Interactive comment on "Glacial cycles simulation of the Antarctic Ice Sheet with PISM – Part 1: Boundary conditions and climatic forcing" by Torsten Albrecht et al.

Response to Referee Lev Tarasov (lev@mun.ca)

(Received and published: 4 July 2019)

We thank Lev Tarasov for his helpful suggestions and very detailed and helpful review. Find in **blue** the referee's comments in in **black** the author's response.

This large paper explores PISM sensitivitities 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.

We understand the reviewer's suggestion to consider a more model-focussed journal such as GMD instead of The Cryosphere (TC) and we agree that the model sensitivity has received the dominant focus in this study. Although, we actually learn many new things from this study, for instance when we compare individual and combined effects of climatic and sea-level forcing. Also previous glacial model descriptions have been published in TC with quite some impact (e.g. Briggs et al. (2013), Winkelmann et al. (2011)). We believe that TC is the perfect journal to reach the growing scientific community, who uses the PISM for various applications to actually gain a better understanding of the cryosphere within the climate system.

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).

The reviewer raises an important limitation of the ensemble design regarding the reduced selection of relevant parameters, which we have now discussed in much more detail in the companion paper in Albrecht et al. (2019b). Our first intension was to allow for a close comparison to the (full factorial sampling) ensemble analyses by Pollard et al. (2016, 2017). Yet, they used a different model and different model parameterizations and varied mainly oceanic and solid Earth parameters.
In our study, the parameter selection criteria are mainly the sensitivity of both present-day and LGM ice volume to parameter change (we made that clearer in the manuscript). Another aspect was to have one representing parameter for each uncertainty class, as well as a balanced representation of the two Antarctic parts (WAIS and EAIS). Regarding climate forcing, we showed that the parameter PREC fulfilled best the selection criteria,

although other climate-related parameters may also have a relevant impact, i.e. for deglaciation. For the basal sliding parameterization we find the largest uncertainties. In fact, we could have additionally tested different paramterizations implying an even larger uncertainty. Pollard et al. (2016, 2017) found the sliding coefficent underneath modern ice shelves to be the most relevant of their ensemble paramters. We have run an additional basal ensemble, as discussed in the Appendix A of the companion paper (Albrecht et al., 2019b) and found for variation of a similar till parameter on the continental shelf a much stronger variability in the aggrgated scores, than for the PPQ parameters. However, to what extent the one selected paramter PPQ can represent the uncertainty of the basal processes was not so clear when running the sensitivity experiments presented here. This is something we can learn from the ensemble analysis (including paleo data scoring), in particular when taking into account a more refined parameter space.

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 usage of a single aggregated metric provides certainly a very limited view. But in order to avoid confusion and regarding the number of uncertain model parameters, we decided in part 1 to compare the modeled ice volume sensitivity with a reference simulation (with focus on present-day and LGM states), which provides a first one-dimensional estimate of parameter relevance. This is an excellent suggestion to add a comparative table for the main selection criteria, we used (see Table 2 with means and standard deviations in revised manuscript). However, using statistical means over a few samples can provide only a rough comparison of parameter sensitivity. For this relatively limited approach we have at least a chance to attribute anomalies to a range of physical parameter effects. In part 2 (Albrecht et al., 2019b), we actually do a second step and consider the combined affects of parameter variation on the Antarctic ice volume history, but with a closer look to certain key regions.

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.

We understand that this sentence suggests to find „all“ relevant parameters. We make this clearer in the manuscript, that we select one representative parameter for each group of uncertainty, namely climate forcing, basal sliding, as well as ice and Earth dynamics, such that the selection criteria with respect to the Antarctic ice volume are hit. From the sensitivity analysis we can infer a best guess of the individual parameter range, such that the modern observed ice volume can be re-produced within some uncertainty. For the ensemble, however, we chose the parametric range large enough to account for possible effects of parameter interaction. We reformulated the last paragraph of the abstract:

„For each of the different model uncertainty groups with regard to climatic forcing, ice and Earth dynamics and basal processes, we select one representative model parameter that captures relevant uncertainties and motivate corresponding parameter ranges that bound the observed ice volume at present. The four selected parameters are systematically varied in a parameter ensemble analysis, which is described in a companion paper.“

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.

As already stated above, we very much apreciate the reviewer's suggestion for a parameter sensitivity range overview table and added such a table to the revised manuscript. We also consolidated the main manuscript content, such that it should be more suitable for a broader range of TC readers.

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.

This is certainly a good point, and we added some more details to the manuscript.

