Author's response to Tijn Berends (RC2)
egusphere-2023-2690

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1 Review

The authors are deeply grateful to the reviewer for their constructive comments. We now provide our answers (regular font) to the main concerns raised by the reviewer (italic).

Major comments

- *Applicability of the 2-D set-up. I am not entirely convinced of the practical use of the "flowline+vertical" set-up presented here. I understand that such a model could help provide accurate benchmark solutions for future idealised-geometry experiments, but the fact that it can never be applied to a realistic ice sheet severely limits its range of possible applications. I therefore think the manuscript would benefit from a clear statement of the intended applications of the model. I am also unsure whether the term "ice-sheet model" is fitting for a model that cannot be used to study realistic ice sheets. Perhaps something like "idealised ice-sheet model" would be more fitting.*

We thank the reviewer for the opportunity to expand on the applicability of 2D models in a non-idealised configuration. The applicability of these ice-sheet models (in the sense of flowline+vertical) is extensively present in the literature. Let us now highlight a number of studies that exhibit the practical use of such a setup and provide insight on realistic ice sheets. Hindmarsh and Le Meur (2001) assessed the dynamical processes involved in the retreat of marine ice sheets, with a particular interest in West Antarctica after the Last Glacial Maximum. Haseloff and Segienko (2018) later considered the effect of buttressing on grounding line dynamics, thus corroborating the findings of existing numerical studies that the stability of confined marine ice sheets is influenced by ice-shelf properties. Other 2D ice-sheet models additionally employ real bedrock geometry sections. This is the case of Pattyn et al. (2006), who studied the role of transition zones in marine ice sheet dynamics and Jamieson et al. (2012), where ice stream stability was investigated on a reverse bed slope. The realism of the 2D setup can even account for glacial isostatic adjustment. To illustrate this, Payne (1995) studied limit cycles in the basal thermal regime of ice sheets considering a constant diffusivity of the asthenosphere. More recently, Bassis et al. (2017) investigated how Heinrich events are triggered by ocean forcing and modulated by isostatic adjustment. Even though ice shelves are not explicitly resolved in 2D models, the potential role of buttressing can be also considered via a parametrisation (Dupont and Alley, 2005; Schoof, 2007; Jamieson et al., 2012, Robel et al., 2014; 2018). We will elaborate on the introduction to emphasise the strong applicability of two dimensional ice-sheet models.

Information about the real world is not only obtained from spatially explicit models and simpler models allow the investigation of the relevance of specific processes. It is often hard to discern the particular physical mechanism underlying certain behaviours for highly sophisticated 3D models, as the strong coupling among elements become an obstacle to isolate hypothesised causes. For this reason, 2D models are an extremely convenient tool to investigate the physical behaviour of ice sheets.

- *Applicability of the moving grid. As has been pointed out already in the original MISMIP publication, moving grids are very difficult to implement in two horizontal dimensions. This strongly limits the applicability of any findings produced with this model. Again, it might produce more accurate results for idealised-geometry benchmark experiments than other models do (at a similar resolution), but it cannot be used to e.g. find improved ways to represent the grounding line which could be applied in other models.*

Our findings show that active thermodynamics perturbs the hysteresis loop and the overall stability of an ice sheet. To do so, we extended MISMIP benchmarks to a thermally-active version where the system is forced via physical variables rather than an idealised ice rate factor. The particular grid over which the equations are discretised do not alter this behaviour, for that our results are not numerical artefacts of the chosen mesh. Therefore, we do not expect our results to change for a different grid discretisation.

More precisely, we expect the same physical behaviour as long as the ice viscosity varies upon temperature changes, irrespective of the chosen grid. The exact grounding line position may differ for a different grid, yet the physical mechanism underlying this mechanism remains unperturbed.

