Dear Editor, dear reviewers,

We are grateful for the detailed feedback by the reviewers and their very useful suggestions. We have worked on the manuscript since the first review in October 2025 on the following important points:

1) We have clarified the use of our coupled framework, focusing on climatic steady states, where all components reach a quasi-stationary state, included the ones with a slow response such as ice sheets and vegetation. Consequently, we have changed Abstract, Introduction and Conclusions to better explain how our coupled framework can be used to identify climatic steady states and their basin boundaries, where the climate becomes unstable and shifts from one attractor to another.

2) We have updated the pre-industrial simulation (*run1*) using a slightly different setup, which solved the issue of the energy imbalance at the top of the atmosphere, giving overall results that are similar in performance to the original ones with new tuning parameters. As in the old manuscript, *run1* is used to test our new coupled framework by simulating the pre-industrial ice sheet starting from the bedrock elevation and to compare the results with those of two CMIP models with dynamical vegetation.

3) We ran additional simulations:
    - a deep-time simulation (Permian-Triassic cold state based on Ragon et al. Scient. Rep. 2024) to show the ability of our coupled setup of dealing with ice sheet formation in different continental configurations and climate conditions.
    - a forced simulation from 280 ppm to 360 ppm (new *run2*). In this case the ice sheet is kept constant to the extent and volume obtained at 280 ppm. This simulation is used to assess our results against reanalyses and observations in non-stationary conditions.

With these updates in mind, we believe that our manuscript can be strongly improved, following the reviewers' remarks.

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

Sincerely,

On behalf of all coauthors,

Laure Moinat and Maura Brunetti

Response to reviewer 1 for: Moinat et al. – biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploratory climate modelling.

Summary

Moinat et al. present a modelling tool that consists of the MITgcm augmented with additional offline components for vegetation, runoff and ice sheets. The model is run at intermediate resolution and mainly intended for deep time steady state simulations. Simulations under pre-industrial and present-day forcing are compared to other models and observations.

General comments

I have struggled quite some time to understand what this modelling tool is supposed to be used for. It is not until page 6 that this is clearly spelled out ("Since our purpose is to investigate climatic steady states in which ice sheets are in balance with the ocean, atmosphere, and biosphere"). If this is true, the statement should already be present in the abstract and the introduction should be geared towards that purpose. However, it is important to understand that in the real world, an ice sheet is never really in balance with the prevailing climate. While there is also mention of transient simulations, I believe in practice the model can only be used for time slice simulations, and only for those where assuming ice sheets in steady state with that climate is an acceptable assumption. The premise of the presented modelling (operating with climate and ice sheet steady states) excludes the application of the tool to interactions between climate and ice sheets that happen on timescales shorter than the relaxation time of the slowest component (the ice sheets), which is 40-100 kyr (l289). This may e.g. be illustrated by the Greenland ice sheet losing large amounts of its volume in run2 as it is relaxed to a steady state in the present climate, rather than attaining a transient state.

Answer: We sincerely thank the reviewer for the detailed report and for the constructive criticisms, which are very useful for improving our manuscript (see also our response on October 17, 2025). We agree with the reviewer that we need to clarify the applications of our model setup. The main application is to find, for a given climate and CO_2 content, the corresponding ice sheets and vegetation cover over multimillennial timescales. This is particularly useful in deep time, where the ice sheet extent and volume are poorly constrained, as well as the vegetation cover. Our model setup can also be applied in the case where the ice sheet is fixed to find the corresponding climate and vegetation under a given forcing or under the effect of a forcing that changes on a time scale smaller that the ice sheet response (for example, by changing the CO_2 content on centennial time scales).

In the first version of the manuscript, we focused exclusively on steady-state examples, in which the ice sheet evolves toward equilibrium with the corresponding climate and vegetation. This approach is particularly suitable for investigating deep-time climates, such as those of the Early Triassic, where paleogeographic reconstructions provided by PANALESIS or paleoMap do not include ice sheets or account for isostatic adjustments. Nevertheless, the climatic conditions in the Early Triassic, for example, could be sufficiently cold to allow ice sheet formation, as demonstrated in previous numerical experiments conducted within our group (Ragon et al., 2024). The advantage of our ice sheet model is being at global scale, allowing for the description of climates where the ice sheets are not confined to high latitudes, like during glacial periods, waterbelt or snowball states.

However, before applying our coupled setup to deep-time climate configurations, it is essential to verify whether the model can reproduce the preindustrial ice sheet starting from the bedrock topography (*run1*: preindustrial (PI) state).

Regarding *run2* in the first version of the manuscript, which represents the evolution of the ice sheet under increasing CO_2 forcing until a steady state is reached, we agree with the reviewer that this experiment cannot be directly compared to observations, as the latter reflect transient, forced conditions. This is why we have run a new *run2* simulation, as detailed below.

I can see two options for the manuscript to continue, both involving considerable work:

1. There may be value in this tool being applied to simulating deep time climate states (as hinted at e.g. in the title and model name) with steady state ice sheet configurations. In that case, the manuscript has to be strongly refocussed, aspects of transient simulations removed, comparison focussed on the preindustrial and a deep time use case added and discussed.

Answer: We fully agree with the reviewer that including a deep-time simulation is relevant to demonstrate the capability of our coupled setup to be applied to different paleogeographic configurations. We therefore now include the case of the Permian–Triassic cold state described in Ragon et al. (2024), in which we employed the ice sheet model to estimate the thickness and extent of the ice sheet that develops in the Northern polar region.

2. It may be possible to modify the model setup to allow for transient ice sheet simulations. It is common practice to run asynchronously coupled simulations between climate and ice sheet models where the climate forcing is accelerated. In that case the ice sheet component is the 'pacemaker' of the experiment and determines the physical time evolution. In that case more work would have to go into improving the modelling. Adding another example for a climate state different than the PI/PD would also be needed here.