„A comparison to reconstructions with a GCM in the TraCE-21ka project (Liu et al., 2009) shows that short warming periods above present level can occur at intermediate depth, e.g. during ACR around 14 kyr BP, which can not be adequately resolved with our approach. The GCM ocean data are bounded below by the pressure melting point... The here presented parameterization assumes that ocean water masses at depth below 500 m can access ice shelf cavities and induce melting, which is certainly very simplified regarding the complex topography flow patterns around Antarctica. Also, we used data from a simplified sensitivity experiment with ECHAM5/MPIOM, for a much warmer than present climate, which implies various model uncertainties. We had to make assumptions about a suitable response function, which is fitted to model data that are averaged over certain regions and ocean depths, implying further uncertainties.“

However, the glacial-interglacial ice sheet volume response in our experiments turns out to be not very sensitive to the actual choice of the ocean temperature forcing, which might be different with a closer look into the deglaciation or warmer-than-present periods.

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.

We have discussed the preference for the TC journal already above.

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.

We did not mean to upset the reviewer with our oversight mistakes. We went through the manuscript and double-checked symbols, figure captions and formulations.

specific comments

l.26: „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

Definitely true, we were thinking of GCM complexity, but we are happy to emphasize this option in combination with EMICs:

„Coupled climate-ice sheet systems models ***can be*** *~~are~~ computationally ~~too~~ expensive in order to run ~~many long~~* ***hundreds of full glacial-cycle*** *simulations****, depending on their complexity (e.g., Bahadory and Tarasov, 2018, using a model of intermediate complexity with about 1 kyr per day on one core)****.“*

l.34: „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)

Yes, we replaced „and calibrated“ by *„with observational data “*.

l.75: „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

Ok. It is actually the off-mode of the implemented mass-conserving sub-glacial (routing) hydrology model in PISM (Bueler & van Pelt, 2015), but we agree to call this mode „parameterization“ here.

l. 81: „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 halfspace below the elastic plate (Whitehouse, 2018).“
What aspect of the viscous half−space is spatially varying? Viscosity, thickness, ...?

In deed, this seems to be wrong and has been changed to:

„PISM uses a ***modified*** *~~generalized~~ version of the Lingle-Clark bedrock deformation model (Bueler et al., 2007), assuming an elastic lithosphere* ***and*** *a resistant asthenosphere ~~and a spatially-varying~~* ***with*** *viscous* ***flow in the*** *half-space below the elastic plate (Whitehouse, 2018).“*

l.83: „The computationally−efficient bed deformation model has been improved to account for changes in the load of the ocean layer around the grounded ice sheet, due to changes in sea−level and ocean depth.“
how is sea−level being computed?

We use the global mean sea-level height as forcing, which affects the ice via the flotation crierion and hence the grounding line position. In order to account for changes in ocean load, we compute the ocean layer thickness with respect to changes in bed topography and global mean sea-level stand. There is no self-consistent sea-level equation solved.

We added *„**global mean** sea-level **height**“* to the mansucript to avoid false expectations.

l. 90: „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.

Yes, this has not been the best model choice. But this aspect is covered in Sect. 5.1: „Energy spin-up procedure and intrinsic memory“ (now moved to Sect. 1.3 in the revised manuscript). The difference for the ice volume reconstruction, however, is within the „intrinsic“ uncertainty of up to 1m SLE.

l. 110: „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).

Yes, this change has been already merged into the development branch of PISM (https://github.com/pism/pism/issues/412) for the PISM output, but as the difference in sea-level equivalent ice volume is less than 2.4%, we did not want to throw away previous simulations results.

l. 111: „In fact, a density of 1000 kg m^-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.

Yes, we have already had the same thoughts, and found a difference by using the non-linear equation of state vs. adding salinity fluxes (V*S) of less than 2e-4 psu, when the entire Antarctic Ice Sheet were melted to the ocean. (http://fermi.jhuapl.edu/denscalc.html). We added to the manuscript: *„... (which is a good approximation of the equation of state of the freshened ocean water).“*

l. 122: „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?

No, we wanted to say that **„*n* comes with significant uncertainties“** and is not a fundamental universal physical constant. We rephrased this accordingly.

l. 131: „In the model, the same effect is achieved when adjusting the SIA enhancement factor ESIA= 2.0 divided by 50,000 Pa yields 4.0x10^-5 instead“
awkward wording, intent not clear especially since it's not clear where 4.0x10^-5 came from

We have omitted this sentence as it describes just a technical workaround how A=A(n=4) can be simply adjusted in PISM.

l. 162: „However, the simulated ice volume seems to increase by 3-5 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?

