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Interactive comment on "Quantifying the effect of ocean bed properties on ice sheet geometry over 40,000 years with a full-Stokes model" *by* Clemens Schannwell et al.

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This paper describes the application of a full-Stokes ice sheet model to a modest sized region (about 100 x 300 km) of Antarctica over 40,000 years at a reasonable 1km resolution. Full Stokes models are computationally expensive and have typically been used only for shorter simulations: various approximations are normally applied (quite often at coarser resolution too). The paper also makes use of new data and explores the importance of bed friction in the region, so would be of interest even if it did not manage the full Stokes model. Give the use of the higher-fidelity model, this is an important and clearly written paper. I have a few minor comments only.

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Comments

The abstract perhaps emphasises the Stokes model, but there are two conclusions in the paper – one relates to the importance of the basal boundary condition (sliding law), which might have been reached with a more approximate model. At the same time, there is no less-than-Stokes model considered, so the paper provides us with no information on whether 'uncertainties due to physical approximations [have been] be reduced.', at least compared to the uncertainties that would be common to models (e.g the sliding law)

Specific comments

L42: "The rationale behind this tuning is that if the model matches the constraints well, then confidence is high that the model also reproduces ice sheet changes at other times. The risk involved is that the matching may overcompensate for the simplified model physics leading to higher uncertainties in future predictions where model constraints are absent" I don't disagree with the overall statement, but I would suggest that the rationale is simply that if a model matches constraints poorly, then it should be rejected (or given a lower score).

L105; The thermodynamic equation – how is temperate ice treated?

L153 "A linear viscous sliding relation (m=1) was chosen to guarantee consistency between model intialisation and forcing simulation." This is not needed – the inverse problem provides both C_1 and $|u_b|$, so you could carry out runs. with any value of m so long as $C_1|u_b| = C_m|u_b|^m$. Linear sliding is probably the worst choice (see e,g Joughin 2010) and although many (me included) have used these rules in the past, as a community we should move on. I am not suggesting new runs, but an acknowledgement that the authors understand this position.

L183 "While robust, direct solvers do not take advantage of the sparse structure of the matrix and require large amounts of memory." That is certainly true of e.g. LA-

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PACK solvers, but the MUMPS solver is the MUltifrontal Massively Parallel sparse direct Solver, designed for these sorts of problems. That is not to say that an iterative solver has no advantages, but frontal solvers like MUMPS are specialised over general dense solvers.

L226 "We note however that we do not expect a perfect match between the two solver setups due to small differences in the finite element formulation" This needs a bit more emphasis/elaboration. If you were solving the same problem, you would expect the solvers to give the same results (assuming the iterative technique was successful). But the problems are different? ParStokes does seem to work well though (I would have liked see SSA in the same comparison, but in a follow up paper, perhaps)

L220 "For both simulations, there is good agreement in terms of grounding line position over time, with differences never exceeding 5% (Figure 5)." - the difference is in total grounded area.

L264: "Stable grounding line positions for both simulations are associated with periods of ice sheet stability (Fig. 8). " Steady rather than stable? I agree that you are unlikely to see unstable equilibrium in practice, so steadiness tends to imply stability.

L289 "The high mesh resolution required to adequately capture grounding line migration (Pattyn et al., 2013) is hereby maintained.". Perhaps – there is no convergence study in this paper so it relies on external references, and the only Stokes model in Pattyn 2013 is Elmer/Ice which ran at around 50m resolution.

L385 "The difference between Weertman and pressure limited relations is that the latter take effective pressure into account. This means that basal drag goes to zero near the grounding line and reduces to a plastic sliding relation (Brondex et al., 2017). However, this lower basal drag area is limited to a few kilometers upstream of the grounding line. " There is another important difference, which is the independence of Tb and |u| in the region in question, which could be substantial. See for example Joughin 2019 which provides evidence for Coulomb-like sliding a long way from

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the grounding line. No need to speculate, but please, acknowledge Joughin 2019 Âă https://doi.org/10.1029/2019GL082526

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