- *These two points together make me unsure what the added value of this model is over using other existing ice-sheet models that have a wider range of applications. E.g., if I hire a PhD candidate to do some work on the relation between thermodynamics and grounding-line migration, why would I advise them to use Nix instead of, say, CISM, PISM, Elmer, or any of the dozen other available models where such processes can be studied in both schematic and realistic cases?*

In all modelling areas there is an inevitable trade-off between a sophisticated physical description and fast computations. Generally, as the system is described with more realism, the necessary calculations demand strong computational resources. The main goal of this work is to present an ice-sheet model that overcomes this issue in certain cases.

There are several reasons for a researcher to use Nix model, among which we can highlight (summarised in Table 1):

1. **Thermomechanical coupling**. Nix explicitly resolves the energy balance by solving for the ice temperatures assuming a number of processes in the heat equation: vertical diffusion, horizontal and vertical advection, strain dissipation and basal frictional heat. As a result, the ice viscosity is thus temperature dependent. Moreover, the basal friction coefficient is also coupled to the thermal state of the base, thus accounting for a potential friction reduction if the pressure melting point is reached.

1. **Fast computations**. It combines a higher-order stress balance with active thermodynamics, while keeping extremely fast computations that allow for statistical studies with thousands of simulations involved, otherwise prohibited by 3D models.

   2a. For wall-clock times of the order of minutes, Nix allows for resolutions of $\Delta x = 0.1$ km (needed to properly resolve the grounding line) and simulated times of order $t \sim 10^3$ kyr. This extremely low computational cost allows for statistical studies with thousands of simulations involved, otherwise prohibited by 3D models.

   2b. Parallelisation. Nix users can optionally select parallel computing (supported by Eigen library), particularly convenient for high resolutions in the Blatter-Pattyn approximation, where large sparse matrices must be inverted. Moreover, it is possible to use Eigen's matrices, vectors, and arrays for fixed size within CUDA kernels.

   2c. It is hard to give a one-to-one comparison since other models that solve for the same higher-order momentum balance coupled with a thermodynamical solver use 3D solvers (partially providing Nix novelty). To give an estimation, the MALI ice-sheet model (Hoffman et al., 2018) control simulations averaged 5.26 simulated years per wall-clock hour. On the contrary, MISMIP experiments run with Nix reach $\sim 10^5$ simulated years per wall-clock hour on average. Thus, there is a 5-order magnitude difference in terms of computational time.

3. **Time stepping**. Unlike other models, Nix offers an adaptive time stepping based on the convergence of Picard's iteration in the velocity solution solver. This approach differs from the standard proportional-integral (PI) methods (e.g., Cheng et al., 2017) and strongly contributes to its fast computational performance in Nix.

4. **Friendly usage**. As a combination of low computational demands, a 2D setup and a clean structure, Nix can be even run on a regular laptop. Installation, compilation and execution are controlled from a simple Python program. This allows Nix to be used without deep knowledge of the C programming language (low-level, procedural and statically-typed). Even though Nix simulations can run on a personal computer, the user can exploit parallelization on a High Performance Computing cluster.

Table 1: Summary of Nix capabilities.

Requirement	Nix capability
Fast computations	Low dimensionality, adaptive time-stepping, parallelization
Higher-order description	Blatter-Pattyn model
Thermomechanical coupling	Temperature-dependent ice viscosity and basal friction
Easy usage	Python wrapper

In the revised manuscript, we have added some text to more specifically highlight these benefits and the potential use cases for Nix.

Minor comments

- *Line 2: "Nix is a 2D thermomechanical model...". It took me a few pages of reading to realise that you meant one horizontal plus one vertical dimension, rather than two horizontal dimensions. Please explain the "flowline+vertical" meaning of 2-D in the abstract.*

We agree that our description was somewhat confusing and will follow the suggestion.

- *Line 4: "...and shallow-ice." None of the experiments you present use the SIA. If the aim of the model is to study grounding-line dynamics, it seems unlikely that it will ever be used at all. If so, consider removing it from the description.*

Indeed, the SIA solver was merely included for the sake of completeness. We will note this more clearly in the revised text.