There is no switch in PISM, as SSA stress balance is calculated in the entire ice domain as a sliding law for given basal shear stress. Hence we have basal sliding everywhere (Bueler & Brown, 2009). For the advection-conduction-reaction problem of the conservation of energy within the ice domain, PISM uses a BOMBPROOF numerial scheme (https://pism-docs.org/sphinx/technical/bombproof.html), which is conditionally-stable according to the CFL criterion, which is included in the PISM adaptive timestepping technique. Truncation error is O(Δz^2). But it is a known fact that vertical resolution of O(1m) seems to be required for capturing temperate ice adequately in the enthalpy model (Kleiner et al., 2015). The implementation of the conservation of energy in PISM will be overhauled by next year with a vertical coordinate system that will improve accuracy with much higher resolution at the surface and the base of the ice sheet.

l. 179: „SIA enhancemnet generally produces thicker grounded ice.“
−> thinner

True, this has been changed accordingly.

l. 207: „Hence, ice at the calving front thinner than 75m is removed“
Is this condition imposed during each ice dynamic timestep? Or when precisely ?

Calving in PISM is generally applied in each timestep after the mass-transport is applied, but before the surface and basal mass balance terms are calculated. The removal of thin ice tongues of less than 75m is rather for technical (SSA convergence) than for physical reasons.

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

Yes, this is a PISM-specific term of a model option. We have renamed it to "no deep-ocean-calv" and defined it in both the text and figure caption.

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.

OK, we used the d3 categorial 10 color scheme, and switched to categorial 20 and updated all figures accodingly (https://github.com/d3/d3-3.x-api-reference/blob/master/Ordinal-Scales.md#category20). This implies more light/dark graduation and a smaller number of different colors (i.e. no red – orange, or red – green combination).

l. 218: „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"

Well, this may be a relict of reordering the manuscript and has be changed accordingly.

l. 219: „In fact sea−level changes at the grounding 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 c hange

Yes, the local sea-level is what matters at the grounding line, and we changed this accordingly. But the global mean sea-surface height is the driving force that is applied to the whole model domain uniformly.

l. 220: „but also by local changes in the sea floor and bed topography“
what is the difference between sea floor and bed topography? −> bed topography

„Sea floor“ was used here to distinguish between ocean and ice load region, but we omitted it according to the reviewer's suggestion.

l. 228: „The formulation closely approximates the approach used within many GIA models (Whitehouse, 2018), which are defined to 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?

We agree that this formulations was somewhat misleading. We actually have compared the simulated vertical bed displacement for an Antarctic deglaciation scenario (which is similar to the reference simulation) with results of the GIA model used by Pippa Whitehouse, who co-authored a previous PISM study. We rephrased as follows:

„The Earth model can be initilized with a present-day uplift map (Whitehouse et al., 2012) ***and reproduces plausible uplift pattern and magnitudes for a given load history (Kingslake et al., 2018, personal communication Pippa Whitehouse). Yet, it is still a simplification of the*** *approach used within many GIA models (Whitehouse, 2018), which are defined to 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).“*

l. 232: „account for vertical displacement“
−> account for vertical bed displacement

Added.

l. 251: „we presented simulations“
−> we present simulations

Changed.

l. 261: „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 part, in order to evaluate the ice sheet volume's sensitivity to 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.

There are several components in the GIA model, which have been changed since the last PISM version. First, the elastic part has been fixed in PISM v.1.1 (https://github.com/pism/pism/pull/435). As we used an older PISM version for the experiments, we ran only the elastic sensitivity experiment with the newer PISM version. Second, for the viscous part, PISM has accidently used ice thickness (including floating ice) as load in the GIA model. That has been fixed for the presented experiments, such that both changes in grounded ice thickness and ocean thickness are considered as loads. Only the grounded ice part of this fix entered into the stable PISM v1.1 (https://github.com/pism/pism/commit/4b5e14037), as is was obviously wrong, while the ocean load part still requires proper numerical tests cases before it can be merged to the main PISM development branch. We reordered this paragraphin the manuscript:

„In order to evaluate the ice sheet volume's sensitivity to changes in the flexural rigidity parameter value, we have used PISM v1.1 instead, with additionally fixed elastic part. Yet, PISM v1.1 considers considers only grounded ice thickness changes as loads, and not the ocean thickness in the reference*.“*

l. 329: „with fp=7%/K a precipitation change factor with temperature“
from Clausius−Clapeyron or?

