Review of ”Regional modeling of the Shirase Drainage Basin, East Antarctica: Full-Stokes vs. shallow-ice dynamics” by Seddik and colleagues

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This study made by Seddik and colleagues investigates the behaviour of the Shirase drainage basin in East Antarctica, using two sets of equations for the stress balance, thus comparing the most sophisticated full-Stokes (FS) model to the Shallow Ice Approximation (SIA). Prior to transient simulations, both models are initialized by applying an inverse method to the full-Stokes equations, which results in a unique basal friction field used to initialize both the FS and SIA models. The authors then perform a series of transient simulations using some of the SeaRISE experiments. The two models are suposedly based on the same numerics as both included of the finite element software Elmer/ice.

I was asked to review this paper with the specific focus on its novelty. After reading carefully both the papers and the discussion, the only novelty that I can see is the use of the same software (or numerics) and grid for the two representations of the stress balance equation. To my knowledge, the effect of different numerics for this kind of setup has been poorly investigated in the past. I have however the study in mind from Pattyn and Durand (2013) who investigated the results of the MISMIP3D experiments with a focus on the approximation used. They show for instance that all the SSA models behave similarly in the frame of those ideal experiments. For the rest, the fact that SIA gives faster velocities because of the lack of longitudinal and lateral shearing stress, was already shown by, e.g., Le Meur et al. (2004) and, as the authors say, Seddik et al. (2012).

This being said, I have a major criticism of the paper. If the purpose was to reduce the effect of the numerics, I think the way you do your experiments may strongly bias the results, thus vanishing the effect of getting rid of potential numerical issues:

- The initial field of basal friction that the authors use to initialise the SIA model is obtained by applying the inverse method to the FS model. The basal friction field is thus a function of the complete stress tensor, not only the vertical shearing terms (as accounted for in the SIA). While a major assumption is made here, the authors do not discuss the implications or try to show the validity of this approach. For instance, I am quite surprised to see that, while the ratio between the effective strain rate (Figure 10 in the paper) of SIA and FS models is different than the unity only in a limited area of the Shirase basin (the last 20 km or so from the grounding line downstream) the difference between both field of surface velocity is much more extended (up to about 500 upstream of the grounding line, this difference is significant). Looking at the paper from Seddik et al. (2012), the differences between Sicopolis and the FS model of Elmer/Ice seem to be much less spread upstream. In that previous study, the friction law was not relying on the FS equations only, thus it makes me think that the initial field of basal friction is partly responsible for that.
- Also, there is another assumption made that worries me, which is the use of different boundary conditions at the grounding line, either the ocean pressure for the FS model or a constant thickness for the SIA model. This is again something that is not discussed at all, while this crucial boundary is the gate to the ocean and of significant influence on the ice discharge. At least the authors should show an experiment to prove that this assumption has no or little influence on the discharge twoards the ocean.

I have a couple of other critiscims:

- There is no relaxation between the inversion and the predictive simulations. This is made by most numerical studies following an inversion (e.g. Gillet-Chaulet et al., 2012; Cornford et al., 2015) to make sure the initial state is not affected by short-wavelength and large amplitude variation of the surface elevation change rate, which may arise from different resolutions or inconsistencies between the data and the model. If you don’t have any, I would be surprised, but it needs to be mentioned and the initial dhdt need to be quantified.

- The domain is delimited with two lateral flow lines, for which your boundary condition leads to high sliding coefficients. I don't think this is an issue for the most upstream part of the drainage basin (say the ice divide), but this is more worrysome in the vicinity of the grounding line, where it can significantly affect your discharge towards the ocean.

References

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