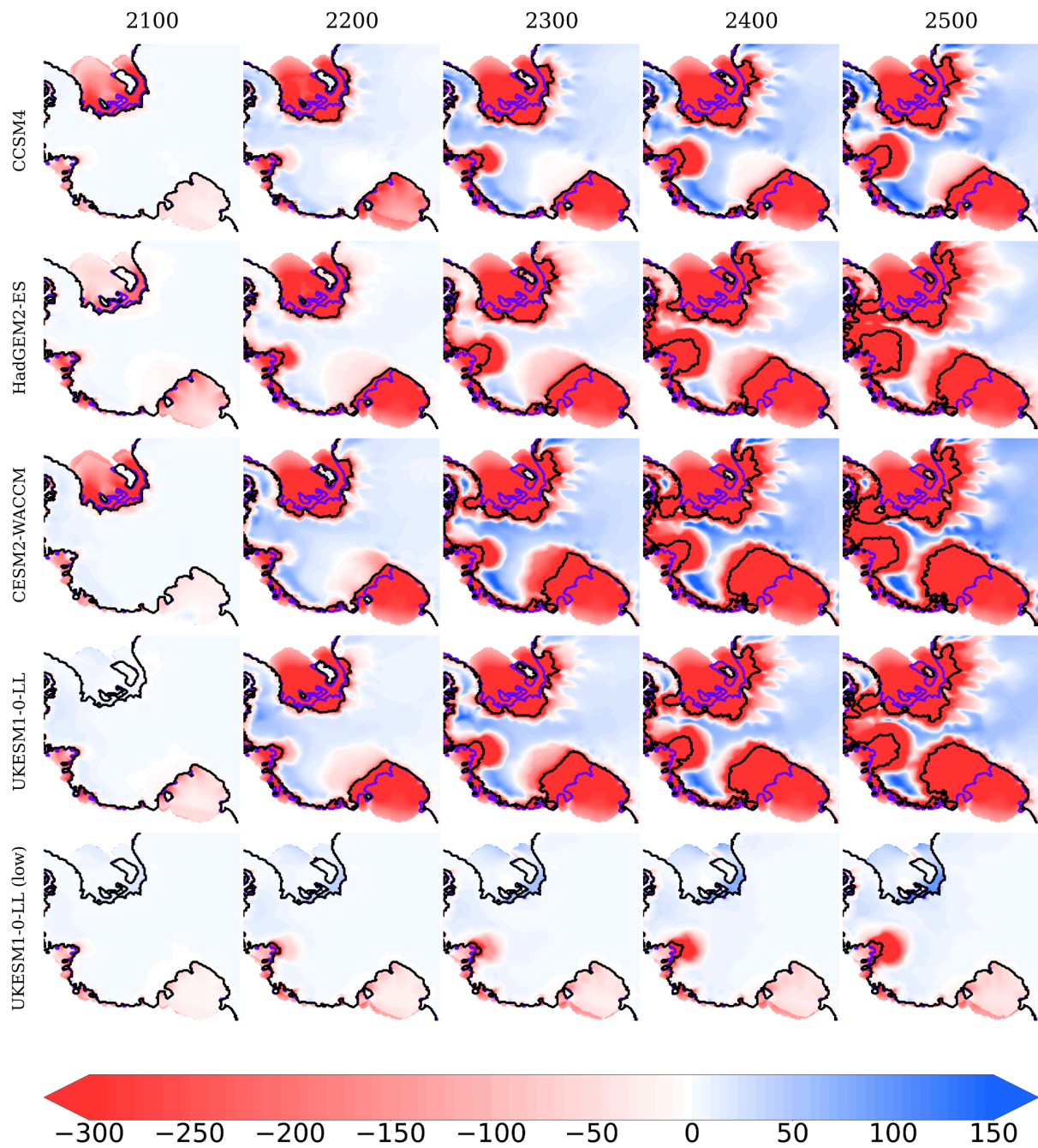
Figure 7*. Ice thickness anomalies (in m) in the WAIS (defined as in Fig. 4a) for the years 2100, 2200, 2300, 2400 and 2500 with respect to the start of the simulations for the set of five experiments carried out with the median value of