Reviewer #1

General comments

The authors have done a great job at addressing most of my comments. There are nonetheless two points that still need to be improved:

First of all, we would like to express our sincere gratitude to the reviewer for their thorough comments and constructive suggestions, which have greatly contributed to improving the quality of our manuscript. We also appreciate the reviewer's recognition of our efforts during the revision process, and we have learned a great deal through this experience. Next, we will address the two remaining points raised by the reviewer.

1- To keep a realistic freshwater budget for the ocean in the accelerated approach, the authors suggest "applying periodic restoration techniques to adjust the ocean's salinity and temperature fields using observed or targeted values". For multi-centennial projections, which are identified as the ideal target for the accelerated approach, such restoring nonetheless requires the prior knowledge of temperature and salinity projections. Hence, the accelerated approach would only be applicable for a kind of downscaling of the CMIP simulations with an ice sheet-ocean model, not for a fully coupled climate model with interactive ice sheets. This should be mentioned in the discussion.

We thank the reviewer for pointing out the limitation of 'applying periodic restoration techniques to adjust the ocean's salinity and temperature fields using observed or targeted values' in fully coupled climate models with interactive ice sheets. In lines 571-574 of the revised version of the manuscript, we have incorporated a discussion of this limitation, which states: "For multi-centennial projections, the ideal target for the accelerated approach, such restoration requires prior knowledge of temperature and salinity projections. As a result, the accelerated approach is most applicable for downscaling simulations from the Coupled Model Intercomparison Project (CMIP) using an ice sheet-ocean model, rather than for fully coupled climate models with interactive ice sheets."

2- It is a problem that the abstract does not clearly state the caveats (challenges) of this approach. Currently, the abstract ends with "When appropriately applied, the accelerated approach can be a useful tool in coupled ice sheet-ocean modelling", which is not really demonstrated given the remaining questions on the mixed time scales (seasonal to climate trends) in realistic simulations and the associated challenge to close the ocean freshwater budget (see previous point).

We apologize for the omission in the abstract. To address the caveats of the accelerated forcing approach in the abstract, we have replaced the sentence "When appropriately applied, the accelerated approach can be a useful tool in coupled ice sheet-ocean modelling" with "We have also discussed the limitations

of applying the accelerated forcing approach to real-world scenarios, as it may not be applicable in coupled modeling studies addressing climate variability on sub-decadal, decadal, and mixed timescales, or in fully coupled climate models with interactive ice sheets. Nevertheless, when appropriately applied, the accelerated approach can be a useful tool in process-oriented coupled ice sheet-ocean modeling or for downscaling climate simulations with a coupled ice sheet-ocean model." This revision has been made in the abstract of the revised version of the manuscript (lines 18-23).

Reviewer #2

General comments

Dear editor Riccardo Farneti, dear author Qin Zhou and others,

I appreciate the changes made to the manuscript and congratulate the authors for the good paper. I feel that my comments and the ones of the other reviewers have been addressed sufficiently.
After responding to some comments, which are mostly of technical nature, I can recommend the manuscript for publication in GMD.

Best regards,

Moritz Kreuzer

We are grateful for the reviewer's positive feedback on our revision and for the technical comments provided on the revised manuscript. We would also like to thank the reviewer for the valuable comments and suggestions from the previous round of review, which have greatly enhanced the quality of the manuscript. Below, we provide our point-by-point responses to the reviewer's technical comments.

Specific Comments

-L. 83: "sensitive to the boundary conditions" - the authors wanted to change this to "sensitive to the timescale of varying boundary conditions"

We have changed "sensitive to the boundary conditions" to "sensitive to the timescale of varying boundary conditions " in the revised version of manuscript.

- l.143: "(ROMS, (Shchepetkin and McWilliams, 2005)" - one bracket too much

Corrected.

- Fig. 3: "FVCOM-ISMOP+" - change to "FVCOM-ISOMIP+".

Corrected.

- l.244: "blue lines" - I think this is supposed to be "cyan lines".

Corrected.

- l.255-259: - Percentages for cavity averaged melt rates given here seem to not exactly match with the values plotted in Fig. 6 (assuming the values are repre-

sented by the center of the blobs).

Thank you for your observation. We apologize for any confusion caused. The numbers in the center of the colored circles in Figure 6 actually represent the period of the oscillating profiles used in each simulation, not the percentages for cavity-averaged melt rates. To clarify this and avoid misunderstandings, we have replaced " The period of oscillating profiles used in each simulation is denoted by the black text within the colored circles" with " Numbers in the colored circles denote the period of oscillating profiles used in each simulation." in the caption of Figure 6 in the revised version of the manuscript.

- l.317: "1200 months" - I suggest to write "100 years" here, similar to the other time spans, that are given in years now.

We have replaced "1200 months" with "100 years" in the revised version of the manuscript.

- l.338 "over" - remove double occurrence.

Removed.

- l. 370 and 430: "Figure ??" - the text reference seems to be corrupted.

Corrected.

-L. 83: - Fig. 11a - Why is the melt rate around 1.4 m/yr? L. 323 states that Pfast experiments are restarted from FI_C2M simulation, which has a mean melt rate close to 2 m/yr (Fig. 4).

The reviewer's observation is valid. The difference in melt rates between the FI_C2M and Pfast experiments is due to the use of different vertical Prandtl numbers (VPRNU) in the stand-alone and coupled model setups. In FVCOM, the vertical Prandtl number (VPRNU) is defined as the ratio of vertical thermal diffusion to vertical eddy viscosity. It is included in the thermal diffusion term in the temperature or salinity equation. A Prandtl number of 1 implies that turbulent mixing transfers heat and momentum equally, while a value less than 1 indicates reduced thermal diffusion compared to eddy viscosity.
In the stand-alone simulations, VPRNU was set to 1, resulting in a higher melting rate of close to 2 m/yr. In contrast, in the coupled simulations, VPRNU was set to 0.01, which reduces thermal diffusion and leads to the mean melt rate dropping to 1.4 m/yr a few days after initialization. Although these settings differ, they do not affect our study's conclusions, as we did not directly compare the coupled with the standalone simulations. Our conclusions are drawn from analyses within each experiment class independently, where the vertical Prandtl numbers were consistent within each class, ensuring that differences in Prandtl number do not impact the validity of our results.

- l.443 and 445: "Figure 14" - Figure 13?

Corrected.

- l.510: "deviates slightly" - maybe add "only"?

Added.

- l.547: "ice(Bintanja" - add space.

Added.

- l.548: "Moorman et al., 2020), (Bronselaer et al., 2018;" - replace "), (" by "; ".

Replaced.

- "Code availibility" - the embedded link to elmerfem github repository accidently contains a space: "elmerfem .git".

Corrected.

Reviewer #3

General comments

I am satisfied with the authors response to my comments and appreciate the effort gone to in adding additional figures of grounding line movement. Provided the other reviewers are similarly satisfied I would be happy to recommend publication.

We are pleased to know that the reviewer is satisfied with the revisions. We would like to express our gratitude once again for their insightful comments on the grounding line movement in the previous review round, which have significantly enriched our manuscript.