Dear Editor,

We are grateful for the possibility to further improve our manuscript. This will not require any additional simulations, as we have already performed the pre-industrial simulation with fixed present-day ice sheets using the Coupled MITgcm Setup. We only need to include additional diagnostics beyond those already presented in the previous version.

Following your advice, in the revised version we have:

1) shortened the evaluation section by moving the description of the simulations in a new section entitled `Experimental setup', as suggested by the reviewer, combining some figures, and moving others in the Appendix.

2) addressed all reviewer comments.

3) removed any reference to tipping behavior. Our simulation setup is useful for identifying attractors and the boundary regions where they become unstable (sometimes referred to as `tipping points'). In the present version, we focus on this aspect without explicitly referring to tipping behavior. This required adjustments to the abstract and introduction, that we have revised and improved.

We have also renamed the simulation tool `Biogeodynamics-Ice sheet-Geneva MITgcm' (BIG-MITgcm for short). The short acronym `BIG' also reflects the addition of new components to MITgcm, which remains the dynamical core of the simulation framework.

Please find below a detailed point-by-point response to the comments of the second reviewer.

Sincerely,

On behalf of all authors

Laure Moinat and Maura Brunetti

REVIEWER 1

For final publication, the manuscript should be accepted as is.

REVIEWER 2

For final publication, the manuscript should be reconsidered after major revisions.

Suggestions for revision:

Re-review of "biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploring climate steady states with a new global-scale ice sheet model" by Moinat et al.

Overall comments
Having reviewed an earlier version of this manuscript, I can confirm that important updates have been implemented and the text has been considerably improved. I particularly appreciate the inclusion of the Permian-Triassic test case. Nevertheless, I find a large number of remaining issues that I think need to be addressed to bring the paper to a point where it can be published in GMD.

The most important point is how the new couplings influence the behaviour of MITgcm (see my comment l382-461). A detailed comparison should be included for the pre-industrial. Results in Table 3 for the Permian-Triassic may indicate limited influence. If this is also the case for the pre-industrial, it could be that the whole 6-page comparison l382-461 is basically a repetition of MITgcm validation exercises already done before. Otherwise, the comparison should focus on differences between biogeodyn-MITgcmIS and MITgcm.

We thank the reviewer for their positive feedback on the revised version.

We already added a comparison between our model (run1, 280 ppm, started from the bedrock topography, i.e. zero ice thickness) and a simulation using the Coupled MITgcm Setup (started from ETPO2 topography, i.e.

including present-day ice sheets). This was mentioned in Sec. 3.2.4 (first paragraph) and Fig. A6 (old numbering). We agree that we can include more diagnostics on this comparison (for example, additional curves in the zonal profiles, as we will detail below). Overall, this comparison shows that our model (now renamed BIG-MITgcm) is able to spontaneously grow plausible ice sheets, since differences in both the resulting climate and ice sheet volume/extent are marginal in the two simulations.

From now on, we refer to the marked revised version for line, section and figure numbering in our response.

Please see further detailed comments below.

L1
Suggest "Modelling the climate system on multi-millennial timescales" to be clear about the timescale of interest from the start.
We agree with this suggestion.

L8 "we added asynchronous couplings with a vegetation model (BIOME4), a hydrological model (pysheds)"
According to Ragon 2024, coupling with BIOME4 already exists. Be clear about what is new in the current work.
Here, the coupling with vegetation is included in a more complex numerical protocol. We explicitly mentioned this BIOME4-MITgcm coupling performed in Ragon et al. 2024, 2025 at L327, and now also at L167.

l13 "initialized from bedrock topography" is confusing. Maybe "initialized with zero ice thickness".
We have reformulated as `initialised from observations of bedrock topography''

l16 "we discuss also a Permian-Triassic simulation."
Add a brief summary of the results in the abstract.
We have added a brief description.

l19-33
I am still not convinced about opening the introduction with focus on tipping. I think a better beginning would build on material l33- "We are interested in a modeling setup ..."
We have eliminated references to tipping dynamics.