- *Line 6: "...including those of stochastic nature." This option is not used in any of your experiments. Either add relevant experiments or remove this statement.*

We will remove all mentions to stochastic boundary conditions capabilities as they are implemented but unused in the present study.

- *Line 6: "Nix has been verified for standard test problems" Please state here already that these are the MISMIP experiments.*

We will do so.

- *Line 15-16: "...Nix combines rapid computational capabilities..." You have not shown any results concerning computational capabilities. Either add relevant experiments or remove this statement.*

We will include a figure to show the computational speed as a function of the spatial resolution.

- *Line 21-23: I think the ISMIP intercomparison papers really need to be referenced here.*

We will do so.

- *Line 24: "...leading a number of authors to question their stability" Which authors?*

There is an extensive list of publications on this topic. Among others, we can highlight Bamber et al. (2009) Mouginot et al. (2014), Paolo et al. (2015), Feldmann and Levermann (2015), Shepherd et al. (2018), Rignot et al. (2019), Robel et al. (2019) and Pattyn and Morlighem (2020), Garbe et al. (2020), Jouhin et al.(2021) and Hill et al. (2023). We will include these references in the updated version.

- *Line 42: "...concluding that moving grid models are the most reliable..." Only two of the models in the first MISMIP paper used the flux condition ("Schoofing") approach, and if I recall, none of them used sub-grid friction scaling, both approaches that have since become commonplace in large-scale ice-sheet models (as opposed to moving grids, which I don't think any models use). Please discuss this.*

The reason why a moving grid is not employed in 3D models relies on the technical difficulties of implemention. Nevertheless, explicitly tracking the grounding line position and including it as part of the solution of the problem is still more convenient as shown in Pattyn et al. (2012), and, as in Nix, can be implemented in a flowline model without too much difficulty. A finite element discretisation with adaptive mesh refinement provides an alternative computation approach. We will expand our discussion on the implications of a flux condition ("Schoofing") and sub-grid friction scaling approaches.

- *Line 71-72: "...similar accuracy to the Blatter-Pattyn momentum equations..." While the leading error term might be second-order with respect to the aspect ratio epsilon* *in both approximations, the velocity solutions produced by the DIVA are quite less accurate than those of the Blatter-Pattyn (try running the ISMIP-HOM experiments at the different length scales and you'll see a big difference when L < 20 km). Please include this nuance.*

Indeed, we will include this caveat.

- *Line 76-77: "... emerges as a clear outlier in terms of both model performance and its representation of the ice-flow physics itself." Robinson et al. (2022) did not include a Blatter-Pattyn solver in their comparison, so this statement is slightly misleading.*

We meant that DIVA outperforms all other solvers tested in the study. We will clarify this.

- *Line 81-82: "..., numerical simulations of these rapidly flowing bands are a well-known difficulty." Please elaborate on what difficulties with simulating ice streams are well known.*

This is stated in the first part of the sentence "*[...] the broad range of ice flow speeds observed in real ice sheets (Shepherd and Wingham, 2007; Truffer and Fahnestock, 2007; Vaughan and Arthern, 2007)*". This difficulty partially rests on the inability of ice-sheet models to simulate the observed large range of ice flow speeds. As noted by Bueler and Brown (2009), fast grounded ice flow is a combination of sliding over a hard/soft bed and shear deformation of the basal ice. Nevertheless, high-quality spatially distributed observation of near-base conditions are rare and constraining models becomes challenging. We will clarify this in the text.

- *Line 82: "Diverse approaches are found in the literature..." What approaches in what literature?*

Various modelling approaches have been considered to correctly model the large complexity in ice-stream dynamics. Tulaczyc et al. (2000) found that subglacial hydrology yields multiple modes of ice stream flow in a highly reduced model. Parameterizations of observed small scale phenomena (e.g., drainage networks) were later considered by coupling a flow band model and a simple hydrological model (Bougamont et al., 2003a; 2003b). Another flow band model was employed by van der Wel et al. (2013), additionally introducing a dynamic drainage model. We will elaborate on this briefly in the text.

