**Rebuttal to the reviews by Josefin Ahlkrona and Ralf Greve**

We thank both reviewers for their insightful comments on the manuscript and would hereby like to address the concerns they raised. Comments in italics, below our rebuttal.

> *JA: The mesh adaptivity is the main feature of the manuscript, and this should be advertised better in the manuscript. The authors added a text on how the mesh adaptivity is used in the benchmark experiments, but I think it is still easy to miss this point. A graphical representation of the mesh used in the benchmark experiments would help. The point of the mesh adaptivity should also be made even clearer in the introduction /abstract: i.e. that despite that it is impossible to resolve dynamics due to the model, there is still a point in resolving topography/data.*

We will explicitly state that the mesh is adapted to the modelled ice-sheet geometry during a simulation in the abstract, and we will add a line stating the main advantage of a locally high resolution as being able to better resolve topographical/other features. We will also add a line to the first paragraph of Sect. 3 (Model verification and benchmark experiments) stating that all benchmark experiments were performed with dynamic adaptive meshes. Lastly, we will briefly mention the dynamic mesh adaptivity in Sect. 2.2 (Unstructured triangular mesh), referring to Appendix E for details (specifically to Fig. E3, which shows a set of different meshes for the Antarctic retreat simulation and thereby illustrates to the reader what a dynamic adaptive mesh means in practice).

> *JA: "Motivate why you benchmark the SIA and SSA separately, instead of benchmarking your complete hy- brid model."*

The newly added MISMIP experiment uses the complete hybrid SIA/SSA approach, not just the SSA. We will clarify this in the manuscript.

> *JA: "Discuss what the need for the semi analytical solution to the grounding line flux means for the accu- racy of practical applications."*

The most important drawback of the GL flux condition is likely still the poor representation of buttressing. No studies have yet investigated just how large an effect this particular model error has on palaeo-ice-sheet dynamics with respect to the uncertainties arising from proxy data, paleoclimate forcing, and other physical processes. We will add a line to the Discussion section to reflect this.

> *JA: "Line 21: "pixels" -¿ grid points?"*

We will correct this.

> *JA: "Equation B6a-b: I would prefer these equations in the main manuscript. Mention what Dc is in a place close to this equation."*

We will move these equations to Sect. 2.3 (Model description - Ice dynamics).

*RG: "On the abbreviation "UFEMISM": I still don't like it. Every knowledgeable reader will immediately assume that it is a finite-element model, and this is unnecessarily misleading. Granted, "UFVMISM" is not so nice to pronounce, but still, this is not good... Should be reconsidered."*

The discussion about the (lack of a) fundamental distinction between finite elements and finite differences has been going on for a long time. Deriving a system of linear equations representing a PDE using finite elements (with linear basis functions) on a regular square grid yields the same result as using finite differences, which suggests that FD is simply a special case of FEM (although many people claim that there still is a fun- damental difference in interpretation, and the identical equations are merely coincidental). In practise, it seems that the choice of name correlates mostly with the choice of grid; discretising and solving a PDE on a regular square grid is called finite differencing, whereas doing it on an unstructured mesh is called finite elements. In our experience, most people associate the phrase "finite elements" with irregular triangles/polygons, and "finite differences" with regular squares. We therefore believe our model name to be appropriate.

*RG: "P. 5, Eqs. (1), (2), and explaining text: The quantity D shouldn't be called a "diffusivity" as it does not appear in any diffusion equation. What is usually called diffusivity D in this context is the depth integral of your D (e.g., Huybrechts et al. 1996, Greve and Blatter 2009 ["Dynamics of Ice Sheets and Glaciers", Springer]). This is the quantity that appears as a diffusivity in the SIA version of the ice thickness equation. However, it is irrelevant in your context as you don't have pure SIA dynamics, so that you must solve the general form of the ice thickness equation (your Eq. (B1))."*

We will change the phrasing of the text accordingly.

*RG: "P. 5, Eqs. (1): The variable zeta should be defined (appears only later in Eq. (D1)). As the non-transformed integral goes from b to z [e.g., Eq. (5.93) by Greve and Blatter (2009) without the contribution from Weertman sliding that you don't have], the transformed integral must go from zeta to 1, and the integration variable should be called zeta' or zeta-bar rather than zeta."*

We agree that the letter $\zeta$ was a confusing choice for the dummy integrand; we will change this to $z'$. Also, the integral should indeed have been from $z' = b$ to $z' = h$; we will fix this.

*RG: "P. 6, l. 5-8: Related to the above said, classifying your model as a Type I model in the sense of Huybrechts et al. (1996) is not appropriate. It does not fit this classification pattern at all as you do not solve the diffusive SIA version of the ice thickness equation."*

We agree that the reference to Huybrecht's et al.'s SIA-only model classification is confusing. We will re- move it.

*RG: "P. 6, Eqs. (6),(7), and explaining text: As for the strain heating, it is not sufficient to say that this is "for grounded ice only". The form you give in Eq. (7) is only valid for the SIA. Since you have hybrid SIA/SSA dynamics for grounded ice rather than SIA, it is an additional simplification to assume that the strain heating is SIA-type. This should be stated clearly."*

We will clarify this in the manuscript.

*RG: "P. 15, l. 3-9: I understand that the experiment, in the authors' words, "is not intended to present a realistic depiction of possible future Antarctic retreat. It only serves to demonstrate computational performance of the model." Nevertheless, I think it should be described in more detail to allow the readers assessing what is going on. Quoting my original review: "What are the initial conditions for the experiments? What are the physical parameters (rate factor, basal sliding law, heat conductivity and capacity, geothermal heat flux, etc.)? What is assumed for ice-shelf basal melting?" If the authors don't want to have this in the main text, it may be put in an appendix section."*

We will add this information to the manuscript.