l40 "coarse spatial resolutions and simplified parameterizations"
At least the first part arguably also applies to your model. Maybe "reduced complexity and simplified parameterizations".
We prefer to specify the resolution explicitly to avoid a repetition of `complexity'

l42 "Nowadays, there are few CMIP6 models with interactive ice sheets"
Either open reference list with "e.g." or include all CMIP6 models that have interactive ice sheets (MPI-ESM, NorESM). You should also cite https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2021PA004272, which uses CESM2 with an asynchronous coupling to BIOME4.
We have included `e.g.', and included the suggested reference as an example of asynchronous coupling.

l49- "Here, we propose a simulation tool, called biogeodyn-MITgcmIS"
Why not list vegetation and ice sheets already in this first sentence?
Good point, we have included ice sheets (vegetation was already mentioned).

l87 "KPP scheme"
What does KPP stand for?
The K-profile parameterization (KPP) scheme is typically used in low-resolution ocean models to describe upper ocean turbulent mixing. We have eliminated the acronym.

l108 "a smoothing procedure is needed"
This calls for a reference or further explanation of the smoothing procedure.
We have reformulated as `interpolation and smoothing are needed' and we refer to Ragon 2024.

l114 "surface air temperature (SAT)"
Is this the 2m air temperature?
Yes, we now specify this.

l114 "Additional inputs are soil depth and texture"
Not clear where these come from. Explain.
These values are generally taken from present-day soil characteristics, as specified at L150.

l117 "The albedo"
Is this the same as "bare-surface albedos" on line 92? Clarify (singular vs plural?).
Yes, and it is indeed plural, corresponding to different biomes.

l120 "BIOME4 follows the principle ..."
You may consider moving this general model description before information about the re-coupling "The albedo and the vegetation map obtained as outputs ..."
We prefer to keep like this to avoid repetition on required input/outputs.

l142 "spatial resolutions of around 2◦"
Not quite the same as 2.8◦. Maybe "Since we are interested in simulating ice sheet dynamics at relatively coarse spatial resolution"
We said `around 2◦ or coarser' since we aim to increase the resolution of our simulations in the future.

l168
Maybe "Ice deformation can be simplified by neglecting basal sliding, by considering that the only important type of deformation is vertical shearing, and by assuming a power-law shear thinning viscous rheology"
We have reformulated as suggested.

Equ. 4
I think gradient(Z_B) is not defined.
zB is defined after eq. (1)

l201 "Ta is the temperature at z= h"
Is this the same SAT in l114? Make consistent.
Yes, we have now specified this.

l208 "temperature Ta at h= 2 m"
Is this the same as SAT?
Remove "surface" to remove ambiguity with specification 2m.
"The required input is the air temperature Ta at h= 2 m"
We have followed this suggestion.

l210 "During each model iteration"
What is the time step between iterations? It does not make sense to include the lapse rate update on a daily basis.
We describe in detail the coupling procedure in Sec. 2.6. The lapse rate is not calculated on a daily basis, but using averages over 30 yr from the previous iteration.

L214 "MITgcm land module does not include accurate snow physics"
I miss an explanation here or elsewhere how snow and ice albedo or handled in the coupled setup.
We specify how the accumulation rate is estimated from snow precipitation (which is a diagnostics in MITgcm) at L245-249.

l217
"to obtain the accumulation rate in [m s−1 ice equivalent].
We have included this in the revised version.

l218 "the surface air temperature"
Rather "the 2 m air temperature"
We have included this in the revised version.

l220
Already mention briefly here what happens with rain over the ice sheet? Since this is not part of the surface

mass balance (as opposed to some PDD approaches where rain enters potential refreezing in the firn layer), how is it handled further (routing, combined with runoff, ...)? Extended description should follow in section 2.5.
Rain over the ice sheet is not included in the surface mass balance. It is handled by the runoff routing scheme described in Sec. 2.5, as we now specify at L251.