Answer: We agree with the reviewer that setting the ice sheet component (pacemaker) and let the climate evolves consequently is the way we can run transient simulations. This is what we now do in *run2:* the ice sheet is kept fixed at its PI configuration, while the responses of the climate and vegetation are examined under a gradual increase in atmospheric CO_2 concentration from 280 ppm to 360 ppm. This forced simulation can be readily performed with our coupled setup and is compared to available reanalysis data. Such an approach is commonly employed in CMIP-style CO_2-forced experiments. Note that this procedure can be applied to any arbitrary initial ice sheet configuration. However, since this is a validation paper, we have applied this procedure to PI/PD.

The following comments are mostly independent of this choice.

What I miss in the introduction is a clarifying discussion of limiting factors in ESM modelling: spatial resolution, complexity and timescale. With limited HPC resources compromises are typically made on all three of them with different weight depending on the specific application of interest. What is the benefit of the specific choices made here? There is a relatively high resolution climate component compared to traditional EMICs but very low resolution of the ice sheet components. I don't understand the latter choice, since ice sheet models are typically the least expensive component and it would be feasible to run at a reasonable resolution without slowing down the system considerably. If the ice sheet resolution is not improved, it should at least be made clear that the current setup has a spatial resolution of the ice sheet models that is well below of what is considered appropriate to resolve ice sheet dynamics.

Answer: We have reformulated the discussion on the limiting factors (spatial resolution, complexity and timescale) in the Introduction. The main advantages of our setup are the following:

1. The ice sheet model is global, which is particularly important for deep-time simulations where data are scarce and the potential location, extent, and thickness of ice sheets under cold scenarios are not known a priori. The fact that is at the global scale allows our model setup to describe climates when the ice sheets are not confined to high latitudes, but spread to large regions, like the waterbelt and snowball states.

2. It includes the first-order, large-scale dynamics of ice sheets, allowing global-scale simulations on millennial time scales to be performed without the need for high spatial resolution and with relatively low computational costs.

3. It employs the same cubed-sphere grid as the underlying climate model (MITgcm), thereby avoiding interpolation errors.

Once the ice sheet extent and volume are estimated at the first order with our setup, one can always run high resolution ice sheet models to investigate other processes, but this is outside the scope of the present setup.

I think it should be demonstrated that the model can be set up for a considerably different climate.

For a tool that supposedly can run multi-millennial simulations with ice sheet components it would be reasonable to at least be able to show an LGM configuration. Ideally I would also like to see a warmer climate configuration in addition like the last interglacial or the Pliocene. The setup of the tool may be in principle the same for any other time slice, but as modellers we know that there are always more complications once it is actually done. As is, the pre-industrial and present-day climate are too similar to serve as good validation experiments alone.

Answer: We have added a deep time simulation, the Permian-Triassic cold state in Ragon et al. 2024, to demonstrate the ability of our coupled setup of simulating very different continental configurations and climate conditions.

The analysis is heavily focussed on comparison to other CMIP models and observations, (where the latter is questionable due to the steady state nature of the modelling). Since I imagine that the uncoupled MITgcm has been validated extensively before, I believe there should be more focus in 3.2.1 and 3.2.2 on comparison to a pre-industrial state of the reference MITgcm without the additional components. This would show how the presented developments change the model behaviour.

Answer: In our *run1* simulation, the present-day ice sheets are obtained starting from bedrock conditions. This is now compared with MITgcm present-day simulations starting from etopo2 (which includes ice sheets), using the same parameters as in *run1*. Another MITgcm simulation is in Brunetti & Vérard 2018 but with different parameters.

Specific comments

Title and model name "a biogeodynamical tool"
I am a bit confused about the term "geodynamical". According to the Wikipedia definition (https://en.wikipedia.org/wiki/Geodynamics), this should involve mantle convection and plate tectonics. If a deep time application with different continent distribution and climate could be shown, this would make a bit more sense.

Answer: The Permian-Triassic example is now added; therefore, the title and name of the model is more adapted.

l2 "climate tipping elements"
The mention of climate tipping elements early in the abstract and in the introduction is confusing. Studying climate tipping points most people are concerned about requires full physical coupling between different climate components, which involves specific time scales, not steady states.

Answer: The part on the tipping elements was removed in the Introduction, which is now refocused on steady-state simulations and its application to deep time.

l7 "a complexity level intermediate between EMICs and CMIP-class models"
Consider another description of the complexity level of your model.
EMIC already contains the term intermediate. I would say your model is by definition an EMIC, albeit maybe of relatively high resolution of the climate component compared to many other EMICs (add references).

Answer: This part in the abstract was reformulated. The following references were added in the introduction: Holden et al. 2016, Willeit et al. 2022.

l9 "offline couplings"
Depending on where the modelling goes, I would suggest to use the term "asynchronous coupling" instead. "Offline" may be read as one-way coupling without feedback on the core climate model.

Answer: It is effectively asynchronous, as there is feedback to the core climate model. This is corrected in the revised manuscript.

l10 "(MITgcmIS)"
The naming is a bit confusing. If MITgcm in itself is the climate model, MITgcmIS sounds like it could be the coupled climate-ice sheet model.

Answer: The motivation for this name is that the ice sheet model is explicitly designed for the cubed sphere that is used in MITgcm.

l10 "The latter is implemented on the same cubed-sphere grid"
This should be motivated at some point. It also has to be noted somewhere that this resolution is well below of what is considered appropriate to resolve ice sheet dynamics.

Answer: The motivation about using the same cubed-sphere grid for global-scale ice sheet reconstructions has been added in the Introduction, and more in detail in Section 2.4.

l12 "the new ice sheet model and the coupling procedure"
Focus is on the ice sheet component, which is not represented in the title. Consider modifying the title accordingly.

Answer: We have changed the title to:

biogeodyn-MITgcmIS (v1): a biogeodynamical tool for exploring climate steady states with a new global-scale ice sheet model

##l12 "We evaluate biogeodyn-MITgcmIS" and l14 "biogeodyn-MITgcmIS successfully reproduces the large-scale climate and its major components"
Offline coupling of the new components (l8) would suggest that the base climate state of biogeodyn-MITgcmIS is identical to the core MITgcm. I suppose MITgcm has been validated before. Clarify what is evaluated here specifically and why another response is expected.