γ_0. The grounding line is represented by black and violet colours for 2500 and 2015 respectively. To show negative anomalies in the WAIS, no masking has been implemented as time evolves, keeping the original coastlines.
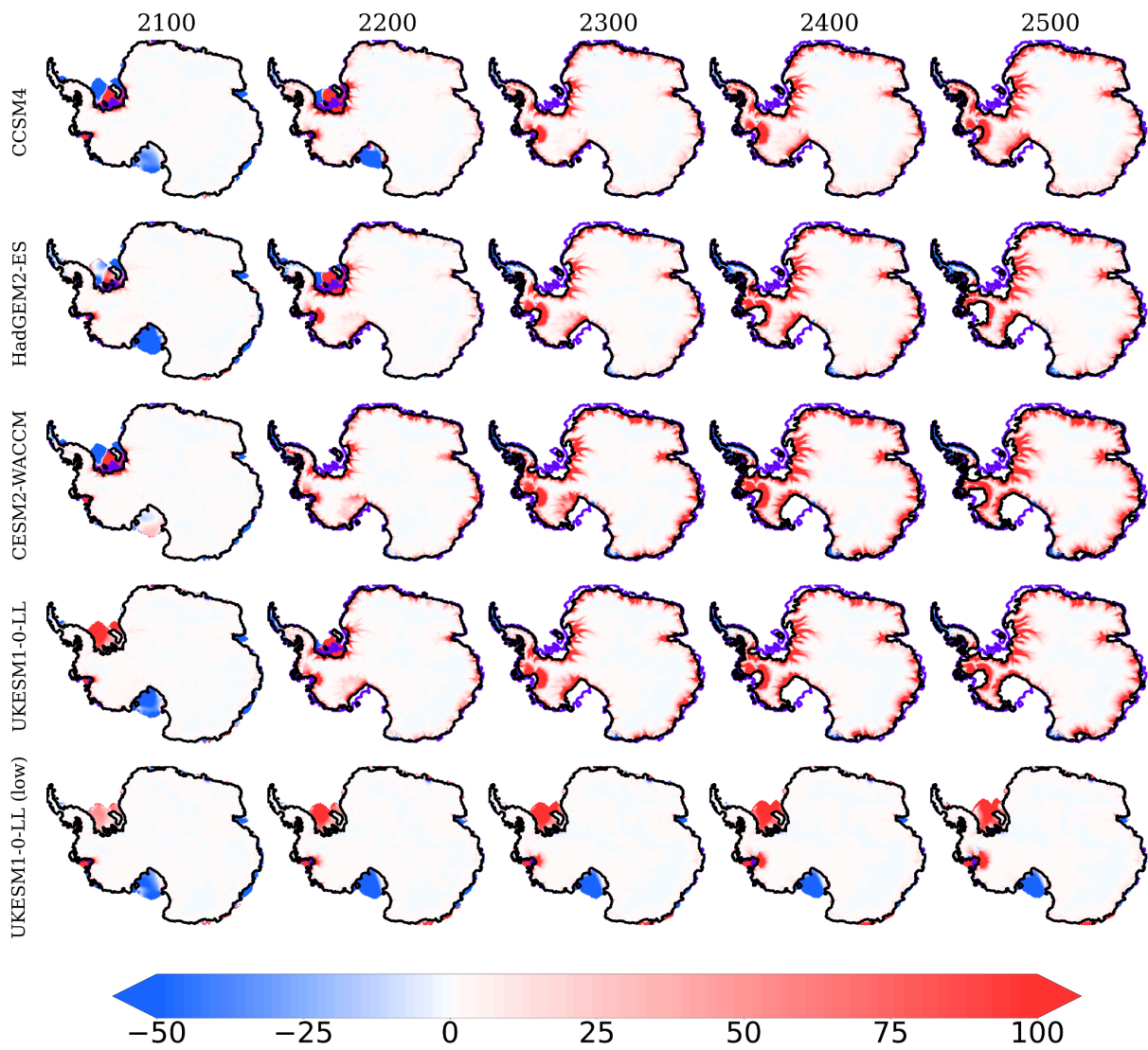