l223 "Isostatic" --> "Isostasy"
We rewrote as `Isostatic adjustment'

l235 "where τa is typically set to 3000 years"
I still don't understand why you need this time delay. If you are interested in the ice sheet steady state, you can speed up convergence by just setting τa to zero.
This is an interesting remark since we are evolving the ice sheet towards a steady state, and a formulation without this time delay has been used, for example, in the 1D model by Weertman 1961
https://agupubs.onlinelibrary.wiley.com/doi/10.1029/JZ066i011p03783
However, we need to evolve the ice sheet at a small time step anyway for numerical stability reasons and for the surface mass balance calculation.
Therefore, we use the time delay τa in the ice sheet model MITgcmIS, where ice sheets are simulated to grow (always starting from the bedrock topography) under different climatic conditions given by the previous iteration for a total simulation time of the order of T = 40000-100000 yr and a time step Δt = 1 yr, as now specified at L269. We now specify that the time step used in MITgcmIS is Δt = 1 yr and reformulate the sentence by avoiding the term `iteration'.

l238 "where Htopo is given by the topography file"
What does the topography in the file represent? I suppose for ETOPO you assume balance with the current load. How do you initialise for an unknown ice sheet configuration? Explain!
For deep time, we use reconstructions of paleogeography given by Vérard 2019, Scotese 2021, Merdith 2021, while for present day we use the bedrock topography from observations. We added a sentence to specify all this.

l239 "also the SAT"
Same as Ta in 208?
Yes

l240 "by estimating the lapse rate dT/dz"
I think with Equ 12 you are estimating the temperature correction, not the lapse rate itself. Clarify!
We are indeed correcting the temperature field by including the lapse rate effect. We have reformulated this sentence. We have also reformulated the sentence at L.244 accordingly.

l242 "where Tnew is the temperature after the lapse rate correction"
What is Told? Also coming from The MITgcm output of the previous run? Where the previous run is the run that provides that forcing for the ice sheet run? Clarify.
We now call Tn = Tnew and Tn-1 = Told, with lapse rate = dTn-1/dz. We have better explained the procedure and set definitions.

l242 "The lapse rate is computed at each ice-sheet grid point using the
MITgcm output of the previous run"
Is the previous run the same as the previous iteration (as used in l210)? Clarify.
The reviewer is right, we meant the previous iteration, we have corrected this typo.

l244 "based on the corresponding altitude"
How? Maybe using bounding pressure level altitudes around the current surface elevation?
We have reformulated this sentence in the revised version.

l245 "since some new ice sheets"
--> "since some new ice sheet grid points may have"
We reformulated as `some ice-sheet volume may have formed or disappeared'.

l245 "the amount of water that has been exchanged with the ocean is estimated"
Explain how this is done in detail!
We have explicitly added how it is estimated and added a reference for this volume compensation.

l246 "as it is done in North Atlantic hosing experiments"
There are a few different approaches documented in the Jackson paper. What exactly are you doing? In my understanding, the ice sheet is presented to the climate model as a steady state boundary condition. This means that no additional freshwater fluxes should be coming from the ice sheets in the climate iteration other than routing total precipitation over the ice sheets similar to any other land surface. Is this the case?
We have reformulated this sentence and included a formula.

l248 "updated coastlines defining new topography (including ice sheet height)"
The other way around makes more sense to me:
updated topography (including ice sheet height and sea-level) defining new coastlines"
Since at the end we need to produce a new topography file, we prefer to keep the former sentence.

l256 "(unless elevation reaches values below−2000 m)"
Set in parentheses to avoid interpreting this as "such cases" in the next sentence.
We agree.

l256 "we re-assign the elevation"
Important to mention if this changes the ocean volume and sea-level, and if so, how.
This procedure does not affect the ocean volume, as we now specify.

l279 "redirected into the ocean via the runoff"
How is energy conservation handled? Is the energy needed to melt the snow/ice taken from the ocean? Where?
Energy is conserved in MITgcm once the runoff routing map is defined (see Campin JM, Marshall J, Ferreira D (2008) Ocean Model 24, 1, 2008, doi:10.1016/j.ocemod.2008.05.005)

l281 "in the first steps of the coupling procedure"
Not clear why this is only needed in the first step. Explain.
This correction is usefull until a steady state is reached and the ice sheet stops to grow. We have added this information in the revised version.

