This large paper explores PISM sensitivities to various parametric, forcing, and boundary condition uncertainties for the AIS glacial cycle context. Aside from a need for consolidation and organizational/editorial work and some missing critical information about the model, for me the underlying weakness stems from the choice of journal. I take the Cryosphere to be about the science, ie understanding the world around us. Models are a tool for this, but in this "cookbook", the model has become the dominant focus.

There are also a few key implicit assumptions that are never justified, eg the choice of only 4 ensemble parameters. Is the relevant uncertainty in the climate forcing over the last 2 glacial cycles for the whole Antarctic ice sheet really reducible to a single parameter? Is the uncertainty in basal drag representation well captured by a single parameter? This is effectively an implicit claim of this paper for which I’m curious to see what kind of justification can be provided (aside from the choice to use an inefficient full factorial sensitivity analysis with its resulting computational limits).

Furthermore, the main reliance on a single metric (ice volume evolution) masks many other potential sensitivities in the model (eg grounding line position for different basins, regional ice thickness at LGM for various ice core sites,...). Though other aspects are at times discussed and a few ice sheet thickness snapshots are shown, without a detailed comparative table of all tested parameters and various metric values, it’s hard to see any clear justification for the chosen parameters.

The submission states that they "identify relevant model parameters and motivate plausible parameter ranges" but I find the approach weak and shallow. I would submit that at the minimum, ensemble parameter selection (in good part by appropriate sensitivity analysis) should show that within observational/proxy uncertainties, the model + ensemble parameters can "bound reality" and capture relevant uncertainties. This is not explicitly done. And given available proxies, the comparative description of "reality" should be much more than just the ice volume time series.

This paper would strongly benefit from some consolidation (few will read this many pages), and a summary table with various metrics comparing the glacial cycle sensitivity to the various possible parameters discussed. This table would help justify the choice of final ensemble parameters.

Wrt paper content/consolidation, as a general rule, think of who your intended audience is. For the Cryosphere, it has to be more than the few dozen of us doing glacial cycle AIS modelling. Include what is relevant to that audience and stick the rest in a supplement for the smaller community who will be interested in all the details (keeps page charges down as well...). This problem is also evident in the overly detailed conclusions section. Again, very few readers are going to care about the detailed sensitivity range of your model setup to your 4 chosen ensemble parameters. That information would be much more usefully presented in a summary table with a more complete set of metrics.

I did like the approach of section 4.3 (ocean temperature forcing is a
challenge), but I’m surprised no discussion is raised about associated uncertainties, assumptions, and limitations. The biggest assumption is that the critical stabilization ratio of mid-depth Antarctic ocean temperature and global mean temperature anomaly from a single datapoint (i.e., from a single model) appropriately reflects the real ocean response.

If the central purpose of this paper is to be a "cookbook for the growing community of PISM users" then I would think GMD would be more appropriate for this paper. This would mitigate some (but not all) of my issues raised here.

This paper would also benefit from more attention to punctuation, appropriate completeness of figure captions, and consistent description of symbols when first introduced. A reviewer is not meant to be a copy editor, so I have only identified example infractions of this in my detailed comments below.

# Specific comments

Coupled climate-ice sheet systems models are computationally too expensive in order to run many long simulations # depends on the complexity of the climate model and what is meant by # "long", cf Bahadory and Tarasov, GMD 2018

parameters need to be constrained and calibrated (Briggs et al., 2014) # that wasn’t a calibration, just a large ensemble analysis (cf # Tarasov et al., EPSL 2012 for more of a sense what a full calibration # entails)

Here we use the non-conserving hydrology model # pretty crude to call this a model ->
# Here we use the non-conserving sub-glacial hydrology parametrization

PISM uses a generalized version of the Lingle-Clark bedrock deformation model (Bueler et al., 2007), assuming an elastic lithosphere, a resistant asthenosphere and a spatially-varying viscous half-space below the elastic plate (Whitehouse, 2018).
# What aspect of the viscous half-space is spatially varying?
# Viscosity, thickness, ...?