Answer: We have specified that the assessment is performed starting from bedrock topography in pre-industrial climate conditions until our coupled setup gives rise to the ice-sheet formation. We then compare the results of our simulation (*run1*) with the control runs of CMIP models in terms of the overall climate, vegetation cover, and ice sheet extent and volume, assuming that the pre-industrial climate is at steady state. A second experiment (*run2*) explores the forcing conditions in the 1979-2009 period, and the results of our simulation are compared to reanalyses and observations. In this second experiment, the ice sheet is kept constant to its pre-industrial extent and volume.

l13 "pre-industrial period and the 1979-2009 period"
Difficult to imagine how to evaluate climate-ice sheet feedbacks for these rather steady periods. How does the model perform when the ice sheets are changing considerably?

Answer: To tackle this question and the concern about the model application to deep-time climates, we added a simulation for the Permian-Triassic paleogeography based on Ragon et al. (2024), starting from the cold-state conditions. In this case, an ice sheet develops in the Northern Hemisphere, with a volume of 8.5 million km^3, corresponding to around 20 m of sea level change. This is in the range of possible values in the Early Triassic (Simmons et al. 2020). A new section has been added in the revised manuscript with the following figure:

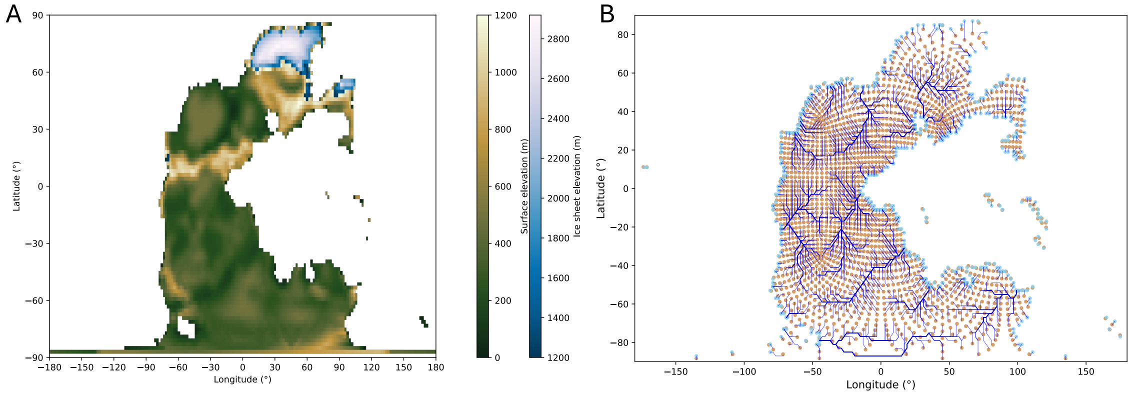

Figure 14. *A Permian-Triassic topography with ice sheet elevation obtained using biogeodyn-MITgcmIS (the bedrock elevation used to start the simulation can be found in Ragon et al., 2024); B. Runoff routing map for the corresponding topography.*

l18 "CO_2 concentration raises under the present-day climate crisis"
Strange combination in this sentence of future perspective and "present-day". Reformulate.

Answer: Reformulated in the revised manuscript.

l32 "simulations cannot reach stationarity in slow deep-ocean and ice sheet dynamics"
Focus only on stationarity seems like an insufficient or even problematic goal when interest is in tipping, feedbacks and dynamics.

Answer: We have now better defined stationarity and tipping behaviour in the Introduction. Simulations reaching a steady state are used to understand the dynamical structure of the Earth climate by identifying the different climate steady states (attractors) under external forcing (e.g. CO_2), their limits of existence (basins of attraction), and the associated boundaries, which may correspond to global tipping points (that is, forcing values beyond which the climate system shifts towards a new steady state). Reaching stationarity is an important property for characterizing climate attractors (Hawkins et al. 2011, Drótos et al. 2017, Lucarini and Bódai 2017, Brunetti et al. 2019, Brunetti and Ragon 2023, Moinat et al. 2024).

l35 "the nonlinear interaction among climatic components"
In your setup, this could only be addressed with high enough frequency of the asynchronous coupling and transient ice sheets.

Answer: We meant that in EMICs, some simplified parameterizations may have an impact on the nonlinear interaction among climate components. In our setup, the numerical representation is improved with respect to EMICs (in terms of spatial resolution, ocean dynamics, asynchronous coupling with ice sheets and vegetation). However, as we mentioned above in the response to the general comments, the ice sheet is too coarse to run transient simulations on centennial time scales, since km-scale processes are not included. We have improved the Introduction following the reviewer's remarks.

l46 Add reference to Smith et al. 2021 (UKESM)

Answer: Added in the revised manuscript.

l56 "complexity that is in between EMICs and CMIP-like models"
See comment l7

Answer: This has been reformulated in the revised manuscript.

l59 "We will provide a complete description of all the components"
A complete description of all the components of a coupled ESM does not fit in this paper. Maybe you meant a brief description of the climate components and a full description of the ice sheet component?

Answer: This is indeed what we meant. This part was reformulated in the revised manuscript, where we mention that we will provide a short description of the dynamical core (MITgcm), the vegetation model (BIOME4) and the hydrology model (pysheds), as they have been already detailed elsewhere, while we will extensively describe the ice sheet model (MITgcmIS).

l74 "SPEEDY is in the intermediate complexity class"
See comment l7

Answer: This has been reformulated in the revised version of the manuscript, where we explicitly state that SPEEDY is an intermediate complexity model for the atmosphere.

l80 "planetary boundary layer"
Is this level used to provide boundary conditions for the ice sheet model? Usually PDD models ingest 2m air temperature. Discuss this as caveat of the coupling.

Answer: The temperature for the ice sheet model is indeed taken at 2m. The lapse rate is calculated using the temperature in the atmosphere levels. This information is included in the revised manuscript.

l90 "runoff map"
Is this runoff mapping, like a runoff routing scheme? It prescribed the pathway but not the amplitude? Clarify.

Answer: Indeed, the runoff map that we need to provide in input is a routing scheme and not an amplitude scheme. This has been clarified in the revised manuscript. We added two new figures showing the runoff routing maps for *run1* and the Permian-Triassic case.

l90 "salinity and sea temperature for all the ocean levels"
Sounds like salinity and temperature are prescribed rather than dynamically calculated. Is that the case? Or is this just for the initial state? Clarify.

