Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-288-RC2, 2020

Geoscientific Model Development Discussions
Open Access

Interactive comment on "The Utrecht Finite Volume Ice-Sheet Model: UFEMISM (version 1.0)" *by* Constantijn J. Berends et al.

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Received and published: 23 December 2020

In this manuscript, the authors introduce a new ice-sheet model called UFEMISM. It is based on hybrid SIA-SSA dynamics for grounded ice, SSA dynamics for floating ice, and employs finite-volume techniques on an adaptive mesh for solving the model equations numerically. The SIA part of the model is verified against analytical solutions and previous results from EISMINT. An application to the Antarctic ice sheet illustrates the capability of the model to deal with real-world problems and the computing time demands depending on the number of cores and the resolution.

While the new model is very interesting, I find the paper pretty much incomplete. The most severe omission is that no attempt is made to verify the SSA part of the

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model, and the performance of the full model with respect to grounding line dynamics. Granted, analytical solutions are lacking. However, model intercomparisons have been done within the several MISMIP initiatives, most recently MISMIP+ by Cornford et al. (2020, doi: 10.5194/tc-14-2283-2020). To enable the reader to appreciate the performance of the new model, it would really be crucial to carry out such types of experiments and demonstrate how the model behaves in terms of grounding line advance and retreat as a function of resolution.

In Section 2.3, the short description of the ice-thickness solver only applies to the SIA, for which the ice-thickness equation can be written in diffusion form. How is the general ice-thickness equation (that includes the SSA part of the dynamics) solved?

In Section 4, the description of the experiments, given in a mere four lines (p. 14, l. 15-18), lacks detail. What are the initial conditions for the experiments? What are the physical parameters (rate factor, basal sliding law, heat conductivity & capacity, geothermal heat flux, etc.)? What is assumed for ice-shelf basal melting? The tested resolutions should be mentioned in the text, not only in the figure captions. What happens to the ice sheet in a control scenario (no warming applied)? I am actually quite surprised that the ice sheet reacts so strongly on just a surface warming without (I presume) changes in the SMB or the ice-shelf basal melting.

For assessing the computing times (are these wall-clock times?) reported in Sect. 4, some information about the used computer system and compiler would be nice. The reported "5 h 30 m" on p. 17, l. 15, lack any context and should be compared with the values reported earlier for the case of a constant resolution for the ice margin, grounding line and calving front.

Abstract l. 18 (also end of Sect. 4) vs. p. 2, l.11: I find this quite contradictory. If we take the statement seriously that a resolution of < 1 km around the grounding line is needed, then the example of a simulation for all major ice sheets with a 4-km resolution is insufficient. What happens if one really goes down to the required < 1 km? Won't the computing times then become prohibitive?

Minor issues:

P. 2, l. 2/3: This justification does not really work. It is no problem to melt down a significant part of the Greenland ice sheet within only 10ˆ3 years under a decent climate warming. Same for West Antarctica, triggered by the marine ice sheet instability. The mentioned processes do not always require 10ˆ5-10ˆ7 years to become relevant.

P. 2. l. 30: I don't think that Elmer/Ice has an adaptive grid. Not sure about ISSM or MALI either. Please check this in the cited references. BISICLES (https://commons.lbl.gov/display/bisicles/BISICLES) definitely has, it should be mentioned in this context. BISICLES also employs finite-volume techniques.

P. 3, l. 4: "constitutes an optimum" is a pretty strong statement that implies that nobody will ever be able to do it better. Perhaps toning it down a bit; something like "we seek a compromise between these two families"?

P. 3, l. 5: "UFEMISM" for "Utrecht Finite Volume Ice-Sheet Model"? The abbreviation does not seem to fit very well, and the "FEM" in it rather alludes to the finite-element method.

P. 3, l. 16/17 (and again p. 6, l. 14): It is misleading to say that "Flow velocities for grounded ice are calculated using the shallow ice approximation". This applies only to the part due to internal deformation, while the part due to basal sliding is SSA. This is explained only later. Should be reformulated.

P. 3, l. 23: A reference should be provided for the Arakawa C grid.

P. 6, l. 14/15 and Eqs. (1) and (2): u, v and D do not only depend on z, but also on x, y and t.

P. 7, Eq. (4): This form of the strain heating holds only for the SIA. What about the general form for SIA/SSA hybrid dynamics and the SSA for floating ice?

P. 7, Eq. (5): This equation holds for cold ice only. How is temperate ice treated? Just by cutting off temperatures exceeding the pressure melting point (not energy-conserving), or something more sophisticated?

P. 7, l. 21: I find it hard to believe that the stability is independent of the horizontal resolution. There is still horizontal advection in the equation, which is discretized explicitly. The time step of 10 years may be sufficient for all tested cases, but there will be a limit as one goes to higher resolutions.

P. 8, Eq. (7) and p. 9, Eq. (10): I would suggest to give up this separate Gamma factor and simply integrate it in the main equations.

P. 11, caption of Fig. 5: What is meant by "plotting artefact"?

P. 12, Fig. 6: The 50-km resolution can be nicely seen near the ice margin, which is fine. But I'm wondering why it cannot be seen equally well in Figs. 3, 4, 5 and 7? Different interpolators perhaps? If so, I'd suggest to use the same interpolation for all plots and, if it is not simple linear interpolation, it should be mentioned.

P. 13, Fig. 9; p. 14, Fig. 10: Perhaps different colours for the 20-kyr and 40-kyr cases? This would make the figures easier to read.

P. 14, l. 11: "MPI" should be defined.

P. 18, l. 33: Which value of this relaxation parameter omega have you actually used for the Antarctica tests?

P. 25/26, Appendix B: Apparently, a notation is used where subscripts (indices) x and y denote partial derivatives. This should be stated clearly. Further, some references to "Eq. 1" etc. appear that should probably be "Eq. B1" etc.

P. 25, l. 11: I don't understand this inequation. How can one compare the derivative of a quantity with the quantity itself? The units are even different.

P. 28, l. 3: The "solution" of the system of equations is the unknown vector of updated

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temperatures. delta should rather be called the "right-side vector" (or "vector of the right sides").

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