Reviewer report EGUSPHERE-2025-2242

MinSIA v1: a lightweight and efficient implementation of the shallow ice approximation

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The current work explores the numerical benefits of a new algorithm to solve the Shallow Ice Approximation (SIA). It combines a semi-implicit time-stepping with a dynamic smoothing to deal with the nonlinear flow velocity. The scheme is only implemented in MATLAB, as the Python version is still under development. Numerical tests are performed to study the numerical stability in terms of the smoothing factor and timestep, focusing on the potential staircase oscillations and mass conservation.

Overall, I find that the message of the paper is successfully conveyed. Nevertheless, I have a number of major remarks that I would like the author to address and further elaborate on the manuscript. In particular, the work falls short in certain aspects that I have listed in the Major Remarks section. Lastly, I have included a short list with other minor remarks at the end of the document.

1 Major remarks

- **Analogy with Airy's linear theory.** The author did not provide any physical justification of the analogy with Airy's wave theory: a linearised description of the propagation of gravity waves on the surface of a homogeneous fluid layer. A priori, this problem fundamentally differs from the SIA description of glaciar ice tackled by MinSIA. In my view, there are several flaws:
  - Airy's exponential decay is a consequence of water wave kinematics (Dingemans, 1997), not directly transferable to ice sheet mechanics. How is the horizontal smoothing in ice surface justified by the exponential decay along the vertical dimension in Airy's linear theory? Why larger ice thicknesses imply a larger length scale of smoothing?
  - Ice behaves as a non-newtonian fluid, not considered in Airy's theory. Viscous dissipation in ice dampens short-wavelength perturbations, altering how surface effects propagate.

  In summary, the analogy lacks physical justification. Ice thickness does not play the same role as wavelength in water waves decay, unless otherwise shown by the author. With the current description, the smoothing appears as an heuristic fix (as stated by the author: "*the thickness-weighted average avoids artifacts at the boundaries of ice-covered area*"), not a physics-derived approach. I encourage the author to either provide a detailed physical justification of the analogy or remove the reference to water wave propagation, thus re-framing the motivation.

- **Inadequacy of SIA.** As described by Greve and Blatter (2004), the SIA is justified for large ice sheets where conditions generally vary little over horizontal distances (i.e., 5-10 times the local ice thickness). However, this is not often the case in mountain glaciers, where longitudinal and transverse coupling of stresses are important and should therefore be considered. To illustrate this inadequacy, Fig. 7.4 (Greve and Blatter, 2004) shows a scatter plot comparison of the First Order Approximation and the SIA velocities for Haut Glacier d'Arolla: the latter underestimates the velocities for values below 10 m/yr, and vice versa for velocities above this threshold.

  Given that "*The purpose of this paper is to challenge the hypothesis that the potential for further improvements in computational efficiency with classical numerical methods is limited (Jouvet et al., 2021)*", I encourage the author to choose a more suitable stress balance approximation and discuss the differences with a more sophisticated description. If the SIA is nonetheless correct, then a simple plot as Fig 7.4 in Greve and Blatter (2004) will suffice to justify and discuss the choice over the domain. Note that the work of Jouvet et al, (2021) employed PISM output to train the emulator, capturing the physics contained in longitudinal stresses present in the SSA. It would be also convenient to discuss how the second-order SIA peforms in this context (e.g., Ahlkrona et al, 2013.)

- **Lack of smooth-free reference.** As stated by the author: "*It would be desirable to use a simulation without smoothing as a reference scenario*". There is an inevitably trade-off between smoothness and numerical stability. I consider that it is thus mandatory to have a reference simulation to quantify the deviation from the smooth-free solution. As stated in the paper: "*the smoothing factor was set to f = 0.25 as a reference*". Why is it so? This number seems arbitrary without further justification. In fact, it corresponds to a strongly oscillating result (Fig. 4) that need a nearly 10^8 m^3 of ice in order to keep the ice surface consistent (Fig. 5). Please, provide detailed physical justification on why $f = 0.25$ should be a reference value.