Answer: The input salinity and temperature files are used just as initial conditions, then these variables are dynamically calculated. This information is now included in the revised manuscript.

l90 "Orbital parameters can be set by specifying the obliquity, the duration of the day and the radiative influx from the sun"
Orbital parameters are typically obliquity, precession and eccentricity. I don't think "duration of the day" and "the radiative influx from the sun" are strictly speaking orbital parameters. Is it maybe possible to say that the effect of the orbital configuration can be set by specifying ...?

Answer: This sentence is indeed ambiguous. The orbital parameters can be explicitly set by changing obliquity, precession and eccentricity. In addition to this, it is possible to change the duration of the day and the radiative influx from the sun. This is clarified in the revised manuscript.

l91 "200 years per CPU day using 25 cores."
Does this mean you can run 200 years in 24h or 5000 years? Please present two numbers: 1) how fast is the

model practically, i.e. how many years per day do you run with your typical configuration, 2) what is the efficiency of the model, e.g. how many CPU hours does a 100 year run consume.

Answer: The model in our setup can run 200 years per day using 25 cores, which is equivalent to 300 CPU hours for 100 yr (25 CPU x 12 hours). These two numbers are added in the revised manuscript.

l93 "as well as deep-time climates"
Since the focus of the section is not different climates but different palaeogeographies, I would reformulate here.

Answer: This has been reformulated in the revised manuscript.

l107 BIOME4 is described here as a purely diagnostic component forced by output of MITgcm. Is there any aspect of the vegetation model that would feed back on climate? I am mainly thinking about albedo, but also runoff, since you mention water holding capacity.

Answer: The albedo and vegetation cover are given back to the coupled MITgcm. This is added in the revised version of the manuscript. The water holding capacity can also change depending on the vegetation cover and is used in the MITgcm land module. However, we kept it constant in our simulations, as specified now in the revised manuscript.

l120 "28 including land ice"
What does that imply for the interaction between BIOME4 and the land ice component? You do not mention ice sheet extent as input to the vegetation model. What happens when the ice sheet retreats or expands? Can vegetation grow on land that has newly become ice free? What if vegetation is overrun by expanding ice sheets?

Answer: BIOME4 is the last part of the coupling, therefore the vegetation can grow on land that has become ice free. If expanding ice sheets overrun the vegetation, the albedo (0.75) and the vegetation fraction (0%) of the ice sheet is given. This has been clarified in the revised manuscript.

l130 "interested in horizontal resolutions of the order of 2◦ or coarser"
A bit strange to state that you are "interested" in such low resolutions. I think you should aim for a setup that resolves the processes you are interested to represent.

Answer: Corrected in the revised manuscript.

l130 "we can neglect basal melting and other fine-scale processes, as calving and ice streams"
I would rather say that you neglect those processes because they cannot be represented at the coarse resolution you are modelling. I still do not understand why you chose such a low resolution for the ice sheet components.

l132 "STREAMICE"
Could you explain why you are not using this module?

Answer: Our wording at L130 of the original manuscript is revised. The reasons for our low-resolution approach to ice-sheet dynamics are as follows. We are primarily interested in the first-order effects that ice sheets play in setting the global climate state i.e. their orographic and albedo forcing, as well as large impacts on global mean sea level. As such, fluctuations in marginal geometry e.g. rapid speedup of marine ice streams or tidewater glaciers due to calving or ice-ocean interactions are not our primary focus.

While a higher-order ice-sheet model such as STREAMICE could potentially be used, there is little purpose in doing so. Running STREAMICE at the same resolution as other components would require extensive computation for higher-order stresses that are not relevant at these scales, and as STREAMICE uses an explicit time stepping scheme, would limit the time step. At the same time, running STREAMICE at kilometer-scale resolution, as it is generally used, would preclude long-timescale simulation due to the computational cost of the associated nonlinear elliptic equation. Moreover, it likely would not improve the physical representation of the slow-moving interiors of ice sheets, and there would not be a reliable means of representing ice-ocean interactions at the margins with such a coarse ocean model. In general, as well, using STREAMICE would require complicated interpolation between a cube-sphere grid, and the cartesian space within which STREAMICE runs.

We do concede we are unable with our current framework to study large-scale drawdown mechanisms triggered by ocean melt of ice shelf or tidewater calving (e.g. Marcott et al, 2011). Still, we feel our approach is sufficient to represent the aspects of ice sheets in which we are interested at low computational expense.

l135 "improved Positive Degree Day"
Is your method improved compared to Braithwaite (1977) or compared to other more recent PDD implementations?

Answer: It is improved compared to Braithwaite (1977), as shown in the original paper by Tsai and Ruan (2018).

l135 "as we also want to apply our coupled framework to paleoclimates."
Not sure to understand the reasoning. Is this an argument for using PDD instead of prescribing a given SMB?

Answer: This sentence has been removed in the revised manuscript. This point was indeed discussed in the next part of the paragraph, where the choice of the PDD against the Surface Energy Balance (SEB) method was motivated.

Also here, I would like to see how the PDD results would look like for a considerably different climate.

Answer: This is shown in the new section on the Permian-Triassic cold climate.

l137 "We choose an improved PDD approach"
Same question as l135

Answer: It is reformulated in the revised manuscript where we explicitly mention the paper by Tsai and Ruan (2018).

l139 "This is an important limiting factor to consider that makes it difficult to apply SEB to paleoclimate simulations"
I am not convinced about the reasoning here. Similar limitations (too coarse resolution, missing important elements of the surface energy budget) certainly also apply to the PDD method, except that it is not as obvious/explicit as for an SEB method.

Answer: We agree that both methods are affected by resolution. We therefore deleted this sentence in the revised manuscript and instead focused on the fact that we consider the PDD method to be the best choice when atmospheric and snow processes are represented at the global scale on a coarse grid and treated in a simplified way, as in the SPEEDY and land modules of our setup.

l163 "where $\tau d = \rho igH\alpha$, $\alpha = -\nabla zS$"
I don't see the purpose of defining alpha here. It is also easily confused with "a" defined in l160

Answer: We agree, alpha is removed in the revised manuscript.

l173 "Since our purpose is to investigate climatic steady states in which ice sheets are in balance with the ocean, atmosphere, and biosphere"
This should be declared much earlier in the manuscript!

