Reviewer 2 (Stephen Cornford):

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.

We thank the referee for the thoughtful and thorough review of our paper. We appreciate you taking the time to complete the reviews and welcome your helpful comments. We have revised the manuscript to address your review comments (see below). Throughout this response to review document your (referee review) comments are provided in regular, non-italic font text, our response comments are provided in red font (as here).

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)

We agree that our paper does not show that uncertainties due to different physical approximations have been reduced. Rather, the goal of the paper is to provide a first step towards being able to do this in the near future. To reflect this appropriately in the text, we changed the corresponding sentence to: “Therefore, there is a need to extend the applicability of regional FS ice sheet models to timescales longer than 1,000 years so that uncertainties due to physical approximations in the force balance can be quantified and reduced in the near future.”

We also agree that at least qualitatively, we could have reached the same conclusions regarding different levels of bed friction with a more approximate model. However, the magnitude of grounding-line advance and retreat over such a long time period will most likely be different across different ice mechanical models. This has been shown in the previous intercomparison studies using idealised geometries (e.g. Pattyn et al. 2013).

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).

Agreed. We changed this accordingly.

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

We added the following: "The ice temperature T is bounded by the pressure melting point T_m, so that $T \leq T_m$."

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 C1 and |ub|, so you could carry out runs. with any value of m so long as C1|ub|=Cm|ub|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.

Yes, we are aware of this and agree with the reviewer here. We changed this sentence to read: "A linear viscous sliding relation (m=1) was chosen. Alternative and physically more realistic sliding relations exits (e.g. Joughin et al., 2019) and the consequences of our choice of using a linear sliding relation on the results are discussed below (see section 5.5)."

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. LAPACK 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.

We agree with the reviewer here. Our formulation was not precise enough. MUMPS is certainly tailored towards solving large sparse linear systems. However, the fact that it remains a direct solver still leads to the solver being memory bounded. Therefore, it does not scale at all beyond 80 CPUs. We adjusted the sentence as follows: "While robust, direct solvers require large amounts of memory."

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?

Yes, the problems are slightly different due to different stabilisation methods employed by using MUMPS or ParStokes (see response to other reviewer).

ParStokes does seem to work well though (I would have liked see SSA in the same comparison, but in a follow up paper, perhaps).

We agree that this would have been interesting. However, as of today there is no thermomechanical coupling available when using reduced models in Elmer/Ice (e.g. SSA, SSA*) and that's why we did not perform the same simulations with a reduced model.

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.

Yes. Thanks for spotting this. Changed accordingly.

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.

Yes, steady might be the better term to use here. Changed accordingly.

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 50 m resolution.

The reviewer is correct that we did not perform a convergence study. Given the runtime of the model, we do not think it is feasible to carry out a convergence study for long-term simulations at the moment. Moreover, a mesh resolution of 50 m is certainly only ever applied in simplified settings and for shorter simulation times. To acknowledge the fact that we cannot show that this resolution is adequate, we reformulated the sentence as follows: "We hereby maintain a mesh resolution (~1 km) that is finer than in most other paleo ice sheet simulations (Pollard and DeConto, 2009; Golledge et al., 2014; Albrecht et al., 2020) albeit at a regional scale."

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

We have expanded this section and added the reference. It now reads: “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). This results in the basal drag becoming independent of the sliding velocity. Most previous studies using pressure-limited relations confine areas of lower basal drag to within a few kilometers upstream of the grounding line (e.g. Schannwell et al., 2018; Brondex et al., 2019). There is however evidence from observations and modelling that areas of low basal drag can extend much farther inland (Joughin et al., 2019).”

Pattyn, F., Perichon, L., Durand, G., Favier, L., Gagliardini, O., Hindmarsh, R., . . . Wilkens, N. (2013). Grounding-line migration in plan-view marine ice-sheet models: Results of the ice2sea MISMIP3d intercomparison. Journal of Glaciology, 59(215), 410-422. doi:10.3189/2013JoG12J129