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
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 (ie 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 comunity 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 introduce. 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 (B
ueler et al.,
2007), assuming an elastic lithosphere, a resistant asthenosphere and a spatiall
y-varying viscous halfspace
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 ar
ound 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 bou
ndary 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 h
ere
I see no justification for this. This should be fixed (and should be
easy to fix).

 In fact, a density of 1000 kgmâM−^HM−^R3 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 Â• 10âM−^HM−^R5
 instead
awkward wording, intent not clear especially since it's not clear
where 4.0 Â• 10âM−^HM−^R5 came from

However,
the simulated ice volume seems to increase by 3âM−^@M−^S5m SLE for doubling vert
ical 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 enhancemnet 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 b
y local changes in the
sea floor and bed topography.
incorrect, global mean sea-surface height change -> local sea-surface height c
hange

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 EarthâM-^@M-^Ys 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 numerica
l implementation was
flawed. Instead we have used PISM v1.1, which considers only grounded ice thickn
ess changes as
loads, with additionally fixed elastic part3, in order to evaluate the ice sheet
 volumeâM-^@M-^Ys 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) d
etermines the
yield stress c as a function of small-scale till material properties and of the
effective pressure Ntil
385 on the saturated till
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
(Coloumb plastic) basal drag.

eq 9
what do the constants \delta, \epsilon_o, C_c represent?

The effective pressure cannot exceed the overburden pressure, i.e., Nmax
til = P0 (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
Nmintil = \delta P0,
# 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 avaiable.

We hence choose âM-^@M-^XPRECâM-^@M-^Y as relevant climate forcing
what is PREC? Not shown in any provided equation

later page:
The simulations hence suggest that the precipita660
tion scaling parameter fp is highly relevant for the ice sheetâM-^@M-^Ys extent
at glacial maximum and will be
considered as ensemble parameter âM-^@M-^XPRECâM-^@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
ce of MWP1a
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 cont
rast to previous
815 studies
Again I can't evaluate this without knowing what kind of bed thermal
model is used.

835 and air temperature PISM-PICO simulates similar LGM states. However, the ons
et of deglaciation
and hence present-day ice volume can differ by a few meters SLE. This means that
, compared to the
other forcings, ocean temperature forcing is of minor relevance for glacial cycl
e 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 relevan
t parameters
845 representative for each of the different sections

misleading wording. I take it you mean "we select a total of four
relevant parameters, one each for the 4 different sections" but I'm
not clear what 4 sections you are talking about. Be explicit.

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.