Figure 8. As Figure 7 but for ice surface velocity anomalies (in m/yr). Note that only the cells where there is ice (floating and grounded) at the specific times are represented.
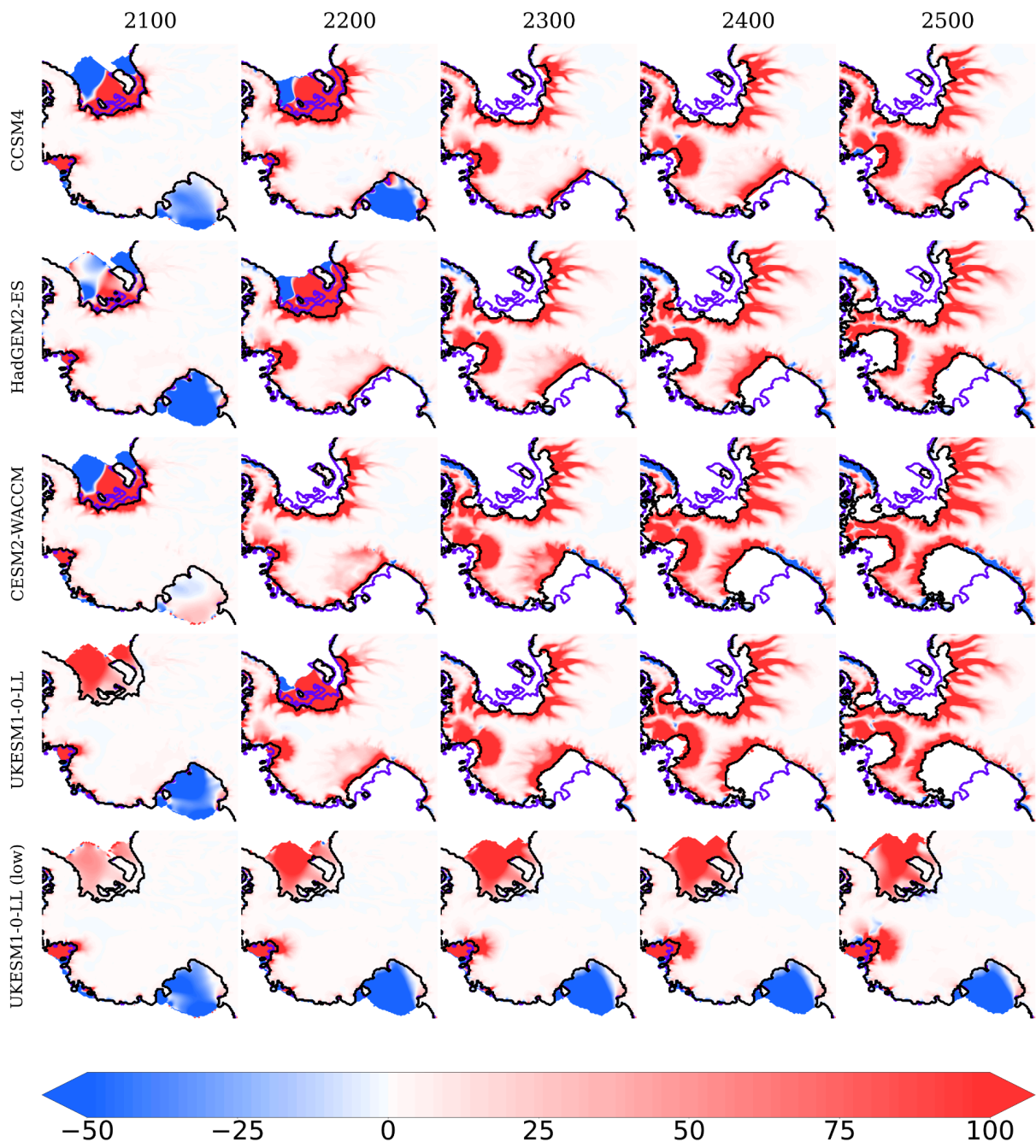
Figure 8*. As Figure 7* but for ice surface velocity anomalies (in m/yr).