I am trying to make sense of the implications of this statement: an ice sheet fully adjusts to new climate conditions on a 100 kyr time scale. In the real world, an ice sheet is therefore never really in balance with the prevailing climate. I suppose this means that you are not interested in the coupled interaction between ice sheets and climate, which involves specific time scales?

Answer: We are interested in climatic steady states, and more generally in how a given climate is compatible with the presence of ice sheets. Therefore, the feedback of ice sheets on climate needs to be evaluated iteratively until convergence is reached, since we run our ice-sheet model starting from bedrock configurations. As a consequence, the model is not intended to perform *transient* simulations, for which time-scale issues would arise. Moreover, our ice-sheet model cannot resolve the kilometre-scale processes involved in ice-sheet transitions, as discussed above. In the revised version, we have clarified from the Introduction that our focus is on climatic steady states.

l174 "we choose not to represent these higher-order processes"
In ice sheet terms, "higher-order processes" has a specific meaning. Please use another description.

Answer: This is reformulated as 'km-scale processes' in the revised manuscript.

l192 "ODE"
Introduce abbreviation.

Answer: We removed the acronym and write it explicitly as `ordinary differential equations'.

l198 "The required input is the air surface temperature"
See comment l80

Answer: The MITgcm output of the surface air temperature is provided at 2 meters. This is specified in the revised manuscript.

l199 "significant improvement in capturing early-season melting"
After reading l173 and in the light of all the other simplifications in the model this seems like a quite unimportant refinement to me. But I guess it is good to use recent developments. I would be concerned if the tuning of PDD model parameters presumably done at much higher resolution by Tsai and Ruan and e.g. with a geometry that resolves the ablation areas is translating to your setup. Have you considered retuning the PDD model?

Answer: We have adapted some of the PDD parameters to our setup to correctly reproduce the volume and the orography of Antarctica. In particular, we set $k/h = 40$ W m^{-2} $^{\circ}C^{-1}$, $H_p = 10$ m, $h = 2$ m in our setup, and we tuned the Glen's law parameter.

Maybe more importantly for the deep-time application you envision, the assumption that the PDD with present day parameters translates to other orbital configurations is highly problematic

Answer: We mentioned this potential limitation of PDD in Section 2.4, citing the paper by Plach et al. 2018.

l209 "In summary,"
This summary concerns both 2.4.2 and 2.4.3 but appears in 2.4.3. I would suggest to describe ablation and accumulation in a joint section.

Answer: These two subsections have been merged in the new 'Surface mass balance' section.

l212 "accumulation rates"
"accumulation" and "accumulation rate" are used interchangeably. Please be consistent in terminology.

Answer: Corrected in the revised manuscript.

l215 "Taking into account the isostatic correction"
Why "correction"? Maybe "isostatic adjustment"?

Answer: Corrected in the revised manuscript.

l216 "where a time delay is included"
Why not use instantaneous adjustment in line with your interest in steady states? This does not seem consistent with the approach in other components.

Answer: The time delay is used during the growth of the ice sheet in the ice-sheet model that is run for 40000 yrs. In contrast, during the run of the core climate model, the topography does not change.

l232
Define Tnew and Tpickup

Answer: The term *Tpickup* is replaced by *Told* as pickup refers to the output files of the coupled MITgcm. *Tnew* is the temperature after the lapse-rate correction. These new definitions are added in the revised manuscript.

l233 "lapse rate is computed at each ice-sheet grid point using the MITgcm output"
Can you explain how this is done?

Answer: A temperature value is extracted from each pressure level in the atmosphere, and then, based on the altitude of each pressure level, a linear regression is made between the temperature and the altitude. The slope from this linear regression is the lapse rate. Finally, zonal average is performed.

l235 "freshwater flux to or from the ocean is computed and included at restart at the ocean boundary of the ice sheet"
I am confused about how that works. Does the freshwater flux enter the ocean instantaneously? What determines the time scale of release?

l236 "To guarantee the conservation of salt, a compensation is performed at the global scale"
How does that work precisely? Is the model operating with physical freshwater fluxes or with (negative) salt fluxes?

Answer: The model operates with virtual salt fluxes. We adjust the salinity content in the global ocean considering the volume of water that is stored in the ice sheets.

l283 "the other with daily frequency (for MITgcmIS)"
It seems counterintuitive that the ice sheet component needs a higher frequency than the vegetation model. In some PDD implementations the seasonal cycle is parameterised to reduce the input to monthly or even annual. This should be mentioned and you should consider it to improve the performance of your model.

Answer: The computation in the ice sheet model has been improved; therefore, it is no longer a limitation. To improve the performance of the ice sheet model, the following was done: 1) for the first 10 yr, the model computes the surface mass balance for every land grid point; 2) afterwards, it identifies the land points where the surface mass balance was systematically negative since the first iteration; 3) it withdraws these points from the computation of the surface mass balance. Applying this procedure to the pre-industrial run, the number of land points reduced from 1772 points to 166, giving a reduction of the computation time by a factor of 4.

l285 "if the sea ice thickness"
Do you mean "ice sheet thickness"?

Answer: It is the sea ice thickness indeed. We are describing here how the ice sheet can extend towards a shallow ocean covered by sea ice.

l289 "MITgcmIS is run for an equivalent of 40-100 thousand years"
Why "equivalent"? Isn't it run for an actual 40-100 kyr?

Answer: It is run for an actual 40-100 kyr. It has been clarified in the revised manuscript.

l290 "isostatic, lapse-rate and sea level corrections (Sec. 2.4.4) are included at this stage"
Are these corrections applied after the ice sheet model has run in MITgcm? Clarify. If so, how are feedbacks between isostasy and ice sheet SMB incorporated in this way?

Answer: This was not clearly explained in the manuscript. The isostatic adjustment is calculated during the ice-sheet model runs, using the lapse rate estimated from the previous convergence step. Then, the ice sheet volume is determined and is converted to a sea level change. The mask, topography and bathymetry files are changed in consequence. This explanation is now included in the revised manuscript.