- *Line 88-89: "It is a common approach to reduce the number of horizontal dimensions to the main flow direction so as to minimize computing time." This is only true for idealised-geometry experiments, not for realistic applications.*

There are several realistic applications published where models solve for two spatial dimensions (Hindmarsh and Le Meur, 2001; Haseloff and Sergienko, 2018), further employing real bedrock geometry sections (Pattyn et al., 2006; Jamieson et al., 2012) and glacial isostatic adjustment (e.g., Payne, 1995; Bassis et al., 2017). Additionally, the potential role of buttressing has been also considered in 2D models via a parametrisation (Dupont and Alley, 2005; Schoof, 2007; Jamieson et al., 2012; Robel et al., 2014; 2018).

Even so, we are fully aware of the limitations of a 2D setup. For this reason, the intended applications of the Nix model substantially differ from e.g., volume estimations. We will clarify the text to reflect this.

- *Line 91: "Unlike previous models..." The original MISMIP paper includes both a higher-order model and a full-Stokes model. Several existing 3-D ice-sheet models can use higher-order or full-Stokes dynamics with thermomechanical coupling (e.g. Elmer/ice, PISM, ISSM, UFEMISM). Please add some nuance here.*

There are indeed a number of models that use higher-order or full-Stokes dynamics with thermomechanical coupling. Nonetheless, they solve a far more computationally expensive 3D problem. Nix model demands far lower computational resources, offers an easy usage, straightforward visualisation and maintains the higher-order physical description of the system. We will include some nuance in the text.

- *Line 101-102: "...for efficiency and extremely fast computing..." Define "extremely fast".*

It is hard to give a one-to-one comparison since other models that solve for the higher-order momentum balance coupled with a thermomechanical solver are full 3D solvers (partially providing Nix novelty). To give an estimation, MALI ice-sheet model (Hoffman et al., 2018) control simulations averaged 5.26 simulated years per wall-clock hour. On the contrary, MISMIP experiments run with Nix reach $\sim 10^5$ simulated years per wall-clock hour on average. Thus, there is a 5-order magnitude difference in terms of computational time.

- *Line 103: "...NetCDF and Eigen libraries" Please include references for these libraries. Also, do any of them make use of parallel computing?*

Nix users can optionally select parallel computing (supported by Eigen library), particularly convenient for high resolutions in the Blatter-Pattyn approximation, where large sparse matrices must be inverted. Moreover, it is possible to use Eigen's matrices, vectors, and arrays for fixed size within CUDA kernels. We will include references to NetCDF and Eigen libraries.

- *Line 113: "Our system is thought to thermodynamically evolve in time..." A strange way to phrase this.*

We will rephrase.

- *Line 128: "...extremely high spatial resolutions..." Define "extremely high".*

We refer to $\Delta x \sim 0.5$ km. We will be explicit in the manuscript.

- *Eq. 11: The basal drag coefficient beta does not appear in any other equations. Is beta an input use to calculate c_b in Eqs. 9 and 10?*

No, β is in fact calculated in Nix and c_b is an input coefficient. We wanted to follow the standard notation for basal friction as $\tau = \beta u$, where $\beta(u)$ can be a function of the velocity. We will clarify this in the text.

- *Eq. 18: the squiggly rho (ratio of densities) is difficult for me to distinguish from the regular rho (density). As you simply write out ρ_w/ρ in e.g. Eqs. 19, 21 and 22, consider doing so here as well and removing squiggly rho altogether.*

The symbol $\varrho = \rho/\rho_w$ was solely introduced to lighten the expressions. We will be consistent with the notation and update the manuscript accordingly.

- *Eq. 18: what does S stand for?*

S stands for surface mass balance (already noted in line 171).

- *Eqs. 21-22: the melt rate M does not appear anywhere in the continuity equation. Also, T0 in these equations in my understanding indicates the pressure+salinity-corrected (and therefore depth-dependent) ocean freezing temperature, is this also depth-dependent in your model?*

Following Christian et al. (2022), we include the melt rate M in the ice flux computation as an additional term at the terminus position.