Figure 10:

1. Do these time series begin with the Yelmo ice extent which is larger than observed?

Yes, the initial state of the simulations is the last state of the spin-up which in fact has a larger than observed ice extent in some ice shelves around Antarctica, especially in the west region but also in the east. For the revised manuscript, we will write a sentence at the caption or in the text explaining this.

Figure 11:

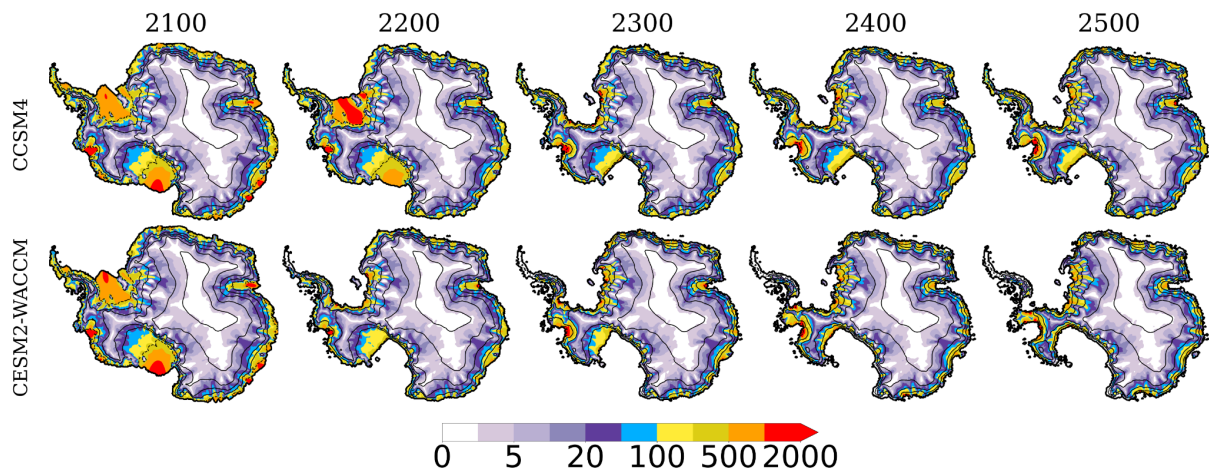1. Make this figure bigger if you can.

Figure 11. Ice surface velocity evolution (in m/yr) at the beginning of each century for the experiments with the GCM models CCSM4 (top) and CESM2-WACCM (bottom). Solid lines represent the coastline and surface elevation levels at the year considered while dashed lines delineate grounding lines.

Figure 12:

1. Place the title from the y-axis to the top of the figure.
2. Add a y-axis title "Atmospheric forcing only" for panel (a) and "Ocean forcing only" for panel (b).
3. In figure caption, instead of saying "is neglected […]" maybe say something like "the inversion process lead to similar initial ice sheet states regardless of \gamma_0". (\gamma_0 influences the impact of ocaen state in ice cavities.)
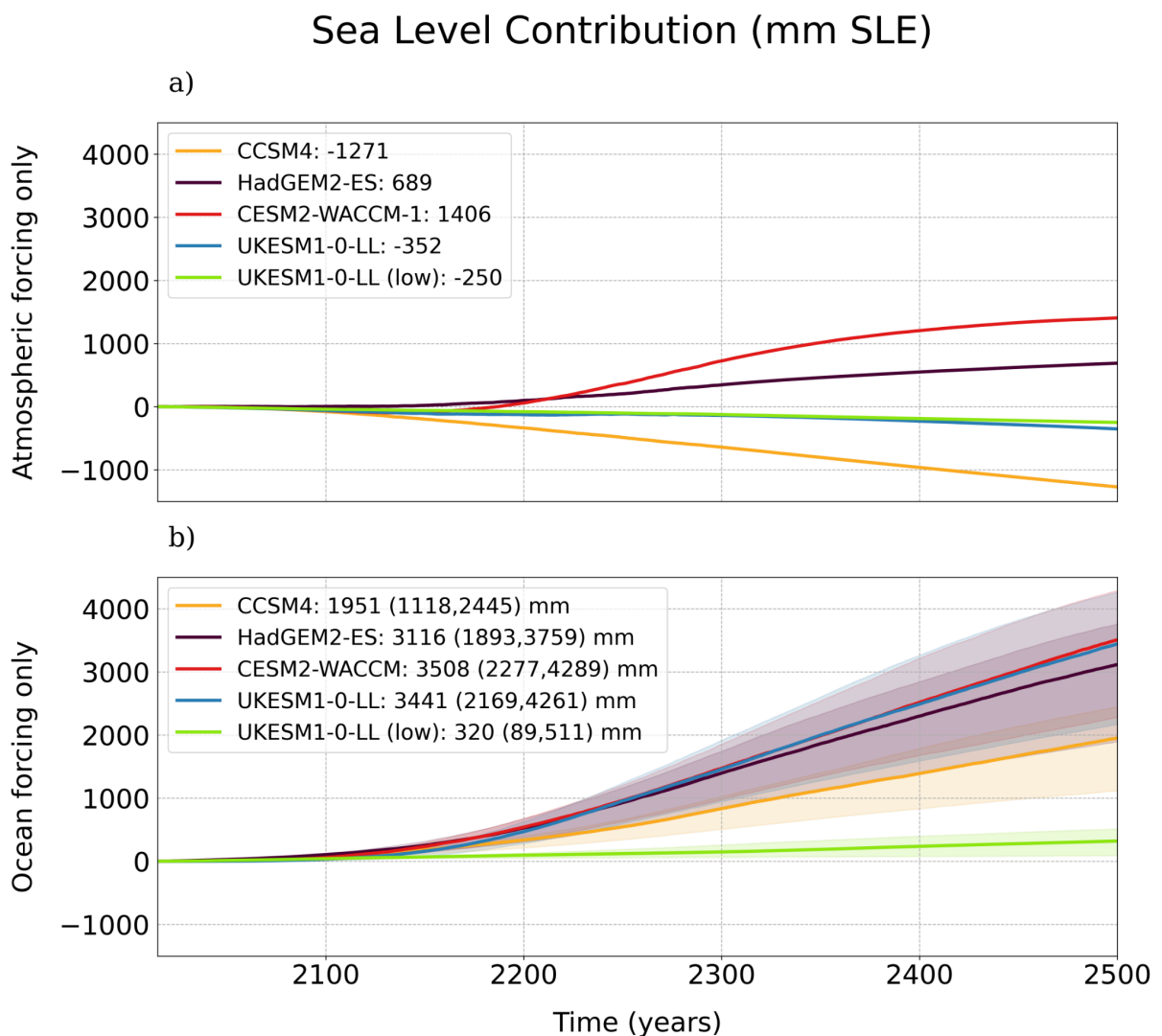