l281 "precipitation storage area"
It is not clear to me what the precipitation storage area is and how it can be used to correct for a mass flux imbalance. Explain.
We mentioned the precipitation storage area at L291. In general, it corresponds to the area of the land grid cell where precipitation falls.

l284 "Pitot is the total rain precipitation"
Is that maybe total precipitation? Otherwise, why is snowfall always larger than rainfall? We will also need to know how rain is treated over ice-covered grid cells. Is it just routed without interaction?
The reviewer is right, Ptot is the total precipitation. We have corrected this typo. The rain is just routed without interaction with the ice sheet, as now stated at L251.

l287 "Coupled MITgcm atmosphere-ocean-sea ice-land Setup"
This was defined as "Coupled MITgcm Setup"
The reviewer is right, we now write simply `Coupled MITgcm Setup'.

l296 "the ocean point becomes a land point"
Does this imply a change in the ocean volume and sea-level?
Yes. This is included in the estimation of the ice sheet volume after MITgcmIS has run.

l296 "(up to−20 m of depth)"
Consider moving this to after "advancing ice flow over shallow ocean" in l295.
Thanks, we agree with this suggestion.

Fig1
The numbering 1., 2., ... is confusing as it interferes with the iteration count (initial, 1) and with BIOME version number. Are you using these numbers at all. Remove?
"Coupled MITgcm setup" --> "Coupled MITgcm Setup" in box 1 and 5.
We agree with both suggestions, the figure has been updated.

l310 "salinity and sea temperature at all ocean levels are averaged over the last 30 yr"
Is this ocean model output from the first MITgcm iteration? Why do they need to be averaged? Why not restart the model from the end of the last iteration? Are there other changes applied to these fields, like new masks?
Since the land-ocean mask changed, we cannot use the restart files directly.

l312 "are given back to the MITgcm to run the whole coupling process at least twice, so that the GCM has time to adjust to the new input files"
A bit confusing. You could run MITgcm long enough so that it "has time to adjust to the new input files". But I think what you are saying is that you iterate at least twice (with updated input files each time) so that the whole asynchronously coupled system converges.
How does "at least twice" go together with your own experience for the present day in l372: "Five iterations of the procedure illustrated in Fig. 1 were necessary"?
We confirm that at least two iterations are necessary so that the whole asynchrounous coupled system converges. We compare two iterations to assess convergence (this is why we need at least two iterations), as explained at L350-354 (we check the surface energy imbalance and relative changes in both biome distribution and ice sheet volume). In run1, we needed 5 iterations.

l320 "3.1.1 biogeodyn-MITgcmIS initial conditions"
I feel many aspects presented here are not well placed in a "Results" section. Description of the initialisation and tuning may be better placed in a section about experimental setup before discussing results.
We agree. We have thus changed the section numbering as follows:
3 Experimental setup
3.1 Comparison with data and CMIP models
3.2 BIG-MITgcm initial conditions
4 Results
4.1 BIG-MITgcm evaluation
4.1.1 Atmosphere
4.1.2 Ocean
4.1.3 Vegetation
4.1.4 Ice sheet
4.1.5 Runoff
4.2 The Permian-Triassic case
5 Future developments
6 Conclusions

l321 "To start the first run of our simulations"
Maybe you have to first explain what simulations are planned in a section called "Experimental setup" or similar?
We agree. We have changed the organisation of the subsections (see previous point).

l327 "A smoothing procedure"
Details needed. Describe it in the modelling part.
See comment L108.

l330 "with the addition of an isostatic correction as in Paxman et al."
Are you using the Paxman procedure with your LLRA model, or are you using their corrections derived with another Earth model? If the latter, then there is an inconsistency expected between the re-loaded bed and observations. Please explain.
We use the correction in Paxman. At the end, we evaluate the surface elevation in our modeled ice sheets against that in the BedMachine dataset. This comparison (which shows an overall agreement with the BedMachine dataset, see Fig. A8) includes also inconsistencies in the isostatic correction.

l335 "PANALESIS or other reconstructions directly provide bedrock elevations"
Do they typically provide unloaded or loaded bedrock elevation?
These reconstructions provide unloaded bedrock elevation, as now specified at L366.

l338 "albedo values for [..] snow and ice have been fixed"
I missed where this was explained. How are they chosen?
Does that mean that your tuning happens after the first iteration? If not, what is the impact of the coupling on the uncoupled climate state?