- **Python implementation.** As stated by the Editor, MATLAB is not the most "open software", so it would be convenient to provide the Python software under development and discuss the performance in the manuscript. In this line, NVIDIA recently announced the cuNumeric library, a drop-in replacement for the NumPy library that allows to run on multi-core CPUs, single or multi-GPU nodes, and even multi-node clusters without changing your Python code. Operations are executed by Legate's task engine and accelerated on one or many NVIDIA GPUs (if no GPU is present, on all CPU cores). Linking with the previous comment, as the author justify the absence of smooth-free reference simulations as a results of prohibiting computational costs, this apparent issue could be potentially overcome by using cuNumeric library. Either way, since the main focus of the paper is to show that there is still room for improvement in computational efficiency with classical numerical methods, I encourage the author to include a section where parallel performance is elaborated.

- **Missing comparison with measured glacier velocities.** I consider that the perturbation induced by smoothing should be rather framed in the context of observed glacier velocities. It is expected that timestep and spatial resolution will impact the presence of numerical artefacts such as the oscillations discussed, but a comparison with observed velocities is fundamental. If this paper aims at keeping "*classical numerics competitive*", it requires some sort of validation with measured velocities. My suggestion would be to compare MinSIA results with observed values as Jouvet et al. (2021). This will serve as a validation test to quantify to what extent smoothing perturbs the velocity field.

- **Overstated stability claim.** Line 107 of the manuscript reads: "*This semi-implicit scheme already ensures stability for arbitrary time increments δt*". This claim is quite strong and generally incorrect for this specific scheme. If D changes rapidly in space or time, evaluating it explicitly can lead to instabilities for large δt. The scheme is not unconditionally stable. In fact, the author later states in the paper that special focus is needed on the timestep to avoid numerical oscillations (see my next comment) and even discusses situations where the CFL criterion is relevant. Please, revise other vague statements regarding numerical stability.

- **Focus on "time increment" δt.** The paper reads that: "*[...] the systematic error in the volumetric balance is negligible compared to the immediate effect of the staircase oscillations on accuracy. So focus should be on limiting δt to avoid these oscillations*". Can we simply conclude that, given the numerical nature of a semi-implicit scheme, the stability is determined by the timestep? (and therefore contradicts the pervious claim that the semi-implicit scheme ensures stability for arbitrary timesteps). If so, a fully implicit scheme would overcome this issue? Further experiments are needed to support or reject this hypothesis.

- **Timestepping and redundant sections.** I would suggest merging Section 4.2 (The maximum time increment) and 5 (Finding the best time increment) in a single section. All numerical results regarding timestepping should be described and discussed therein.

  Moreover, I find a great amount of effort on the present work while lacking some important points. For instance, Cheng et al. (2017) introduced an adaptive time step control for simulations of the evolution of ice sheets using Elmer/Ice (Gagliardini et al., 2013). Semi-implicit and fully implicit methods are compared for a number of discretization stencils. I consider that if the problem of "finding the best time increment" is to be tackled, the paper should dive into the predictor-corrector (among others) approaches and showcase the performance in MinSIA.

- **Figures:**

  - Figure 1. This plot is hard to interpret: colour bar is missing, legend is missing, spatial scale is missing, inset with geographical zoom-out is also missing. Please, improve the figure so that they are as much self-explicative as possible.

2 Minor remarks

- It would be very convenient to include explicit discretization schemes implemented in MinSIA. Section 3 (Numerical scheme and implementation) elaborates on the smoothing algorithm, but the finite volume and upstream diffusivity schemes are not given in the manuscript. An appendix with the explicit discretization is beneficial for reproducibility and future comparison.

- I wonder how the CFL criterion looks like overlaid in Fig. 7. It is illustrative to show the timestep restriction imposed by the CFL criterion for different resolutions and smoothing values. Moreover, Fig. 7 should also include the deviation from the smooth-free reference simulation. Large values of the smoothing factor could imply unrealistic velocity fields.

- Line 5: What does this mean: "*MinSIA5 is designed for simulations with several million nodes on standard desktop PCs*"? What does the author mean by *node* here? Regular desktop PCs usually have only $\sim$ 16-32 CPU cores.

- Lines 264-270. This paragraph could be synthesized by plotting the computing time as a function of different parameters (e.g., δt, δx, f, etc.). In log-scale, the slope of the linear fit will show the exponential dependency.

References

- Dingemans, Maarten W. (1997) Water wave propagation over uneven bottoms. part 1: Linear Wave propagation. Singapore: World Scientific.

- Cheng, Gong, Per Lötstedt, and Lina von Sydow. "Accurate and stable time stepping in ice sheet modeling." Journal of Computational Physics 329 (2017): 29-47.

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