L291 "pysheds is applied using these files"
What are "these files"?

Answer: *pysheds* needs the mask and the topography files. This is now explicitly mentioned in the revised manuscript.

l299 "the pickup files"
What is that?

Answer: We reformulated this part as follows: `… salinity and sea temperature at all ocean levels are averaged over the last 30 yr to generate new input files for the coupled MITgcm setup. Then, these files, along with the new vegetation fraction, albedo, topography, bathymetry and mask files, are given back to the MITgcm to run the whole coupling process at least twice…' .

l303 "describes in a consistent way the evolution of all the climatic variables"

I do not I agree with your notion of "consistent" evolution. The resulting evolution is not physically consistent in that the time scales of various processes are modified or removed. In the real world, e.g. the ice sheets do not have 40-100 kyr time to adjust to a new climate condition.

Answer: The term `consistent' has been replaced in the new sentence: `… which thus describes the climatic steady state, including those components with a slow response… '

l304 Figure 1 "Transient simulations: repeat the same procedure by changing the forcing each N years"
See point l303. This setup is not made to run transient simulations with time steps shorter then 40-100 kyr.

Answer: This part was removed from the figure, and the figure was updated in the revised manuscript.

l317 "with the addition of an isostatic correction"
Could you give a short summary of how that works? I am mostly interested if this is in line with the LLRA model you are using.

Answer: The ice-free isostatic topography is estimated in Paxman et al. (2022). We directly took the map from this paper and adapted its resolution to the cubed-sphere grid used in our setup.

l321 "The resulting isostatic map"
What does "isostatic map" mean? Is this the bedrock adjusted for unloading of the ice sheets? If so, it would be interesting to see the difference to the reference bedrock in addition.

Answer: It is the bedrock adjusted from the unloading of ice sheets. The isostatic adjustment to the reference bedrock (from BedMachine) is provided in Paxman et al. 2022.

l324 "PANALESIS or other reconstructions directly provide bedrock elevations"
Not sure about the ice sheet loading in that case. Is that always included?

Answer: In all the reconstructions (PANALESIS, PaleoMap) the ice sheet loading is not included. There are indeed huge uncertainties in ice sheet extent and volume at deep time, this is why we need a global-scale ice sheet model able to provide a first-order estimate of the ice sheets.

l325 "pre-industrial conditions at 280 ppm"
I am confused about combining present-day ice sheet reconstructions with pre-industrial climate. Is that the idea here?

Answer: We compare the present-day ice sheet to the results of *run1* since there are no observational data available for the pre-industrial period. Note that for models without dynamical ice sheets, the present-day ice sheets are used in *piControl* runs (Eyring et al. 2016).

l331 "set to a= $1.2 \cdot 10^{-15}$ Pa^{-3} s^{-1}"
What was the tuning procedure/target to find this value?

Answer: The tuning procedure, which was detailed in section 3.2.4, is now moved to Section 3.1.1 (Validation procedure) in the revised manuscript. The target is to obtain an Antarctic ice sheet volume within 10% of the observed values, while maintaining a similar surface elevation profile. The surface mass balance produced for Antartica is also estimated to be of the same order of magnitude of that obtained in an ensemble of Regional Climate Models.

l333 Figure 2. I find this projection unusual and not very intuitive to interpret. Could you give additional figures with traditional lat-lon projection for global views and polar stereographic projections for the ice sheets?

Answer: We now show the topography in the lat-lon projection in Figure 2, and stereographic projections for the ice sheets in Figure 10 of the revised manuscript.

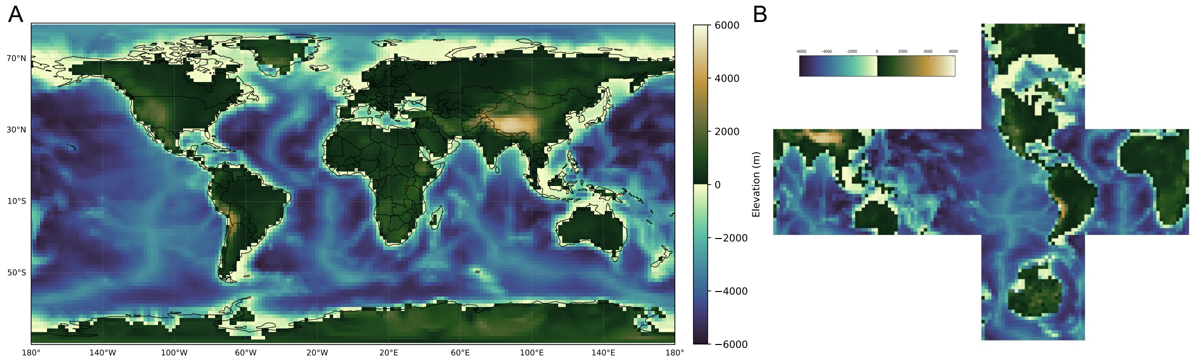

Figure 2. *Bedrock topography (with isostatic adjustment) and bathymetry used as initial boundary conditions in run1 on the latitude-longitude grid (A) and on the cubed-sphere grid (B).*