Right, has been added to the revised manuscript: *„...with f_p=7%/K a precipitation change factor* ***according to Clausius-Clapeyron relationship*** *with temperature...“*

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

Yes, there is a bed thermal unit, see details below.

l. 383: „In our PISM simulations the Mohr−Coulomb criterion (Cuffey and Paterson, 2010) determines the yield stress c as a function of small−scale till material properties and of the effective pressure Ntil 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.

The basal drag exponent q is an (ensemble) parameter in the generalized „pseudo-plastic" sliding relationship (Eq. 7), in which τ_c is called the Mohr-Coulomb yield stress (Eq. 8), and here actually valid for all exponents $0 \leq q \leq 1$. This implies that basal shear stress can be larger than the Coulomb yield stress in fast-flowing regions ($u > u_{thr}$) for $q > 0$, in contrast to purely plastic sliding for $q = 0$, when $|\tau_b| \leq \tau_c$ and $|u| > 0$. In fact, purely plastic behavior of till must be assumed to derive τ_c values from in situ measurements.

„In our PISM simulations the Mohr-Coulomb failure criterion (Cuffey and Paterson, 2010) determines the yield stress τ_c ***in Eq. (7) (valid for*** $0 \leq q \leq 1$ ***)**..."*

l. 450: eq 9
what do the constants \delta, \epsilon_o, C_c represent?

*„...while all other parameters are constants (adopted from Bueler and van Pelt, 2015**, see Table 1 for parameter meaning and values**)."*

l. 474: 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.

Omitted reference.

l. 475: „we find a lower limit Nmintil = \delta P0,"
So would anyone "find". This directly follows from eq 9.

Modified to: *„..., while in the case of saturated till layer (s=1)* ***Eq. (9) yields*** *a lower limit..."*

l. 512: In particular the so−called meltwater pulse 1a
remove "so−called". Or do you want to start stating "so−called Last Glacial Maximum"....?

Omitted.

l. 543: „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

Yes, we addressed this in the revised manuscript and separate each message into one sentence with actual numbers:

*„**In our simulations, the Antarctic Ice Sheet volume responds with several thousand years delay to the surface temperature forcing. The LGM minimum in surface temperatures reconstructions happened around 22kyr BP in the WDC data, while largest ice volume is simulated at 14kyr BP. The main temperature rise at WDC occurred between 18kyr and 12kyr BP contributing to initial deglaciation at around 12kyr BP with major retreat between 8kyr and 4kyr BP. At EDC location the reconstructed temperature rise happened about 1kyr later with more variability leading to a more gradual deglaciation between 12kyr and 2kyr BP.** Comparisons with other ice core temperature reconstructions, however, suggest a superimposed **lapse rate** effect ~~of~~ due to surface height change during deglaciation at WDC **location** (Werner et al., 2018).“*

l. 552: „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.

This idea seems great, as the response function analysis in Sect. 4.3 has only been applied to warmer than present conditions. For comparison, we have plotted the WDC temperature anomaly and associated ocean temperature anomaly estimate in intermediate water depth against ocean temperature data (mean over regions south of 66°S) from the TraCE-21ka deglaciation GCM (https://www.earthsystemgrid.org/project/trace.html), and added the corresponding graph to Figs. 22 and 30.
Generally, we find in the GCM data similar response trough the Holocene period as in our parameterization, while the temperature dip at ACR is much more pronounced in the GCM data (Fig. R1). For colder glacial conditions another discrepancy arises, as ocean temperatures in the data are bounded below by pressure melting temperature (in Fig. R1 for anomaly -1.8C). The PICO module can handle such ocean temperature forcing for colder than present climates and bounds ocean temperatures in each basin below by pressure meltung temperature.