Figure 12. Sea-level contribution of the AIS (mm SLE) in the cases where only the forcing of the atmosphere (a) or the ocean (b) are considered. The shading in b) indicates the spread produced by the effect of the values of γ_0, while in a), as the optimization process leads to similar initial ice sheet states regardless of γ_0, the ocean fields maintain the present-day state.

Figure 13:

1. Make the figure and the font size bigger.
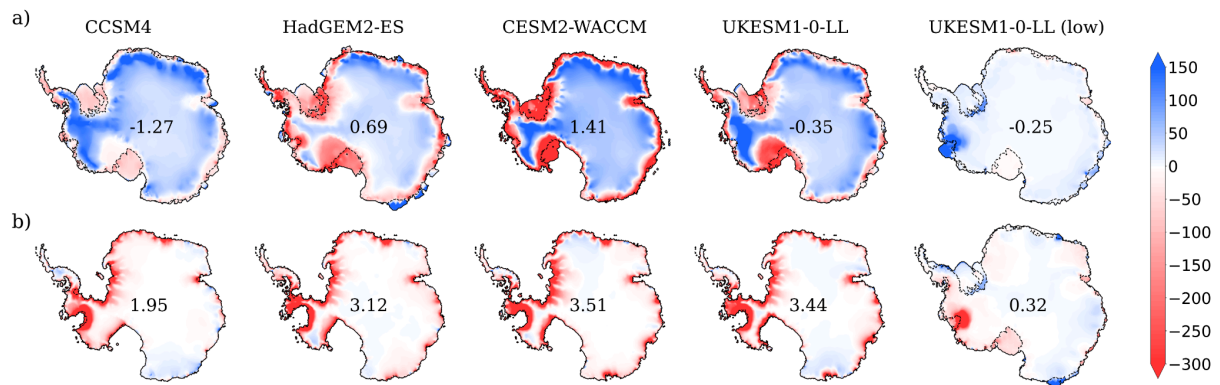2. Refer to the panels in your caption.

Figure 13. Ice-thickness anomalies (in m) for the atmosphere-only (a) and ocean-only (b) cases between the year 2500 and the start of the simulations. Solid lines and dashed lines represent the coastline and grounding line, respectively, at year 2500. Numbers included in the maps represent the total sea level contribution in metres for the medium value of the basal melting parameter.

References We will add them to the next version and cite them properly. Note that there are some others in blue cited by us in our answer.

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