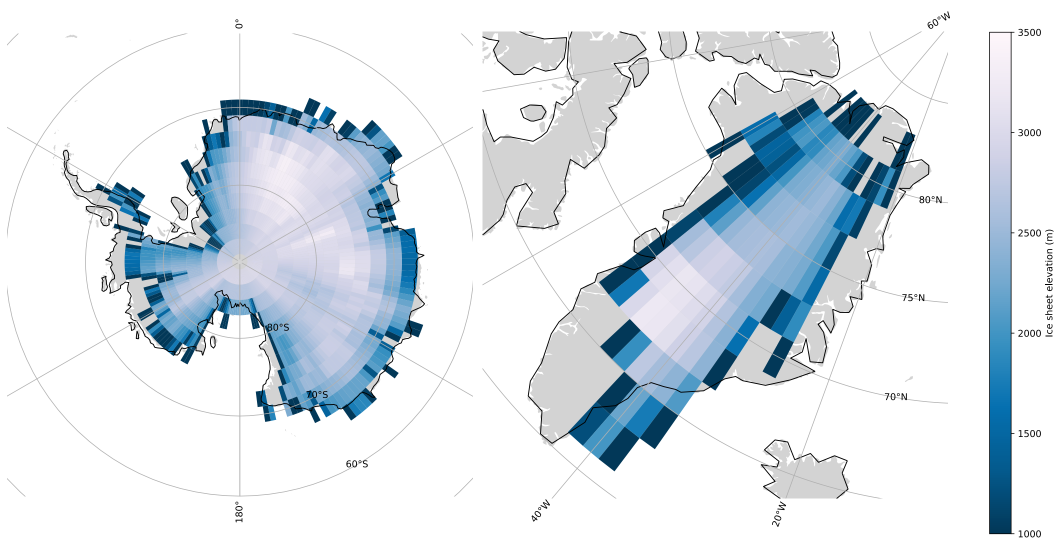

Figure 10. *Map of the region where ice sheets form with biogeodyn-MITgcmIS, showing the ESPG:3031 and ESPG:3413 projections for Antarctica and Greenland.*

l335 "The second simulation (run2)"
Can you give more details on how this run is set up? What is exactly the difference to run1 other than another CO2 concentration? Is this also a steady state run.

Answer: In the original version of the manuscript, *run2* was performed until the climate reaches a steady state at 360 ppm. As discussed earlier, these stationary conditions cannot be compared with the forced conditions of reanalyses. Therefore, following the reviewer's suggestion, we replaced this *run2* by a new run that is started from the 280ppm steady state (*run1*, pre-industrial simulation). Then, we applied a CO_2 increase until it reaches the 1979-2009 CO_2 average value (360 ppm). This description is now added in the revised manuscript.

l336 "This run will be evaluated against the reanalysis and observational data"
Are you assuming that the real climate is in equilibrium with a 360 ppm CO2 forcing over this period? Are the ice sheets? How is it conceptionally possible to compare the steady state model against transient observations?

Answer: We agree with the reviewer, and we have now replaced *run2* with a forced simulation, therefore this is not an issue anymore.

l352 "5 hours of CPU time due to the daily stepping of the ODEs"
That seems very expensive for such a low resolution ice sheet model. Consider optimizing the SMB calculations.

Answer: The ice-sheet model has been optimized, as explained earlier (see comment at l283).

l352 "for all land points"
Maybe you can exclude some points from the daily calculations when no ice is present?

Answer: It has been done in a similar way (see comment at l283).

l351 "one week to reach equilibrium"
Could you describe further how many years have been run for the different components?

Answer: This sentence refers to the coupled MITgcm, which is run for thousands of years. We have reformulated as follows: `The MITgcm took approximately 1500 simulated years per week to reach equilibrium using 25 processors for each iteration. BIOME4 and pysheds run in less than 5 minutes on a desktop computer. MITgcmIS needs around 1 hour of CPU time due to the daily stepping of eqs. (6)-(7) for all land points.'

l353 Table 2.
Specify meaning of variables in first column in the caption.

Answer: Table 2 was modified accordingly in the revised manuscript.

l356 Figure 4
Add latitude tick labels for NorESM2-LM

Answer: corrected in the revised manuscript.

l361 "Excluding the ice sheet formation"
It would be interesting to see an analysis how the inclusion of the new components (vegetation, ice sheets) changes the model behaviour compared to the reference MITgcm.

Answer: The coupled MITgcm setup (including ocean-atmosphere-land-sea ice) has been compared to observations in Brunetti & Vérard 2018 but starting from ETOPO2 topography (including present-day ice sheets). Here, we start from the bedrock topography and perform a detailed comparison with CMIP models and reanalyses, obtaining ice sheets and vegetation cover in good agreement with these products. Thus, the asynchronous coupling of the ice sheet and vegetation models allows us to obtain a climate in good agreement with observations. We added comparisons with a simulation which starts from ETOPO2 (like in Brunetti & Vérard 2018 but with all parameters as in *run1*) in the revised manuscript.

l369 "which underestimates the elevation of Antarctica and Greenland"
Maybe this should be verified with a biogeodyn-MITgcmIS run where the ice sheet elevations are prescribed. Having that option in the model would anyhow be a good development.

Answer: This option is already available. As the topography is prescribed in the coupled MITgcm, one can always start from a topography that includes ice sheets, as for example ETOPO2. This corresponds to the simulation performed in Brunetti & Vérard 2018 and the new *run2*.

l371 "the model capability to correctly reproduce the Hadley cells"
In line with comment l361, I seem to understand that MITgcm has been validated extensively. If true, this suggests strongly to focus the present analysis on how including the new components changes the model behaviour compared to the uncoupled reference in addition to comparison with other models.

Answer: MITgcm in its ocean-only version and the SPEEDY atmosphere have been distinctly validated. As said in the comment at l361, Brunetti & Vérard 2018 already described a coupled MITgcm simulation starting from ETOPO2 (including present-day ice sheets).

l398 "the energy used for ice sheet growth is not included in this diagnostics"
Could it be? Shouldn't it be? Does that mean energy is not conserved? What is the result when you include it?

Answer: The TOA budget problem was linked to a parameter that was inactive in previous versions of MITgcm and became active in the c68s version used here. We realised this issue with the help of Dr Jean-Michel Campin at MIT (his help is acknowledged in the revised version). Therefore, all simulations were rerun, and the problem is now solved. The results of the new simulations (with new tuning parameters) are very similar to the previous ones, except for the TOA budget that is now in the range of CMIP models.

l412 "due to higher surface temperatures"
Could this be related to equilibrium climate vs transient climate. That's an important shortcoming of this comparison that should be discussed.

Answer: This was indeed a problem of the previous version. Now that we compare reanalyses with a forced simulation (new run2), this issue is no more to be discussed.

l438 "3.2.3 Vegetation"
I seem to understand that the offline vegetation model BIOME4 has been extensively validated. In addition you state "Offline coupling between the coupled MITgcm atmosphere-ocean-sea ice-land setup and BIOME4 has been already successfully applied in Ragon et al. (2024, 2025)". I am wondering about the added value of section 3.2.3 in that context. Instead, it would be interesting to learn if including the ice sheet and runoff components changes the model behaviour. Is there an interaction between ice sheet change and vegetation? What vegetation grows in a much warmer climates where the ice sheets retreat?