Regarding T_0, the model currently works with temperature anomalies ΔT that determine the melt rate M. Since this term is included via ice flux calculations that consider the vertically-integrated velocity (i.e., $q = \bar{u}H$), there is no depth-dependency of temperature anomalies at the present moment. We will include a statement to clarify this.

- *Line 263: "Oceanic melting beneath ice shelves..." Earlier, you explained that in the flowline case (which assumes no buttressing), the geometry of the shelf does not affect the flow of the grounded ice, and that therefore you do not need to model a shelf. How then can sub-shelf melt affect your model? Is this included as a negative mass balance term at the last grid point of your grounded domain, as a sort of frontal melt rate? If so, how is this derived from the sub-shelf melt rate?*

The sub-shelf melt is included as an additional term in the ice flux computation (see our previous answer). The ice thickness then adjusts to this increased outflow of ice at the grounding line via the mass continuity equation (see also supplementary material in Christian et al., 2022).

- *Line 280-281: "...the grid points distribution yields higher resolution near the grounding line following a polynomial or an exponential law." Please provide this law.*

The law will be provided in the Appendix.

- *Line 283: "...moving grid models are presumably the best choice from a numerical perspective..." Please clarify that this does not apply to models with two horizontal dimensions.*

We will include a statement to clarify this. To the authors' knowledge, a moving grid mesh has been only applied to one horizontal dimension, upon which it outperforms the other discretisation choices as shown in Pattyn et al. (2012).

- *Figure 2: It is unclear on which grid points you define velocities, and on which you define ice thicknesses/temperatures. Also, the caption states that "..., the position of the last horizontal point (r - 1/2) explicitly tracks the grounding line L(t)". Does that mean that grid point r is floating?*

No, the point r does not exist as we start counting in 0 (Fig. 2). The point $r - 1/2$ is the very last point and it explicitly tracks the position of the grounding line. We follow an Arakawa grid type C. Velocities are thus evaluated at the centers of grid faces and scalar magnitudes (e.g., ice thickness and temperatures) are computed at the grid centres.

- *Section 6: at what horizontal and vertical resolutions do you run your simulations? What kind of error would you expect based on the convergence tests you mention (but do not show) later on?*

Simulations are run at $\Delta x = 2$ km. We have found negligible differences among simulations with $\Delta x \leq 5$ km.

- *Figure 3b: does this include both the advancing and retreating phases of the experiment?*

Yes, both branches coincide and points overlap. We will clarify this in the text.

- *Line 405: "...near the melting point (Fig. 4d)." This should be Fig 5d.*

This is a typo indeed. Thank you.

- *Line 408: "...minimum temperature of 23^{o}C (Fig. 4c)." This should be Fig. 5c.*

This typo will be fixed. We thank the reviewer.

- *Figure 5: panels A and B are not referenced in the text.*

References will be included in the manuscript.

- *Line 424: "Figure 7 illustrates the high sensitivity to that stems from the heat exchange velocity parameter gamma." I find it difficult to understand the goal of this experiment. If I understand correctly that you use the melt rate M as a sort of frontal melt/calving rate, then M is a scalar number which scales linearly with gamma. So if you were to put M on the horizontal axis, then the curves of the four experiments should overlap, correct?*

The goal of this experiment is to show the strong sensitivity to the heat exchange velocity parameter γ range of values given in Favier et al. (2019). When presented as a function of M, the four experiments should overlap. Nonetheless, the main point here is the large range in temperature anomalies at which the ice-sheet collapse occurs depending on the particular γ value.