The computationally-efficient bed deformation model has been improved to account for changes in the load of the ocean layer around the grounded 85 ice sheet, due to changes in sea-level and ocean depth.
# how is sea-level being computed?

PISM paleo simulations are initiated with a spin-up procedure for prescribed ice sheet geometry, in which the three-dimensional enthalpy field can adjust to mean modern climate boundary conditions over a 200 kyr period.
# given the thermodynamic timescale of the Antarctic ice sheet, it # makes no sense to equilibrate against "mean modern climate boundary # conditions" when that is not the mean boundary condition over the # last 200 kyr.
For consistency reasons with the used PISM version, we use ocean water density here.
# I see no justification for this. This should be fixed (and should be easy to fix).

In fact, a density of 1000 kgm$^{-3}$ should be used instead as ice melts to fresh water.
# Actually, this is not quite correct either given the non-linearity of the equation of state for seawater. But it is a much better approximation than using the nominal density of seawater.

such that the flow law fitting exponent is no fixed physical constant.
# not clear what the intended meaning is here. Do you mean to say that the the effective exponent is empirical since it depends on different processes that have different exponents?

In the model, the same effect is achieved when adjusting the SIA enhancement factor ESIA= 2.0 divided by 50,000 Pa yields $4.0 \times 10^{-5}$ instead
# awkward wording, intent not clear especially since it’s not clear where $4.0 \times 10^{-5}$ came from

However, the simulated ice volume seems to increase by $3\times 10^{-5}$ m SLE for doubling vertical resolution (see red line in Fig. 2), as less temperate ice is formed in the lowest layers of the ice sheet.
# This is disconcerting. Any ideas why? Does the thermodynamic solver have a sub-iteration to ensure the CFL condition is not broken? What kind of switch is used to turn on basal sliding?

SIA enhancement generally produces thicker grounded ice.
# -> thinner

Hence, ice at the calving front thinner than 75m is removed.
# Is this condition imposed during each ice dynamic timestep? Or when precisely?

figure 4
# captions should explain any non-obvious figure keys (eg "no oceankill")

# General figures: the red and orange colours will be hard to differentiate by anyone with weak eyesight. Please choose a stronger contrasting colour and/or add textures.

We have shown that sea-level changes drive grounding line migration as have many others. And with no citation, should only state "we show below"

In fact sea-level changes at the grounding 220 line are not only caused by global mean sea-surface height change but also by local changes in the sea floor and bed topography.
# incorrect, global mean sea-surface height change -> local sea-surface height change
but also by local changes in the
sea floor and bed topography
# what is the difference between sea floor and bed topography? -> bed
topography

The formulation closely
approximates the approach used within many GIA models (Whitehouse, 2018), which
are defined to
230 account for the response of the solid Earth and the global gravity field to
changes in the ice and water
distribution on the entire Earth's surface (Whitehouse et al., 2019).
# Provide a citation to support claim that ignoring geoidal spatial
# variations and use of half-space approximation gives a "close
# approximation" to full solution of sealevel equation with a full
# visco-elastic model with radially varying viscosity or otherwise
# this drop claim. Also, be more precise than "closely
# approximates". What does that really mean?

account for vertical displacement
# -> account for vertical bed displacement

we presented simulations
# -> we present simulations

We have deactivated
the elastic part of the Earth model in our reference simulation, as the numerical
implementation was
flawed. Instead we have used PISM v1.1, which considers only grounded ice thickness changes as
loads, with additionally fixed elastic part3, in order to evaluate the ice sheet
volume's sensitivity to
265 changes in the flexural rigidity parameter value
# Now I'm not clear what exact GIA model is used. You first claim to
# include changing ocean load but here you state that you do not.

with \( fp=7\% / K \) a precipitation change factor with temperature
# from Clausius-Clapeyron or?