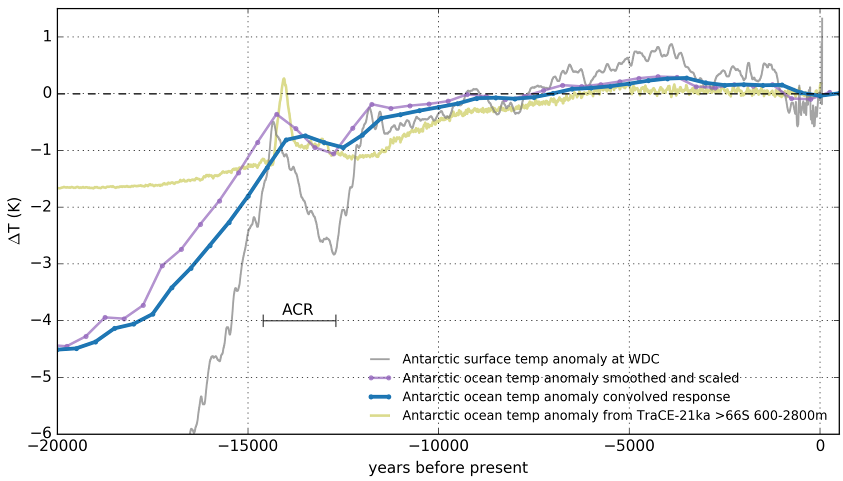

Fig. R1: Surface temperature anomaly over the last 22 kyr as reconstructed from WAIS Divide Core (WDC, grey) and indermediate depth ocean response estimate (blue) as described in Sect. 4.3, compared to scaled timeseries (purple) and to TraCE-21ka (Liu et al., 2009; https://www.earthsystemgrid.org/project/trace.html) ocean model anomaly between 600m and 2800m depth (olive).

We have also performed the response function analysis with these TraCE-21ka data, but we could not find a meaningful response function, as the analysis is suited to step forcing experiments. But we were able to calculate the convolved ocean response estimate from TraCE-21ka global mean surface temperature (with the previous fitting paramters), which is similar to the one derived from WDC reconstructions, and which can also not adequately resolve the variability around ACR (1K warming followed by 1K cooling), at least for 500 yr bin size (Fig. R2).

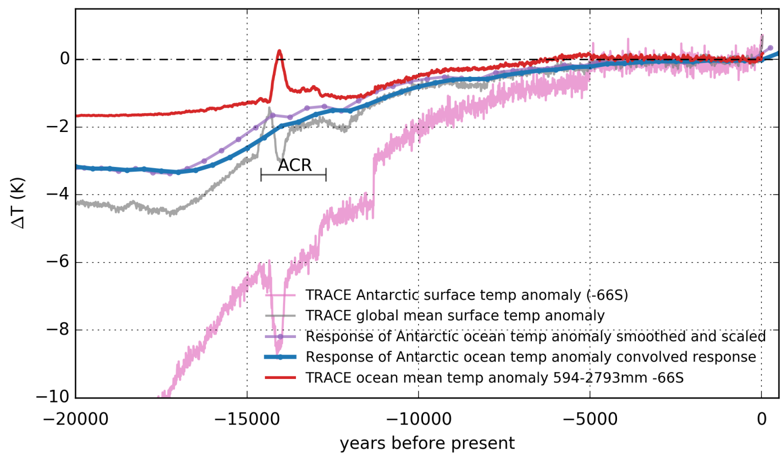

Fig. R2: TraCE simulated surface temperature anomaly over the last 20 kyr as global mean (grey) and mean over the Antarctic region south of 66S latitude (rose), as well as ocean model anomaly between 600m and 2800m depth (red, Liu et al., 2009). In violet the smoothed (500yr bins) and scaled timeseries (factor 0.75) as well as the estimated ocean response ($1/x^2$) in blue.

l. 635: „We hence choose PREC as relevant climate forcing“
what is PREC? Not shown in any provided equation
later page:
l. 659: „The simulations hence suggest that the precipitation scaling parameter fp is highly relevant for the ice sheet's extent at glacial maximum and will be considered as ensemble parameter PREC in Albrecht et al. (2019).“
repeat of early, but now you explain what PREC is. Please clean up paper organization.

Thanks for pointing out this inconsistent declaration of this key parameter. We switched the two sentences.

l. 663: „5.1 Energy spin−up procedure and intrinsic memory“
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.

„PISM uses a bedrock thermal model (1-D heat equation with bedrock thermal conductivity of 3.0 W m-1 K-1, bedrock thermal density = 3300 kg m^-3 and bedrock thermal specific heat capacity = 1000.0 J / (kg K)), similar to Ritz et al., 1996, with upper lithosphere thickness of 2 km discretized in 20 equidistant layers and geothermal heatflux applied as constant boundary condition to calculate the heatflux into the ice at the ice-bedrock interface depending on ice base temperature.“ We added this information to the revised mansucript and the following Fig. R3:

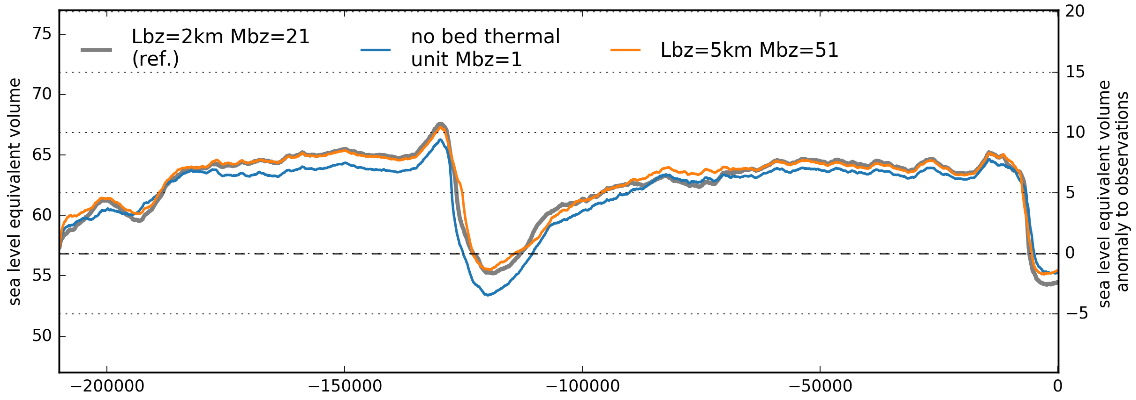

Fig. R3: Timeseries of sea-level relevant ice volume for different bedrock thermal layer thicknesses (Lbz), with 2km with 100m vertical resolution in the reference (grey). In comparison, a 5km thick thermal bedrock layer (orange) has only little influence on the ice volume history, while in the absence of a bed thermal unit (blue) ice volume tends to be 1-2 m SLE smaller.

l. 665: „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"

Changed.

l. 710: „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 consequence 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.

*„**It remains an open question how much Antarctic deglaciation contributed to the the MWP1a.** 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 at 14.6 kyr BP, ~~as a consequence of~~ **synchronously with** MWP1a**, while the RAISED Consortium concluded on an a later retreat with a relatively small Antarctic contribution to MWP1a (Bentley et al., 2014).** „*

l. 813: „we find for geothermal heat flux maps from different available sources comparably little difference in modeled LGM ice volume, in contrast to previous studies“
Again I can’t evaluate this without knowing what kind of bed thermal model is used.

See discussion above.

l. 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, compared to the other forcings, 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.

We agree with the referee that ocean forcing may have played a critical role in major ice volume changes during the Last interglacial. However, in this paragraph of the conclusions we refer to Fig. 23 and hence to the sensitivity of ice volume history to chosen temperature reconstructions and involved parameters. As WDC temperature reconstuction covers only the last 67 kyr we find consequently no differences in the ice volume response before that time, except for the (60%) scaled time series. We changed the manuscript to:

*„However, the onset of deglaciation and hence present-day ice volume can differ by a few meters SLE. This means that, compared to the other forcings, **we find low sensitivity of the ice volume history to the selection of** ocean temperature forcing **in our** ~~is of minor relevance for~~ glacial cycle simulations. Hence, we have not varied PICO parameters in this study, **although ocean forcing in general may play a key role for ice sheet retreat during interglacials.***

In fact, in our simulations (also in the ensemble) we do not find (partial nor complete) WAIS collapse during the Last Interglacial. This is most likely related to the basal friction paramterization, as the optimization algorithm for the till friction angle (Sect. 3.4.2) suggest values of up to 25° in the deepest marine sections of WAIS, which corresponds to relatively high yield stress of the till and hence thicker ice in this region (in agreement with present-day data), which seems to prevent from collapse. In constrast, till friction optimization suggests angles of 1° or 2° in ice stream regions to allow for sufficient ice flux, e.g. in Siple Coast. Other PISM studies (e.g. Golledge et al., 2015; Feldmann & Levermann, 2015; Sutter et al., 2016; Feldmann et al., 2019) may find more sensitivity in WAIS to enhanced ocean melt by using 4-5° till fricition angle in the entire marine sections of WAIS. But also basal melt interpolation and availablity of till water at the grounding line may have enhanced grounding line sensitivity in those experiments.

l. 844: „From the discussed model settings and boundary conditions we select four relevant parameters 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.

We corrected for the misleading wording accordingly by adding:
„...one each for the different sections (Sect. 2: Ice sheet and Earth model parameters, Sect. 3: Boundary conditions and input datasets and Sect. 4: Climatic Forcing).“

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