We now specify that albedo values for vegetation cover, snow and ice are set to values within observational ranges. We set these values at the beginning and they stay fixed during the coupling procedure.

Figure 2
We don't know what run1 is. Please describe the experimental setup in a separate section.
Runs are described in the new section 3 (`Experimental setup'), section 3.1.
We have added the description of run3. This is the simulation at 280 ppm starting from ETOPO2 (i.e. with present-day ice sheets) performed with the Coupled MITgcm Setup. This run3 simulation -- already discussed in the previous version of the manuscript (in the Ice-sheet results section, first paragraph, and the old fig. A6) -- is now explicitly defined here because, following reviewer's suggestion, we have added more diagnostics to compare it with run1 (the pre-industrial simulation performed using BIG-MITgcm starting from bedrock topography, i.e. zero ice thickness).

l343 "Evaluating the surface mass balance produced for Antarctica"
Why is that only relevant for Antarctica and not for Greenland? Are you inheriting values for Greenland from the AIS calibration?
We tune on Antarctica, which is better resolved in our setup. We obtain a value for the Glen's law coefficient that is then used in MITgcmIS for all the ice sheets. We are not assuming the same SMB-elevation relationship for Greenland and Antacrtica.

l349 and l352 "the value obtained in our simulation is slightly lower"
I would argue that more than 25% off is not "slightly lower". Rewording needed.
We now say simply `lower'.

L357 and Table A1
Please check the units are correct for 'a'. The typical rate factor 'a' in Glen's flow law in the literature is 10^{-15} - 10^{-13} kPa^{-3} a^{-1} or
10^{-24} - 10^{-22} kPa^{-3} s^{-1}.
Since basal sliding is not included in the model, one would expect a need to compensate for it by increased deformation. Does that make sense for the choice of parameter compared to typical values of a?
We thank the reviewer for pointing out this error. The correct units are 10^{-15} Pa^{-3} yr^{-1} and not 10^{-15} Pa^{-3} s^{-1}.

Table 1.
Could specify the type of grid in first row ('Resolution') instead of NA.
We agree with this suggestion.

l377 "MITgcmIS needs around 1 hour of CPU time"
Specify for how many model years the ISM is run.
We have specified that this CPU time is required for running 40 thousand years at L381.

l378 "for all land points."
I thought there was a procedure to limit the calculation to ice-covered points? Why not describe this in the manuscript?
Thanks, this was a typo, we have corrected to `ice-covered points'.

l382-461 "Atmosphere" and "Ocean"
What still doesn't become clear in these sections is how the new couplings influence the behaviour of MITgcm. This should be shown by comparing the pre-industrial MITgcm (without coupling) to biogeodyn-MITgcmIS, e.g. with additional lines and panels in figures 3-8 where possible. It is important to understand how behaviour and sensitivity of the model changes with the new couplings.
In the revised version, the pre-industrial simulation with the Coupled MITgcmSetup is called run3 Note that this simulation was already performed and discussed in the previous version (in the Results Section, Ice sheets, Sec. 4.1.4 and fig. A7 in the new version numbering). Now, we have added the curves corresponding to run3 in the zonal profiles (fig. 4) and in the AMOC profile (fig. 5).

l385 "close to the real data of 13.7◦C"
"real" --> "measured"?
Maybe this value should be added to the table?
Clarify that this the the 1850 value.