# is there a bed thermodynamic model or not? If so, please detail. If
# not, justify why not included

In our PISM simulations the Mohr-Coulomb criterion (Cuffey and Paterson, 2010) deter
385 mines the
yield stress \( c \) as a function of small-scale till material properties and of the
effective pressure \( N_{til} \)
# I'm confused, previously you state that the basal drag exponent \( q \) is
# an ensemble parameter, but yield stress is only meaningful for \( q=0 \)
# (Coulomb plastic) basal drag.

\textbf{eq 9}
# what do the constants \( \delta, \epsilon_0, C_c \) represent?
The effective pressure cannot exceed the overburden pressure, i.e., \( N_{\text{max}}^{\text{til}} = P_0 \) (for details see Bueler and van Pelt, 2015, Sect. 475 3.2),
# this follows by definition, so I don’t understand why references are 
# provided.
we find a lower limit
\( N_{\text{min}}^{\text{til}} = \delta P_0 \),
# So would anyone "find". This directly follows from eq 9.

In particular the so-called meltwater pulse 1a
# remove "so-called". Or do you want to start stating "so-called Last 
# Glacial Maximum"....?

As WDC temperature rise occurred somewhat earlier than at EDC the Antarctic Ice Sheet responds
with higher deglaciation rate (cf. grey in blue line).
# readability of this paper would benefit from more punctuation

4.3 Ocean temperature forcing
# this section would really benefit from a comparative repeat of the 
# analysis with the ocean temperature results from the TRACE (Liu and 
# Otto-Bleisner) deglaciation GCM model run that are freely available.

We hence choose \( ^\text{AM-^@M-^XPREC^^M-^@M-^Y} \) as relevant climate forcing
# what is PREC? Not shown in any provided equation

# later page:
The simulations hence suggest that the precipitation scaling parameter \( fp \) is highly relevant for the ice sheet\( ^\text{AM-^@M-^Ys} \) extent
at glacial maximum and will be
considered as ensemble parameter \( ^\text{AM-^@M-^XPREC^^M-^@M-^Y} \) in Albrecht et al. (2019 ).
# repeat of early, but now you explain what PREC is. Please clean up 
# paper organization.

5.1 Energy spin-up procedure and intrinsic mem
# I can’t interpret your spin-up experiments without knowing what kind 
# of bed thermodynamics is implemented though I suspect you have none 
# given that a full (eg 3-5 km deep) bed thermodynamics components 
# would likely show more sensitivity to the spinup climate forcing.

As the three-dimensional enthalpy field carries the memory
of past climate conditions, a more realistic spin-up climatic boundary condition
may be achieved
when the temperature reconstruction of the previous glacial cycles
# "may be" -> "would be"

A timeseries of well-dated sediment data of iceberg-rafted debris (Weber et al.,
2014) suggest that
the main retreat of the Antarctic Ice Sheet occurred 14.6 kyr BP, as a consequen-
# The RAISED consortium of glacial geologists concluded otherwise 
# (Bentley et al, QSR 2014) so this inference remains an open question 
# and this should be made clear.

we find for geothermal heat flux maps from different
available source comparably little difference in modeled LGM ice volume, in contrast to previous studies.

835 and air temperature PISM-PICO simulates similar LGM states. However, the onset of deglaciation and hence present-day ice volume can differ by a few meters SLE. This means that ocean temperature forcing is of minor relevance for glacial cycle simulations.

You seem to forgetting about the Eemian, where sub-shelf melt may play a critical role in partial to near complete WAIS collapse or some such which is what is inferred to be required to explain the sealevel high-stand then.

From the discussed model settings and boundary conditions we select four relevant parameters representative for each of the different sections.

given all the uncertainties in the physics and forcing of the glacial cycle AIS along with the size of the AIS, I’m surprised you only choose 4 ensemble parameters, with no justification for such a small size.