- *Figure 6b: how much time is there between the steps in ocean temperature? It looks like about 30,000 years, is that enough for the temperature to reach a steady state? In my experience the ice geometry itself equilibrates quite a lot faster. What would you expect to see if you reduce the time between the steps, so you deliberately prevent the temperature from reaching equilibrium? This is where the added value of thermomechanical coupling would really appear.*

Yes, precisely 30 kyr. We will state this clearly in the text. This step length allows for the ice ice geometry to reach a steady-state as these experiments are intended to be quasi-equilibrium simulations (as MISMIP). If we were to reduce the time between steps, the hysteresis loop would then be a transitory response given that the ice temperature may not adjust to the new geometry. The added value of the thermomechanical coupling does not solely rely on the transitory response (i.e., thermal inertia), but also on the perturbed stability of the quasi-equilibrium hysteresis loop. In other words, the thermomechanical coupling already determines the stable regions in a quasi-steady description and not only through the effects of thermal inertia. Our goal here is to first show this more fundamental behaviour. But, of course, a further benefit will be to be able to study transient responses too, which we will highlight in the discussion.

- *Line 439: "...a sensitivity test to spatial resolution (not shown)..." Why do you not show this? I'm actually quite interested. The stress-free boundary condition to the Blatter-Pattyn approximation at the ice surface is very tricky to implement, and I'm curious to see what order of convergence you get.*

Convergence of a number of Stokes approximations to the semi-analytical results of Schoof (2007) was already presented in MISMIP experiments (Pattyn et al., 2012). However, we will include an additional figure showing the convergence towards the analytical solution.

- *Line 459: "Setting the vertical advection at the surface equal to the accumulation (0.3 m/yr) is a standard choice..." What do you mean by this? It is my understanding that most large-scale ice-sheet models these days explicitly solve the heat equation in three dimensions, with the vertical velocity that appears in the advection term resulting from vertically integrating the 3-D horizontal ice velocity field (i.e. conservation of mass for incompressible ice). This is not so much a choice as it is simply a direct consequence of basic physics.*

Vertical velocity can be indeed calculated from the incompressibility of the Stokes flow. We meant that the analytical solutions presented in Moreno-Parada et al., (2022) consider a prescribed vertical velocity profile where the surface value equals the accumulation rate (0.3 m/yr). This paragraph will be rephrased for clarity.

- *Line 473: "...stochastic boundary conditions capability..." You have not shown anything relating to his.*

We will remove all mentions to stochastic boundary conditions capabilities as they are implemented but unused in the present study.

- *Line 491-493: "More generally ... and viscosity" What does this imply for models of realistic ice sheets? It is still not uncommon for models in the ISMIP6 ensemble to neglect evolving ice temperatures in future projections of the Antarctic ice sheet. Do your findings imply that this introduces a significant error/bias in the results of these models?*

ISMIP6 projections reached the year 2100. From a purely thermodynamic point of view of the Antarctic Ice Sheet, ~ 100 years is too short a time span to yield notable changes in the thermal structure of the ice sheet. We must stress that experiments carried out in this work range a simulated time of $\sim 10^2$ kyr. Therefore, we would not expect significant errors in such a short timescale.

- *Appendix A: there are two Appendix A2's, please fix this.*

This typo will be fixed.

- *Line 510: "The position in the spatial coordinates is then given by..." These expressions are not correct when the grid is irregular (and $\Delta\sigma_i$ is indeed a function of i). Also, time is not a spatial coordinate.*

Thank you for pointing out this mistake. The position is instead give by: $\sigma_i = \sum_{k=0}^{i} \Delta\sigma_k$ and $\zeta_i = \sum_{k=0}^{i} \Delta\zeta_k$. The Appendix will be updated accordingly.

- *Line 522: "We thus have a linear system of 6 $\times$ r $\times$ p unknowns..." Don't you mean r*p unknowns, interrelated by a matrix with 6*r*p non-zero coefficients?*

Indeed, we will fix this typo.

- *Appendix A1: Please also provide the discretisation scheme you used for the boundary conditions.*

Nix uses a standard two-point discretisation for the top boundary condition. We will expand the appendix to include it.

- *Appendix A5: adaptative = adaptive*

This typo will be fixed.

- *Appendix B: Either include some experiments with stochastic forcing, or remove this text.*

We will remove all mentions to stochastic boundary conditions capabilities as they are implemented but unused in the present study.