Answer: The study in Ragon et al. (2024, 2025) is on the Permian-Triassic climates (without ice sheets), hence the validation of the coupled setup is needed for the present-day geography.

l465 Why is there no evaluation of the runoff component here? That should be added.

Answer: We added two figures showing the runoff routing maps obtained for the PI and Permian-Triassic simulations.

l466 "3.2.4 Ice sheet"
I find it very difficult to justify the presented detailed comparison with observations of ice sheet models at such low resolution. This is particularly true for Greenland, which consist of only 25 grid points in the PI simulation.

Answer: To assess our model, we compared the volume and the elevation distribution with the observations. This allows us to evaluate the global scale features of the ice sheet. As the ice sheet mainly interacts with the coupled MITgcm model through the surface albedo and the topography, it is important to evaluate these two quantities with respect to observations.

l469 "evaluating the SMB produced for Antarctica is a prerequisite to calibrate the Glen's law parameter"
The text until line 484 is not well placed in the Results section. I think this should be described earlier in the manuscript.

Answer: This part is moved to sec. 3.1.1 in the revised manuscript.

l471 Figure 10
Very unusual projection to display ice sheet results. Please consult some recent publications for inspiration. The standard are polar stereographic projections EPSG:3413 for Greenland and EPSG:3031 for Antarctica.

Answer: We now use these projections in the new Figure 10.

l486 "the Greenland ice sheet decreases strongly in height and extent"
This may be explained by the ice sheet being relaxed to a steady state in your model, while it is in a transient state in the real world. This just shows once more that the model is not meant to simulate transients with time scales of interest lower than ~50 kyr. In addition, I am concerned if the remaining 18 grid cells do a great job in simulating the dynamics of the ice sheet correctly.

Answer: *run2* ice sheet was removed, as run2 is now a forced simulation with a fixed ice sheet (obtained in run1 at 280 ppm). Thus, this comment does not apply anymore.

l489 "after smoothing to the same spatial resolution"
How does smoothing change the total volume?

Answer: Interpolation to a different, coarser grid has an impact on the ice sheet extent and volume.

l514 "further improvements are possible"
There are quite a few that should be discussed here.

Answer: Further developments are discussed in Section 3.3. We have reformulated this sentence.

l527 "In future iterations of MITgcmIS, sliding and basal heat balance could easily be implemented"
In my view, the most important improvements are to increase the spatial resolution of the ice sheet models and make the SMB model more efficient.

l528 "this would allow study of nonlinear processes"
Before any of that, your model setup has to be modified to be able to run transient ice sheet simulations and not only steady states.

Answer: As said above (see comment at l283), the SMB computation has been improved and is now more efficient. Concerning increasing spatial resolution of the ice-sheet model,the reviewer can refer to our response at l130.

References:

Brunetti M. and Ragon, C.: Attractors and bifurcation diagrams in complex climate models, Phys. Rev. E, 107, 054 214, https://doi.org/10.1103/PhysRevE.107.054214, 2023.

Brunetti M., Kasparian, J., and Vérard, C.: Co-existing climate attractors in a coupled aquaplanet, Climate Dynamics, 53, 6293–6308, https://doi.org/10.1007/s00382-019-04926-7, 2019.

Drótos, G., Bódai, T., and Tél, T.: On the importance of the convergence to climate attractors, The European Physical Journal Special Topics, 226, 2031–2038, https://doi.org/10.1140/epjst/e2017-70045-7, 2017.

Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, Geosci. Model Dev., 9, 1937–1958, https://doi.org/10.5194/gmd-9-1937-2016, 2016.

Hawkins, E., Smith, R. S., Allison, L. C., Gregory, J. M., Woollings, T. J., Pohlmann, H., and de Cuevas, B.: Bistability of the Atlantic overturning circulation in a global climate model and links to ocean freshwater transport, Geophysical Research Letters, 38, https://doi.org/https://doi.org/10.1029/2011GL047208, 2011.

Holden, P. B., Edwards, N. R., Fraedrich, K., Kirk, E., Lunkeit, F., and Zhu, X.: PLASIM–GENIE v1.0: a new intermediate complexity AOGCM, Geosci. Model Dev., 9, 3347–3361, https://doi.org/10.5194/gmd-9-3347-2016, 2016.

Lucarini, V. and Bódai, T.: Edge states in the climate system: exploring global instabilities and critical transitions, Nonlinearity, 30, R32–R66, https://doi.org/10.1088/1361-6544/aa6b11, 2017

Moinat L., Kasparian, J., and Brunetti, M.: Tipping detection using climate networks, Chaos: An Interdisciplinary Journal of Nonlinear Science, 34, 123 161, https://doi.org/10.1063/5.0230848, 2024.

Simmons, M. D., Miller, K. G., Ray, D. C., Davies, A., van Buchem, F. S. P., & Gréselle, B. (2020). *Phanerozoic eustasy*. In F. M. Gradstein, J. G. Ogg, M. D. Schmitz, & G. M. Ogg (Eds.), *Geologic time scale 2020* (Vol. 1, pp. 357–400). Elsevier. https://doi.org/10.1016/B978-0-12-824360-2.00013-9

Smith, R. S., Mathiot, P., Siahaan, A., Lee, V., Cornford, S. L., Gregory, J. M., Payne, A. J., Jenkins, A., Holland, P. R., Ridley, J. K., and Jones, C. G.: Coupling the U.K. Earth System Model to Dynamic Models of the Greenland and Antarctic Ice Sheets, Journal of Advances in Modeling Earth Systems, 13, e2021MS002520, https://doi.org/10.1029/2021MS002520, 2021.

Willeit, M., Ganopolski, A., Robinson, A., and Edwards, N. R.: The Earth system model CLIMBER-X v1.0 – Part 1: Climate model description and validation, Geosci. Model Dev., 15, 5905–5948, https://doi.org/10.5194/gmd-15-5905-2022, 2022.