We have included these suggestions.

l386 "the global mean SAT of 15.9◦C"
Clarify that this is now for another period.
We have specified that this is for the 1979-2009 period in the revised version.

l387 "Earth Climate Sensitivity (ECS)"
Typically "Equilibrium Climate Sensitivity (ECS)"
Thanks, this was a typo.

l395 "higher temperatures than observations and CMIP models"
We need to know if this due to the ice sheet representation in biogeodyn-MITgcmIS or already in the Coupled MITgcm Setup.
The comparison between run1 and run3 (fixed present-day ice-sheets) confirms this fact (see fig. A7).

l396 "where the ice sheet elevation is fixed to observed values"
What happens in biogeodyn-MITgcmIS when you fix the ice sheet to observed values?
This corresponds to run3 (see the response to previous points: Figure 2, L382, L395).

l490 "The total volume is 24.5·106 km3"
You should comment on the absence of other glaciations (e.g. high-mountain Asia, Canadian Arctic, Alaska, ...), possibly not captured due to the coarse model resolution?
We have added a comment on this at L581. The model does not capture other glaciers, with a global glacier volume of around 158 10^3 km^3 (Farinotti et al. Nat. Geo. 12, 168 (2019)), due to its coarse resolution

l492 "after smoothing to the same spatial resolution"
How is that done? To compare total volumes, you should integrate the ice thickness, which can be done independently on the different grids. For local comparison on the grid cell level, you should conservatively interpolate to a common grid.
We calculated the ice sheet volume obtained in run1 directly on the cubed-sphere grid. For comparison, we interpolated the observed volume on the same cubed-sphere grid, as specified now at L579.

l493 "This overall agreement with observations [...] is reflected in"
There are two different types of agreement. The matching total volume, which you have basically tuned for and the match with spatial distribution, which you are testing with the correlation. The first cannot be "reflected" in the second.
We have reformulated this sentence.

l506 "and measurements of the ice sheet thickness."
Uncertainties in ice thickness are not relevant when comparing surface elevation in your model against BedMachine. Surface elevation of the ice sheets can be measured with very small uncertainties.
This was a general statement, which is now reformulated. Uncertainties in the ice sheet thickness come from the reconstruction of the bedrock position, which can have errors of several hundreds of m in some places.

l507 "we can conclude that our model reproduces the first-order characteristics of the ice sheets reasonably well"
I fully agree with this statement.
Very good.

l524 "slightly lower SAT and higher sea ice extent"
Can you identify the reason for these changes? Is it ice sheet elevation, albedo, runoff? Is there a similar influence in the present day?
The presence of the ice sheet in the northern hemisphere is the only difference with respect to the Ragon et al. simulation.

Figure 13 and Figure 14. Can these be plotted like Figure 12, to get a more familiar and comparable view?
We have included the bathymetry.

Figure A1.
Consider inserting the full ERA5 as middle panel in lower row, to get a similar look as what I suggest for Figure

A2.
Fix gaps across the dateline?
We have followed reviewer's suggestions.

Figure A2.
On first view, all panels look the same, which is probably the idea. But it doesn't help much to identify the differences. Add a difference plot (against ERA) in the remaining panel to reveal where run2 disagrees.
We added a difference plot as suggested.

Figure A4. The Antarctic panels are flipped in the vertical (compare to correct version in Figure A7.).
Also consider to rotate all panels for a more recognisable view (North pointing up in the Arctic, South America on the left for the Antarctic)
Corrected

Something is strange with the colorscale. It is difficult to see where ice-free ocean is. Why is colorscale clipped to 0.2? Consider a color that does not appear in the colorscale to represent ice-free ocean.
Corrected

Figure A7. Consider to rotate Antarctic panel by 90 degrees CCW for a more recognisable view (Antarctic Peninsula on the left). Since it is hard to make out the shape of the ice sheets (especially for Greenland), consider to show 1. the observed ice sheets, 2. the modelled and 3. the differences in 3 panels per ice sheet. Or at least overlay the outlines of the observed ice sheets. What is the isolated red grid point on the left of Greenland? If it is part of Canada, you may want to remove it from the comparison.
Corrected and removed from the anomaly map.

All Figures
Increase all label sizes on axes, legend, colorbar, titles.
We have increased all label sizes. We have also improved the captions.